Vol. III

TRANSCRIPT OF RECORD

Supreme Court of the United States

OCTOBER TERM, 1942

No. 369

MARCONI WIRELESS TELEGRAPH COMPANY OF AMERICA, PETITIONER,

vs.

THE UNITED STATES

No. 370

THE UNITED STATES, PETITIONER

vs.

MARCONI WIRELESS TELEGRAPH COMPANY OF AMERICA

ON WRITS OF CERTIORARI TO THE COURT OF CLAIMS

PETITIONS FOR CERTIORARI FILED { SEPTEMBER 2, 1942. SEPTEMBER 3, 1942.

CERTIORARI GRANTED DECEMBER 14, 1942.

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JUDD & DETWEILER (INC.), PRINTERS, WASHINGTON, D. C., SEPT. 18, 1942.

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It thus comes about, as in the present case, that certain statements covering a particular set of facts, are repeated from one author to another, and they therefore are not to be considered in the light of information carrying the authority of the various writers, but merely in the nature of a reprint of the original statements.

I do not find anything in the quotations from Eccles which have not already been answered, in various ways, with the exception of the statement at the bottom of [fol. 1319] Printed Record, page 1131 and top of page 1132, relative to a test showing a detuning of approximately ten per cent.

I have no hesitation in stating that this particular test is not illustrative of the fact that quenched spark-gap circuits can be detuned ten per cent. and operated efficiently, but simply is a record of the fact that Eccles made a test in which the circuits were detuned ten per cent.

There is no statement in the article that this represented the best attainable adjustment, and if there were such a statement, I should not hesitate to say that the apparatus had not been correctly handled.

Almost any result in the matter of detuning is possible in a laboratory experiment, but in actual practice only the best result is tolerated, and follows only when the circuits are tuned as accurately as possible.

Mr. Loftin's next reference is to a book entitled "Principles Underlying Radio Communication", 2nd Edition, Signal Corps United States Army, published at Washington by the Government Printing Office ("Defendant's Exhibit F-5").

He quotes from this book as follows (P. R., p. 1132):

"It is well to note that the principles of operation of the quenched gap and plain gap are exactly opposite. The former aims to stop the primary oscillations quickly, after the secondary has been brought to full activity. The latter aims to keep the primary oscillations going as long as possible, all the time giving energy to the secondary as it is radiated away; the coupling is loose and the primary decrement is kept low. The rapid decrease of the oscillations in a quenched-gap circuit is assisted by having a large ratio of capacity to inductance. This has the incidental advantage that the voltages across the condenser and coil are thus kept low."

It will be noted that this quotation does not make any distinction between the open gap and the quenched-gap in the matter of tuning, while various other Governmental publications to which I have referred in my answer to Q. 201 of my deposition, specifically direct the tuning of quenched spark-gap circuits.

It is also incorrect in point of fact in stating that the open gap arrangement aims to keep the primary oscillations going as long as possible; whereas, as a matter of fact, the actual adjustment of a plain gap set is always in such a way as to have the primary oscillations last as short a time as possible, without spoiling the wave form.

The difference in the quenched-spark arrangement and the open-spark arrangement is simply that it is possible to transfer the energy more quickly with the quenched spark-gap than with the open spark-gap, without spoiling the wave form.

I might also add that it is quite possible to use a coupling which is too close for the quenched-gap, and which will result in spoiling the wave form, just as in the case of the open spark-gap.

Both kinds of gaps are alike, and not opposite in their [fol. 1320] action, viz., that of recovering their initial high resistance and requiring some particular voltage to break it down again.

The quenched spark-gap is the result of some years of work in endeavoring to improve this characteristic of the open spark-gap, and does represent a substantial improvement.

Mr. Loftin next refers to a book entitled "Wireless Telegraphy," by Leggett, 1921 ("Defendant's Exhibit G-5"), and emphasizes considerably the fact that the author is an Englishman, also bringing out some statements in the book relative to the superior results obtained by a German Warship using the Telefunken apparatus, as compared to the results obtained by an English steamer under the same conditions.

He neglects, however, to point out that Mr. Leggett was formerly with the Technical Staff, Messrs. Siemens Bros. & Company, Ltd., which company is the agent for the German Telefunken Company.

While Mr. Loftin has included Mr. Leggett's name amongst his list of authorities, I may say that prior to taking up the work of this case, I had never heard of him

before, although I am familiar with most of the literature of this art and personally acquainted with the majority of the writers and workers of note in this art.

The statements referred to in the Leggett book do not appear to contain anything new, and which has not already been answered, but he does give one quotation which brings out a fact apparently overlooked by Mr. Loftin. This quotation is as follows (P. R., p. 1134):

"In the case of the open-gap curves when quenching does not occur, the curve will be found to have two distinct peaks of very different wave length and low current value, which is proportional to the energy radiated."

It will be noted in the above quotation that there is a qualifying phrase as follows:

"when quenching does not occur"

and the quotation expresses the fact that when an open gap is used and it does not quench, two wave lengths will be radiated. The qualifying phrase seems to indicate a recognition on the part of the writer that quenching may occur in an open gap. This is in accordance with the point which I have endeavored to make clear, viz., that quenching does occur in the open gap, but less perfectly and effectively than with the so-called quenched-gap.

Mr. Loftin then proceeds to summarize his remarks to the effect that the literature of the art supports his views. My own conclusions, for reasons already given in this answer is that even the literature which he cites contains no fundamental matters in support of his views, is largely a repetition of the same statements by these various writers which are founded not on extensive actual experience but, at most, a few laboratory experiments.

Furthermore, the major portion of the references cited derive their information from the works of Zenneck, Wien [fol. 1321] and Arco, all of whom, in one way or another, were used by the German Telefunken Company in the commercial exploitation of their apparatus.

It is further my opinion that the work of the gentlemen referred to, all of whom I recognize as men of the highest standing in this art, has been correlated and presented for commercial purposes in the article from the London "Electrician" of November 10, 1911 ("Defendant's Exhibit

C-5"), in a manner suitable to that purpose, and carrying impressions, particularly that conveyed by Fig. 3, quite different from the true significance of the work on which it was based.

It is also to be noted in connection with the references cited by Mr. Loftin, that they are all based on work done when the quenched spark-gap was new.

In contrast to these references are the Navy Manuals and Signal Corps publications which are based on years of actual experience in the use of the quenched spark-gap system, and which, as pointed out in my previous answers, state definitely and specifically that the circuits of a quenched spark-gap transmitter must be tuned.

Technical men may differ as to theories of operation, but the facts of long practical experience cannot be denied, and the published articles which have this background and source of information, and which are not written for exploitation purposes, are in agreement with each other and with my testimony, to the effect that the circuits of the quenched spark-gap transmitter are tuned.

* * * * * * *

Mr. Vaill: Counsel for claimant offers in evidence photostat copies of pages 370 to 374, inclusive, of the "electrician" (London), for June 18, 1909, containing the quotation in the last answer from page 371 and requests that the same be marked "Claimant's Exhibit No. 252."

(Said exhibit marked as requested.)

Q. 225. Will you please consider Mr. Loftin's answer to Q. 36, beginning on page 1137 of the Printed Record, and state, according to your knowledge and experience, to what extent the history of the open-gap type and quenched-gap type of transmitters correspond with Mr. Loftin's statements? Please include in your answer the significant facts concerning the Government use of such gaps, as well as those used for commercial purposes.

A. I note that Q. 36 refers to the commercial history of the *persistent* open-gap type and of the quenched-gap, re-
[fol. 1322] spectively, and the word "persistent" as a characterization of the open-gap type I am unable to accept. My reply will be based upon the use of what I understand to be the open gap transmitter.

Prior to the advent of the quenched gap, the open-gap type of transmitter was in very extensive use commercially, and this use continued long after the development of the quenched gap.

Practically the entire British Merchant Marine, the largest in the world, was equipped with the open gap transmitter, and it was also employed extensively on most of the Naval ships of the world, with the exception of the German Navy.

It also found employment in the United States Naval Service.

In addition to being used extensively for Marine work, the open gap type of transmitter was the pioneer in long-distance Trans-Oceanic communication and must be credited with some notable performances in this field. Messages up to some sixty-six hundred miles were transmitted by this system, and a commericial service using it, existed between England and Canada, the United States and the Island of Hawaii and also between Hawaii and Japan.

During the war, communicatioon was maintained between England and Russia by means of this system, as well as ships at sea great distances away from England.

In the United States the United Fruit Company operated an open-spark system at New Orleans, which communicated with their ships at sea, and by means of a relay station at Swan Island, also employing an open-spark system, with Santa Marta, South America.

The Marconi Wireless Telegraph Company of America, prior to the war, had a large number of coast stations along the Atlantic and Pacific Coasts, as well as the Caribbean Sea, which stations were taken over by the Government during the war. The Marconi Company also operated a radio station at Astoria in Oregon, and Ketchikan in Alaska.

In long-distance communication the quenched spark-gap was employed at only one station in the United States, viz., Sayville, Long Island, which was constructed by the German Telefunken Company.

While the quenched spark-gap was employed by the United States Navy, they also made considerable use of the open spark-gap and the very well-known station at Arlington, Va., employed an open spark-gap of the rotary type of very large power, viz., 100 kilo-watts

This installation is the subject of a paper by Capt. (now Admiral) William H. G. Bullard, United States Navy in the Proceedings of the Radio Engineers, October, 1916. I happen to be personally familiar with this installation, since the apparatus was built by the National Electric Signaling Company, for whom I was working at the time of its construction and test.

This particular transmitter was operated for many years to communicate with Naval ships at sea, and to send out Time signals.

As has already been brought out by the witness George H. Clark, an extensive use of the open gap transmitter by the Navy continued, even when the quenched spark-gap was [fol. 1323] employed, since most of the quenched spark-gap equipments employed an open rotary gap of the non-synchronous types. Mr. Clark's testimony is found on page 231 of the Printed Record, with an illustrative diagram on page 249, and his statement relative to their use by the Navy is one which I can confirm from my own experience, as the result of building a very large number of these while Chief Engineer of the Marconi Company, and of having seen many of them built by other companies in the Navy yards.

Mr. Clark's statement on page 231 relative to the use of the rotary gap entirely during the war, is rather interesting. It happens that I was familiar with this fact during the war and the reason therefor, viz., that the interference created by the quenched spark-gap was so great as to prevent other ships communicating when it was used.

Mr. Loftin, at the bottom of Printed Record, page 1137, refers to this use in the following statement:

"* * * This of course, was a use in which dependence was being placed upon inefficiency in the matter of communication and the open-gap type was particularly suitable."

It would seem that his criterion of efficiency was rather a peculiar one and that he considers a system which could be used to maintain communication was inefficient, while one which could not be used was efficient. The actual facts of this situation are perhaps best brought out by a reference to the diagram illustrative of the quenched-spark action, such as Fig. 131. It will be noted in this diagram that the oscillations in the secondary circuit increase very rapidly

from zero to a maximum, during which time the primary decreases very rapidly.

It has already been explained that a rapid decrease in the amplitude of oscillations causes a broad tuning, but it is also equally true that a rapid increase causes the same broad tuning.

It results from this fact that a properly operated open spark-gap transmitter in which the oscillations in the primary die down more slowly, while the oscillations in the secondary increase more slowly, greater selectivity is obtained. This is a theoretical point which had been confirmed practically by the radiomen of the British Navy, and I recollect discussing the subject with a British Naval officer during the war.

It follows from this that while the quenched spark-gap transmitter is superior to the open spark-gap transmitter in so far as obtaining maximum power output with a given power input, it is inferior to the open spark-gap in so far as selectivity is concerned.

In connection with Mr. Loftin's contentoin relative to the inefficiency of the open spark-gap, referred to above, and frequently in connection with his discussion of the open spark-gap, it is of interest to note the performance of the Arlington transmitter. In a paper which appears in the "Electrical World", Vol. 61, of 1913, page 722, some technical data is given relative to this installation and the following quotation:

"The oscillations are particularly persistent, the logarithmic decrement per complete period being only .031." [fol. 1324] This represents some 150 oscillations in the antenna circuit and means extremely good selectivity, while at the same time a very large power was radiated.

While the paper referred to does not give the antenna current, I have personally seen a radiation in excess of 75 amperes, which is a radiation of the same order of magnitude as that of the Sayville transmitter employing a quenched gap.

It is thus seen that the open gap type of transmitter, when correctly coupled to give a pure wave, is still capable of a high order of efficiency in the matter of the transfer of energy from the closed circuit to the open circuit. There also appears in the paper above referred to the following statement:

"The character of radiation sent out by this equipment is very pure in its wave form, being practically seven times as good as that fixed as the legal limit by legislation which became effective last summer."

The "legal limit" referred to in this quotation is that set by the radio law, to which Mr. Loftin has several times referred.

* * * * * * *

Q. 226. To what extent are you personally familiar with the evidence taken on behalf of the parties in the case of Marconi Wireless Telegraph Company of America *vs.* Kilbourne & Clark Manufacturing Company, in the United States District Court for the Western District of Washington, Northern Division, which involved the Marconi patent No. 763,772 and Lodge patent No. 609,154, here in suit?

A. I was a witness in that case and conducted and took part in numerous tests for the plaintiff. I also witnessed various tests conducted by the defendant, including demonstrations at the University of Washington. I also witnessed tests at Harvard University, conducted on behalf of the plaintiff.

I was present practically every day in Court during the taking of the testimony at the trial.

Q. 227. In answer to Q. 37 (Page 1139 of the Printed Record), Mr. Loftin has referred to the transmitters and receivers charged to be infringements in the case of Marconi Wireless Telegraph Company of America *vs.* The Kilbourne & Clark Manufacturing Company, referred to in my previous question. How were the various transmitters designated in that suit?

A. There were three types of transmitters involved in the Kilbourne & Clark case,—one known as the "Thompson Transmitter", the second, known as the "Simpson Mercury Valve Transmitter", and a third type, installed on board [fol. 1325] the steamship Achilles, and referred to by that name.

Q. 228. I will ask you to refer to "Plaintiff's Exhibit No. 9" in the Kilbourne & Clark case and will ask you to state what connection you had with the production of that diagram.

A. The diagram referred to was made under my direction and represents the apparatus which I saw on the steamship Achilles.

Q. 229. Please now refer to "Plaintiff's Exhibit No. 95" in the present case, and I will ask you to state what connection you had with that diagram.

A. This diagram was made under my direction and is a copy of the diagram of "Plaintiff's Exhibit No. 9" in the Kilbourne & Clark case.

Q. 230. I hand you two other sheets of diagrams and ask you what these diagrams represent as concerns the Kilbourne & Clark case above referred to.

A. "Plaintiff's Exhibit No. 2" in the Kilbourne & Clark case (Claimant's Ex. #253) represents the transmitter and receiver of the apparatus installed on board the steamship Alameda. The transmitter is of the type known as the "Thompson" transmitter, and the receiver of the type known as the Kilbourne & Clark standard receiver.

"Plaintiff's Exhibit No. 68" in the Kilbourne & Clark case (Claimant's Ex. #254) represents the Kilbourne & Clark transmitting and receiving arrangement. The transmitter is known as the Simpson mercury valve transmitter, and the receiver is the standard Kilbourne & Clark receiver.

Mr. Vaill: Counsel for claimant notes that a copy of "Plaintiff's Exhibit No. 95" in the present case is found opposite page 183 of the Printed Record, which the witness has identified as the same as "Plaintiff's Exhibit No. 9 (opposite page 225 of the Printed Record, in the Kilbourne & Clark case.

Mr. Vaill: Counsel for claimant offers in evidence a printed copy of "Plaintiff's Exhibit No. 2" in the Kilbourne & Clark case and requests that the same be marked "Claimant's Exhibit No. 253."

Also a copy of "Plaintiff's Exhibit No. 68" in the Kilbourne & Clark case and requests that the same be marked "Claimant's Exhibit No. 254."

(Said exhibits marked as requested.)

Q. 231. Are you familiar with the construction and mode of operation of the transmitters and receivers referred to in your last answer?

A. I am.

Q. 232. What disposition was made by the District Court in the Kilbourne & Clark case of the question of infringement by the transmitter of the S. S. Achilles and the receiver used therewith?

A. This apparatus was installed on the steamship Achilles of the Panama Steamship Company Line, which was owned by the Government.

Because of the fact that the apparatus was therefore Government apparatus, the Court held that in the light of the Act of 1910, the Court had no jurisdiction, its ruling being as follows:

[fol. 1326] "As to the apparatus furnished to the United States, this court, in view of the act of 1910 (Act June 25, 1910, c. 423, 36 Stat. 851 [Comp. Stat. 1913, § 9465]), has not jurisdiction, either over a suit for an injunction, or for an accounting. Foster Hose Supporter Co. *vs.* Taylor, 191 Fed. 1003, 111 C. C. A. 667." (239 Fed. Rep., 355.)

Q. 233. Will you please compare the transmitter and receiver of the Achilles apparatus of "Plaintiff's Exhibit No. 95" in this case with the Wireless Specialty Apparatus Company transmitter and receiver of "Plaintiff's Exhibit No. 87" in this case, and state the similarities or differences in circuit arrangements and mode of operation?

A. Both the transmitting station and the receiving station of "Plaintiff's Exhibit No. 87", the Wireless Specialty Apparatus Company type, and "Plaintiff's Exhibit No. 95", the Kilbourne & Clark type, are substantially the same: they differ in detail in the transmitter merely by the inclusion in "Plaintiff's Exhibit No. 95", Kilbourne & Clark type, of the variable inductance *d''*; the total inductance in the closed oscillating circuit in most quenched spark-gap transmitters is built in a single coil, as *d* in "Plaintiff's Exhibit No. 87", but occasionally it is split into two parts, as *d*, *d''* in the Kilbourne & Clark arrangement on the steamship Achilles, "Plaintiff's Exhibit No. 95". The receivers differ in detail by the inclusion in "Plaintiff's Exhibit No. 95" of the condenser *h*. This provides an auxiliary means of tuning the antenna circuit, but the means provided in the Wireless Specialty Apparatus type, viz., the variable inductance *g* and *i'* is, in general, sufficient.

In both exhibits the secondary of the receiving system is tuned and the transmitting and reiving stations together in

both exhibits, employ four circuits, all in tune with each other.

Q. 234. Will you please make a similar comparison between the Achilles apparatus of "Plaintiff's Exhibit No. 95" and the apparatus of the type manufactured by the Navy of "Plaintiff's Exhibit No. 93"?

A. The apparatus of "Plaintiff's Exhibit No. 93" is substantially the same as "Plaintiff's Exhibit No. 95", Kilbourne & Clark type apparatus of the steamship Achilles. It differs in detail in the transmitter by having all of the inductance in the closed circuit in one coil, viz., *d*, instead of in two coils, as in "Plaintiff's Exhibit No. 95".

The receiver differs in detail by having all of the antenna inductance in one coil *f'*, instead of in two coils, in "Plaintiff's Exhibit No. 95", *f'* and *g'*, and also by having the condenser *h* connected only in series in the antenna, instead of alternatively in series or in parallel in "Plaintiff's Exhibit No. 95".

The telephone circuit shown in green in "Plaintiff's Exhibit No. 93" differs from the same circuit in "Plaintiff's Exhibit No. 95" by the omission of the battery B and potentiometer *y* of "Plaintiff's Exhibit No. 95". These differences represent merely differences in details of construction, but do not affect the mode of operation, and in both exhibits there is shown a complete transmission and reception system, embodying four tuned circuits, all four circuits being tuned together.

[fol. 1327] Q. 235. Will you please make a similar comparison between the Achilles apparatus of "Plaintiff's Exhibit No. 95" and the apparatus of the National Electric Signaling System as illustrated in "Plaintiff's Exhibit No. 105" in this case, which has been referred to By Mr. Loftin in answer to Q. 37 of his deposition, beginning on Printed Record, page 1139?

A. The apparatus of "Plaintiff's Exhibit No. 105" is substantially the same as the apparatus of the Kilbourne & Clark type of the Achilles installation, "Plaintiff's Exhibit No. 95". The transmitting diagram of "Plaintiff's Exhibit No. 105" shows three types of spark-gaps, viz., the plain spark-gap, the rotary spark-gap and the quenched spark-gap.

The circuit containing the quenched spark-gap differs from the Achilles, Kilbourne & Clark type only in the fact that all of the inductance in the closed circuit is contained

in one coil *d*, instead of two separate coils, *d*, *d''* of the Achilles set, which difference has no bearing on the mode of operation.

The receiving circuits of the two systems are identical, except that the condenser *h* of "Plaintiff's Exhibit No. 95" is in series with the antenna in "Plaintiff's Exhibit No. 105", instead of alternatively in series or in parallel, as in the former exhibit.

Both arrangements show a complete transmitting and receiving system with four circuits, all in tune.

I am personally familiar with the apparatus which is illustrated in "Plaintiff's Exhibit No. 105", as I testified relative to it in the National Electric Signaling Company case, and was formerly an Engineer for the National Electric Signaling Company, and took part in the design and manufacture of this apparatus.

The three different types of gap, shown in the transmitting station diagram of "Plaintiff's Exhibit No. 105", all function in the same manner; that is to say, the spark-gap has cooling and quenching capabilities, the plain spark-gap having the least, the rotary spark-gap being next in order, and the quenched spark-gap having the most.

Q. 236. Will you please consider the paragraph at the middle of page 1139 of the Printed Record as to Mr. Loftin's conclusions concerning what types of spark-gaps were included in Judge Veeder's decision in the National Electric Signaling Company case and state, according to your knowledge, what types of spark-gaps were included in "Plaintiff's Proofs as involved in Defendant's apparatus, and also please refer to your testimony in the National Electric Signaling Company case, which substantiates your personal knowledge of the matter?

A. Three types of spark-gaps were in the National Electric Signaling Company case, viz., the plain spark-gap, the rotary spark-gap and the quenched spark-gap, and I recollect that I personally testified relative to them.

Fig. 1 of "Complainant's Exhibit No. 11" in the National Electric Signaling Company case (Claimant's Ex. #255) shows a circuit diagram of the transmitter and on page 14 of the plaintiff's record in that case (Page 46 of the Transcript in the Court of Appeals, Second Circuit), I testified as follows:

"The spark-gap C was ordinarily one of three types; first, [fol. 1328] two metal balls or rods arranged so that the distance between them could be varied and each insulated from the other; second, a rotary spark-gap, consisting of a metal disk mounted on the shaft of generator G, having thereon radially projecting spokes and two electrodes insulated from each other and from the above mentioned spokes, so arranged that their distances from these spokes could be varied and so that their circumferential position could be adjusted; third, a quenched spark-gap, consisting of a series of metal plates insulated from each other and so arranged that the discharge through them was broken up into a number of small sparks, all in series with each other. These plates were so mounted and arranged that they could be cooled by means of a blower."

"Complainant's Exhibit No. 11" in the National Electric Signaling case was made and produced by me in connection with the description of the apparatus made by the National Electric Signaling Company during my employment with that Company, which terminated in March, 1912.

Q. 237. Please state whether or not the "Plaintiff's Exhibit No. 50" in the National Electric Signaling case and "Plaintiff's Exhibit No. 105" in the present case are correct diagrams representing the circuits of "Plaintiff's Exhibit No. 11" in the National Electric Signaling case, when taken in connection with your testimony in the later case, concerning the spark-gaps used with said circuits of the National Electric Signaling Company apparatus of "Plaintiff's Exhibit No. 11".

A. Yes, they are.

Q. 238. Referring to the three types of spark-gaps illustrated in "Plaintiff's Exhibit No. 105" in the present case, will you please state during what period these types of spark-gaps were made and sold, and to whom?

A. These spark-gaps were manufactured between 1908 and 1912 and sets involving the rotary spark-gap and the quenched spark-gap were sold to the United States Government. The period during which these latter sets were sold was, according to my recollection, between 1910 and 1912.

As an example of one of the rotary spark-gap transmitters sold to the Government, I will refer to the 100 k. w.

transmitter illustrated on pages 424 and 425 of the "Proceedings of the Institute of Radio Engineers", "Plaintiff's Exhibit No. 81".

* * * * * * *

Mr. Vaill: Counsel for claimant offers in evidence a photostat copy of "Complainant's Exhibit No. 11, Drawing of Defendant's Apparatus" produced by the witness in the [fol. 1329] case of Marconi Wireless Telegraph Company of America *vs.* National Electric Signaling Company in the United States District Court, Eastern District of New York, and requests that the same be marked "Claimant's Exhibit No. 255".

(Said exhibit marked as requested.)

Q. 239. Referring to Mr. Loftin's testimony beginning at the bottom of page 1139 of the Printed Record, concerning the two transmitters referred to in the Court's opinion (239 F. R., 328), one known as the "Simpson mercury valve" transmitter, and the other referred to as the "impulse" transmitter, will you please refer to "Claimant's Exhibit No. 253" and "Claimant's Exhibit No. 254" in the present case (respectively "Plaintiff's Exhibits 2 and 68" in the Kilbourne & Clark case), and compare the same with respect to construction and operation with transmitters relied upon as infringements in this case; particularly the Wireless Specialty Apparatus type of "Plaintiff's Exhibit No. 87", the type manufactured by the Navy of "Plaintiff's Exhibit No. 93" and Wireless Improvement Company type of "Plaintiff's Exhibit No. 88"?

A. There are a number of important differences between the apparatus of the Kilbourne and Clark case "Claimant's Exhibits 253 and 254" and the "Plaintiff's Exhibit 87, 88 and 93", which are the infringing apparatus in the present case.

Referring now to "Claimant's Exhibit No. 253", the transmitter shows a single turn of inductance without any means of adjustment in the closed circuit. This is indicated by the red line circuit G, *c*, *d*. The receiver drawing similarly shows a secondary coil j^2 having a fixed number of turns. Both of these arrangements constituted exceptions to the universal radio practice, and there is no other commercial apparatus that I am aware of which was without variation or rather, variability, in these two circuits.

I might also add that this use by the Kilbourne & Clark Company of this arrangement constituted a negligible percentage of the radio installations in the United States.

The fact which made possible at all the employment of this arrangement was the fact that they were used principally for one wave length only, viz., 600 meters. As a result of this the secondary coil *j'* was broadly tuned in this particular region. The primary circuit of the transmitter was deliberately operated out-of-tune by a sufficient amount to form a basis for the contention of the defendant in this case, to the effect that the circuit was not in tune.

As a matter of fact, however, it was sufficiently near to being in tune, so that a considerable benefit was derived from this particular adjustment and had the set really been capable of a so-called impulse excitation, it could have been operated very widely out of tune, which was not the case.

Judge Netterer, in his decision (pages 348 and 349 of F. R. 239) states that the Thompson transmitter shown in "Claimant's Exhibit No. 253" carries out the Lodge idea. With this decision I am obliged to disagree from my knowledge of the operation of the Thompson set and the Lodge arrangement.

[fol. 1330] As I have frequently pointed out in the course of my deposition, Lodge's idea was not realized in any way whatsoever by his arrangement of Fig. 4 nor, so far as I am aware, has any apparatus ever been made commercially which has succeeded in carrying out this idea.

The Thompson transmitter was very far from realizing this action. It was shown by tests which I conducted first, that the efficiency of the transmitter was materially improved by bringing the circuits into exact practical tuning, and also that as operated, the circuits were not greatly different from the adjustment required for exact tuning.

I might also point out that these facts are substantiated by a chart drawn by Mr. Kolster, which is Defendant's Exhibit K-9", (Claimant's Ex. #256) and which gives resonance curves of the secondary circuit and the primary circuit: the former of which has its peak at 590 meters, while the secondary has its peak at 650 meters.

If this were a real "impulse" transmitter, it would not be necessary to have the period of the primary circuit so near that of the secondary circuit but, on the contrary, it might have been some 200 or 300 meters instead of 650 meters.

It will be noted that resonance curve No. 3 of the primary circuit is very broad, and extends from about 550 meters to something over 800 meters.

As i have previously explained, when the form of an electrical oscillation is not that of the sine wave, it may be analyzed into a number of such sine waves. In the present case the fact that the resonance curve extends over the wide range indicated, means that there are components of sine form of all the intermediate frequencies, and it will be noted that at 590 meters the height of the curve is about half the height of the peak. This means that there is a sine component of that wave length which is about half as strong as the 650-meter component, and when the secondary circuit is tuned to this, responds to this particular component.

Were the Thompson transmitter a true impulse transmitter, it would not be necessary to have any particular relation between the time constants of the open and closed circuits.

Lodge, of course, not only does not realize this "impulse" action, but fails to even perform in a way that is of any practical use.

Referring now to "Claimant's Exhibit No. 254", the defendant's Simpson mercury valve transmitting station, the diagram of the transmitter shows that this differs from the defendant's apparatus in the present case by the inclusion in the antenna circuit of the condenser *d d'*, and the mercury valve Z, in the power circuit.

The receiving circuit employs a secondary coil j^2, *having a fixed number of turns.*

Judge Netterer, in his decision relative to the Simpson mercury valve transmitting station 239 F. R., page 346, states as follows:

"This circuit has enormous decrement, and is non-persis- [fol. 1331] tent. It is not, as in the Marconi patent in suit, *primarily* a reservoir or persistent oscillator, and does not co-operate with the antenna on the principle of resonance."

It so happens that I personally conducted tests on this transmitter and I found that the closed circuit, shown in red in "Claimant's Exhibit No. 254", was in resonance with the antenna circuit shown in black, and I recollect also seeing a report by the Bureau of Standards on this transmitter, which showed that they had found the same things.

In view of these facts, I am obliged to disagree with Judge Netterer's decision, quoted above.

During the trial of the Kilbourne & Clark case, certain tests were conducted at the University of Washington, purporting to show what was taking place in the oscillating circuit of a Simpson mercury valve transmitter and certain other tests were made at Harvard University on behalf of the plaintiff.

Judge Netterer, in his decision, refers to these tests as follows (239 F. R., pp. 346 and 347):

"The contention of the plaintiff with relation to the Massachusetts test, in which it was shown that there were two and one-half oscillations in the antenna circuit, and that this must refute the contention of the defendants with relation to the Washington University photographic test, may be answered by the suggestion that the Washington University result was obtained, the photograph speaks for itself, and defendant's witnesses to that extent are corroborated."

I was personally present at the tests referred to and understood exactly what was being done in both cases, and the arrangement used at the University of Washington test was incapable of indicating what was taking place in the closed oscillating circuit of the Simpson mercury valve transmitter.

Unfortunately, this was a highly technical matter, which apparently was not understood by the Court and the statement by the Learned Judge to the effect that the photograph spoke for itself, is not in accordance with the technical facts involved.

Whether or not a photograph of this sort means anything, depends entirely upon the nature of the apparatus and its method of use, and in the tests at Washington University the requirements of this sort were not met by the apparatus used. On the other hand, the tests at Harvard University were technically correct, and showed that there were at least two and one-half oscillations in the primary circuit.

It happens that this matter of showing directly the number of oscillations in a high-frequency oscillating circuit is an extremely difficult one and seldom attempted, and while the tests at Harvard showed at least two and a half

oscillations, the way in which the circuits tuned indicated to me that there were considerably more than the two and a half.

The mercury valve Z used in this transmitter served no useful purpose whatever. During the course of the tests which I made on the Simpson transmitter I removed the [fol. 1332] mercury valve Z and operated the set without it, and found that the operation was then slightly better.

Referring once again to the Thompson transmitter, I neglected to point out that inductance *d* was made in the form of a loop of wire which was readily removable, and that with this particular construction it was an easy matter for the operator to remove the loop supplied with this set and substitute another, which would make the closed circuit more perfectly in tune.

On page 348, 239 F. R., there is shown a diagram of the Thompson transmitter, which differs in some non-essentials, from the diagram of "Claimant's Exhibit No. 253". The spark-gap is shown at h^7 and h^6, in two parts, which arrangement is not different electrically from that shown at G.

The condenser indicated by *j* is shown in two parts in series, and this electrically is equivalent to one condenser, as shown at *c* in "Claimant's Exhibit No. 253". Also the inductance in the antenna circuit is divided into two parts h^3 and h^4, instead of being all one coil *d'* in "Claimant's Exhibit No. 253". This difference has no electrical significance, but is merely a matter of convenience in mechanical construction.

Q. 240. Referring to your last answer, where you mention the photographs which were taken during the tests at the University of Washington, purporting to show what was taking place in the oscillating circuit of the Simpson transmitter will you please state what, according to your knowledge, these photographs did show as concerns the oscillating circuit of the Simpson transmitter?

A. This oscillogram merely showed the charging of the condenser *d, d'* by the rectifier Z, and it was incapable of showing what took place in the red circuit after the spark-gap G broke down, and therefore incapable of telling anything whatsoever relative to the oscillations in that circuit.

Mr. Vaill: Counsel for claimant offers in evidence a photostat copy of the Kolster chart referred to by the wit-

ness ("Defendant's Exhibit K-9" in the Kilbourne & Clark case, Printed Record, page 1266), and requests that the same be marked "Claimant's Exhibit No. 256."

(Said exhibit marked as requested.)

Q. 241. In answer to Q. 38 (Page 1147, P. R.), Mr. Loftin was asked to consider the Act of August 13, 1912, and to state what effect the Act had upon the system described in Marconi patent 763,772, and upon the quenched-gap system. Will you please state wherein you may agree or disagree with Mr. Loftin's answer to that question?

A. Mr. Loftin has attempted to differentiate between the quenched-gap system and the Marconi system of patent No. 763,772 by drawing distinctions between the operation of the quenched-gap transmitter and a transmitter having an open gap, these distinctions being largely based on his statements to the effect that the open gap type of transmitter, in order to be usable under the Act of August 13, 1912, must be employed with so loose a coupling that practically it will be very inefficient.

[fol. 1333] With these statements and conclusion I do not agree, for I have seen in operation numbers of open spark-gap transmitters operating within the requirements of the law; that is, the Act of August 13, 1912, and at the same time having a satisfactory over-all efficiency.

In so far as the tuning is concerned, some of these open spark-gap sets are superior to the quenched-spark set and as an illustration of the transfer in an efficient manner of a very large amount of power from the closed circuit to the open circuit, I will refer to the Arlington transmitter shown in Admiral Bullard's paper in the Proceedings of the Institute of Radio Engineers, "Plaintiff's Exhibit No. 81", beginning at page 421.

This transfer of power from the closed circuit to the open circuit takes place without a bad wave-form resulting in the antenna, but, on the contrary, an extremely good wave-form having, as pointed out in my answer to Q. 225, the decrement of which is only about one-seventh of that permitted by law, which means that the wave-form was seven times as good as is necessary to comply with the law.

In the article in the "Electrical World," Volume 61, of 1913, page 722, which, I believe, was written by Mr. S. M.

Kintner and others connected with the National Electric Signaling Company, there occurs the following statement:

"The character of radiation sent out by this equipment is very pure in its wave form, being practically seven times as good as that fixed as the legal limit by legislation which became effective last summer. This purity of radiation is due in a large measure to the low resistance of the antenna, but in order that best advantage may be taken of this condition very accurate tuning of the oscillation circuits in the sending apparatus is required."

I might add that at the time the radio law in 1912 came into effect there was a considerable number of open-gap transmitters in the stations of the Marconi Wireless Telegraph Company of America, both on shore and at sea, and no difficulty was perienced in so adjusting this apparatus that it complied with the law while, at the same time, rendering satisfactory communication service.

Mr. Vaill: Counsel for claimant offers in evidence a photostat copy of page 722 of the "Electrical World" of 1913, referred to by the witness in his last answer and in answer to Q. 225, and requests that the same be marked "Claimant's Exhibit No. 257."

(Said exhibit marked as requested.)

Q. 242. I invite your attention to Mr. Loftin's answer to X Q. 59 on page 1155 of the Printed Record, and will ask you to state according to your knowledge and recollection of the prior litigation and the patents involved in this suit, whether or not any new prior art was considered by Mr. Loftin that was not involved in the prior litigation.

A. Mr. Loftin has not cited any prior art which was not involved in previous cases.

* * * * * * *

[fol. 1334] Q. 243. In answer to Q. 4 of his deposition, Dr. John M. Miller referred to the demonstrations before Judge Mayer (Printed Record, page 758), in the case of Marconi Wireless Telegraph Company of America *vs.* DeForest Radio Telephone & Telegraph Co., on the Fleming patent 803,684 in the Southern District of New York, the supplemental record of which under the supplemental bill has been included in this case as "Plaintiff's Exhibit

No. 171." Are you the Roy A. Weagant whose testimony is printed in said exhibit on pages 37-41, inclusive?

A. I am.

Q. 244. Were you present at the demonstrations before Judge Mayer in the case just referred to, and is the testimony you gave therein correct as set forth in said "Plaintiff's Exhibit No. 171"?

A. I was, and it is.

Q. 245. Dr. Miller, in his answer to Q. 4 (P. R., p. 758), states that:

> "In the demonstrations before Judge Mayer advantage was taken of an obscure, freak, impractical phenomenon in order to demonstrate the alleged inherent capacity of the two-element device for generating radio waves. Even this freak operation of the two-element device is basically different from that of the three-element device. These facts were not presented in the record in the case referred to. On the other hand, testimony was given which was not in accordance with the facts."

Please state from your knowledge of the proceedings in the trial before Judge Mayer, which included the demonstrations referred to, whether or not deficient testimony was given, as stated by Mr. Miller, or whether the testimony was not in accordance with the facts.

A. I am unable to understand Dr. Miller's reference to testimony not in accordance with the facts, and I note particularly that he fails to point out in detail in what respect testimony given differed from his idea of the facts. I was present during the taking of all the testimony in the case before Judge Mayer and took part in all the tests and demonstrations.

From my recollection of the testimony, and from a recent reading of it, I am quite certain that no testimony was given which was contrary to the facts within the best knowledge and understanding of the witnesses for plaintiff.

From the stress which Dr. Miller has laid on the difficulty which he personally has had in making a two-element tube to oscillate, it occurs to me that perhaps it is his impression that the witnesses gave misleading testimony in this respect.

No statements, however, were made to the effect that the two-element oscillating valve was a proven commercial device nor, so far as I can recollect, any statement tending [fol. 1335] to indicate that it was particularly reliable; while, on the contrary, there was testimony which indicated that the results were secured with some difficulty.

I recollect distinctly, when I was asked to make the tests and demonstrations of the oscillating two-electrode Fleming valve, that I was informed that the essential thing was simply to demonstrate that it had the capability of generating high frequency oscillations.

It was, furthermore, my understanding that this was in accordance with the wishes of the Court and that the question of reliability, or otherwise, was not involved. I may also state from my recollection of the trial and my technical knowledge and experience that, in my opinion, no facts within the knowledge of the witnesses were withheld either on direct, or cross examination, and no demonstrations or statements made which would tend in any way to mislead the Court as to the facts presented.

It is, of course, to be understood by this reply, that the above remarks relative to the action of the tube apply to the tests before Judge Mayer and not to the recent demonstrations which I made with the high vacuum two-electrode tube.

Q. 246. Will you please refer to pages 87 and 88 of the Supplemental Proceedings in the case of Marconi Company vs. the DeForest Company above referred to, "Plaintiff's Exhibit No. 171" in the present case, and state whether the diagrams shown on said pages as "Plaintiff's Exhibit No. 2" and "Plaintiff's Exhibit No. 3" illustrate the connections of the circuits used in the demonstrations before Judge Mayer?

A. Yes, they do.

Q. 247. Will you please state whether or not you agree with Dr. Miller's statement on P. R., p. 758, that:

"The two-element valve does not possess inherently the same capacity for generating radio waves as is possessed by the three-element device: * * *."

In giving your answer will you please state your reasons and explain to what extent the two-electrode and three-electrode valves inherently possess, of and by themselves, the capacity of generating radio waves or oscillations?

A. Neither the three-element valve nor the two-element valve possesses, by itself, the capability of oscillation. On the contrary, both devices require connections and associations with additional devices, such as coils, condensers and batteries, which additional devices must be connected and associated in a particular manner, in order to accomplish the result.

With reference to Dr. Miller's statement, I will say that I am quite in disagreement, for the simple reason that, as shown by my recent tests, the two-electrode tube, when associated with the proper circuits, batteries, etc., has the same capability of oscillating as is possessed by the three-electrode tube.

Q. 248. Please state, according to your knowledge, and the results of the tests which were made before Judge Mayer, and the tests which you have referred to in the last answer, to what extent the two-electrode and the three-electrode valves, respectively, possess the capacity for generating radio waves or of oscillating when associated with the proper devices and circuits.

A. Both valves, when connected in suitable circuits and [fol. 1336] arranged in proper electrical association, have the same inherent capability for generating oscillations.

Both devices are capable of acting by what is termed "gas action," and both devices are capable of oscillating when vacuum is extremely high, that is, so high that "gas action" is no longer possible.

Under these latter conditions, both devices function as the result of the electrostatic control of the electron stream and a suitable means for transferring some of the energy in the output circuit back into the input circuit.

In the recent tests which I conducted and testified to in the first part of this deposition, the two-electrode vacuum tube, connected in what was substantially the circuit of Fleming patent No. 803,684, oscillated in a manner identical with that of the three-electrode high-vacuum tube.

The vacuum in the tubes used in these tests was very high—so high, in fact, that the so-called "gas oscillations" could not be obtained with them.

This demonstration was made in order to show that the contention of Defendant's Experts that a two electrode high-vacuum tube would not oscillate, was incorrect. There is, in this respect, an interesting resemblance to the case before Judge Mayer, in which Experts for the Defendant

testified, somewhat emphatically, relative to the impossibility of making any two-electrode Fleming valve oscillate.

In both of these cases, that is the one before Judge Mayer, and the present one, the contention has been met by a demonstration showing that the two-electrode Fleming valve possesses inherently the same capabilities as the three-electrode valve.

It is also of importance to note that the expressions "freak action," "great instability," "impossibility of commercial employment"—which have been used by Defendant's Experts in this case to characterize the device before Judge Mayer, are entirely inapplicable to the recently made tests.

The oscillations obtained with this high-vacuum two-electrode tube are as stable and controllable as those obtained with the three-electrode tube and were the latter unknown, the two-electrode tube so used would have very important commercial usefulness.

Defendant's Experts, in explaining the action of the three-electrode valve, and in attempting to differentiate it from the three-electrode valve, have laid great stress on the electrostatic control of the electron stream, and have contended that this control was an inherent part of the three-electrode structure.

They have neglected to point out, however, and to recognize, that the two-electrode tube was capable of being influenced and operated by an electrostatic control of the electron stream between the electrodes, and that in the three-electrode tube the grid was merely the mechanical arrangement by means of which the controlling electrostatic field was brought within the electron stream to the place of greatest effectiveness.

They have entirely neglected to differentiate between the grid as a part of the valve structure and the electrostatic field, which is the vital thing in the controlling action.

[fol. 1337] DeForest made a particular sort of an invention when he put the grid in the Fleming valve. He did not discover the electron stream, as that was known previously, and the Fleming valve embodied the best method of producing it which is known up to the present time, the hot electrode and the cold electrode. He did not discover that such an electron stream could be modified by an electrostatic field, because that also was known before his invention.

Statements of Defendant's Experts which tend to create the impression that the grid and the electrostatic control are one and the same thing are, therefore, not founded on the facts.

The grid as a structural device in a Fleming valve is DeForest's invention, and one of very great importance; but, while this invention added greatly to the usefulness of the Fleming valve, it did not add to it any mode of operation which is not possessed by the two-electrode tube without the third or grid-electrode.

That DeForest was familiar with Fleming's work is shown by his statement in his patent, No. 823,402 (page 3, lines 7-30, inclusive):

"The device V' (shown in Fig. 1), connected between the antenna at the point *b* and earth, is an asymmetric resistance or electric valve which has been fully described by J. A. Fleming in a paper published in the *Proceedings of the Royal Society of London*, March 16, 1905, to which reference may be had for a more complete description thereof than need be set forth herein. Suffice it to say that the exhausted glass vessel Q" contains the filament F', heated to incandescence by the battery B', and a metal cylinder D', which surrounds said filament F'. This type of valve passes positive electricity from the cold terminal D' to the heated terminal F' more readily than in the opposite direction, and hence when the switch S' makes contact, as shown in Fig. 1, with the contacts 5 and 6 positive static effects will pass to earth by way of the elements D' and F' of said valve, while if the connections be reversed by throwing the switch so as to contact with 4 and 5 negative static effects will pass to earth by way of the elements F' and D' of said valve."

It will be noted that this application was filed in December, 1905, considerably earlier than the date of application of the DeForest patent No. 879,532, which discloses the grid, and which was applied for January 29, 1907.

Mr. Vaill: Counsel for claimant offers in evidence a printed copy of Patent No. 823,402, dated June 12, 1906, to Lee DeForest, and requests that the same be marked "Claimant's Exhibit No. 258."

(Said exhibit marked as requested.)

Q. 249. Will you please state, from a physical standpoint, what are the essential elements and mode of operation of the two-electrode and three-electrode vacuum tubes, which give them the inherent capability when associated with [fol. 1338] proper circuits, to produce radio waves or to oscillate?

A. Both the two-electrode Fleming valve and the three-electrode Fleming valve have the capability of oscillating in the same manner, and their ability to oscillate depends on two things, assuming that the vacuum is high, and that we have no "gas action" to consider, first, that an electron stream shall flow between the hot electrode and the cold electrode, and that an electrostatic field shall be associated with this electron stream in a manner which permits it to have an influence upon the electron stream. The nature of this influence must be such that the energy involved in the influencing action shall be less than the energy released by the resulting variation in the electron stream.

The second requirement is that a circuit in which electrical currents are flowing, caused by the variation of the electron stream, shall be associated with the electrostatic field, which controls the electron stream, in such a way that variations of this electrostatic field are supported by variations of the current produced by the electron stream variations.

Both the two-electrode arrangement, as shown in my recent tests, and the three-electrode arrangement now fulfill these requirements, and they differ only in the degree of perfection with which the control of the electron stream is obtained.

In the three-electrode tube this field is brought into the midst of the electron stream and in a position where it has the maximum effect.

In the case of the two-electrode tube the field is external. It is not so advantageously situated with respect to the electron stream and its control, therefore, is less, but nevertheless, as the tests demonstrated, it is sufficient both for oscillation and amplification. Both tubes, that is, the two-electrode valve and the three-electrode valve, oscillate with an equal degree of stability and controllability, and both are entirely reliable for commercial purposes.

Both the two-electrode valve and the three-electrode valve possess the fundamentally important thing, viz., an

electron stream originating in a hot cathode and flowing to a cooled anode, which electron stream in both devices, can be influenced by an electrostatic field in such a way that both amplification and oscillation are secured.

Q. 250. Please refer to the paragraph at the bottom of page 760 of the Printed Record, where Dr. Miller criticises Mr. Waterman's deposition, and concludes by stating that:

"The high vacuum two-electrode tube does not have this 'falling characteristic' and will not generate oscillations."

Will you please state whether, and under what conditions, a high-vacuum two-electrode tube does involve an action having a falling characteristic.

A. Dr. Miller's criticism of Mr. Waterman's deposition is incorrect, both with respect to the falling characteristic and his statement that it will not generate oscillations. That the high-vacuum two-electrode tube will generate oscillations was, of course, conclusively demonstrated in my recent tests, and when either a two-electrode or a three-electrode device is oscillating, it has a negative char- [fol. 1339] acteristic or, as it is often stated, a negative resistance reaction.

Mr. Miller's statement, quoted above, and Dr. Milliken's statements made in the course of his deposition, to the effect that neither the hard two-electrode tube nor the hard three-electrode tube has a falling characteristic, seems to be based on a consideration of the tube by itself and these statements fail to take into account the fact that it is a characteristic of the tube, when associated with the actual operating circuits that is of real consequence.

Neither the two-electrode tube nor the three-electrode tube, when taken alone, is capable of oscillating, and consequently it cannot be said that either of these is a generator of electrical oscillations except when associated with, and connected to, suitable circuits. When such a connection and association is made, however, both of these devices can oscillate and they do so because of the possession of this negative resistance reaction.

The accompanying sketch, being "Weagant Sketch No. 4" (* * * Claimant's Ex. 259), forms the basis for illustrating in a simple manner the negative resistance reaction of the three-electrode tube.

In this sketch, A is the filament, G is the grid, B is the plate, D is the battery, while the coil E and the condenser F

constitute an oscillating circuit. Attached to the grid G is a variable connection indicated by the line terminating in an arrow.

If the oscillating circuit E, F, has its resistance measured when the three-electrode valve is completely disconnected from it, a certain value will be found. If, then, the three-electrode valve is connected in the circuit as shown, a measurement of this resistance under these circumstances will show that the resistance of the circuit E, F, has increased, showing that the attachment of the valve in the manner indicated in the sketch results in adding a certain amount of the resistance to the resistance of the circuit E, F.

If, then, the pointer I is attached to the coil E at a point determined by experiment, which will be slightly below the point H, a point can be found such that a further measurement of the oscillating circuit E, F will show that it has the same resistance as it had when the valve was completely disconnected, and this means that under these conditions the valve is equivalent to a zero resistance inserted in the circuit E, F.

Another way of expressing this is to say that under these particular circumstances, the valve gives rise to no loss in the circuit E, F.

If, next, the point I be moved somewhat farther down the coil E, and another measurement of the resistance of the circuit E, F, be made, it will be found that its resistance is less than the resistance of the circuit taken alone and with the valve entirely disconnected. This very remarkable fact indicates that with this particular connection the valve has actually removed some of the positive resistance of the circuit E, F; or, in other words, it has developed a negative resistance reaction of some particular value.

If once again, the point I be moved further down the coil E, a point will be reached where the circuit E, F, will begin to oscillate, and this means that the negative resist-[fol. 1340] ance reaction due to the valve, has increased to the point where it exceeds the positive resistance of the circuit E, F.

This particular circuit is the well-known Hartley circuit shown as Fig. 2 in Miller Drawing J, opposite page 804 of the Printed Record.

That this particular action and capability of the three-electrode valve is recognized by workers in this art is shown by patent No. 1,334,165 to M. I. Pupin and E. H. Armstrong. These two gentlemen, both of whom are recognized authorities in radio, have disclosed herein a radio system which depends for its action and results upon the negative resistance reaction of the three-electrode tube. Various curves are given, such as Fig. 2a, and Fig. 5, illustrative of the negative resistance effects and it will be noted that there is a horizontal line which is marked O; above this, numbers are given representing positive ohms and below it, numbers representing negative ohms, and the ohm is the electrical unit of resistance.

The diagrams, Fig. 1, Fig. 3, Fig. 4, and others, clearly show that it is the three-electrode device which is being used for this purpose.

Examination of the specification shows many references to the fact that the arrangement is intended to develop a negative resistance reaction. and on page 1, lines 59 to 86, inclusive, there is a description of the arrangement of Fig. 1, and this description is followed by the following statement, lines 87-90:

"It is this transfer of energy which manifests itself through a reaction between 2, 5, which has all the properties of a negative resistance reaction."

The particular kind of negative resistance exhibited by the three-electrode tube when oscillating, or when used in the manner of Pupin and Armstrong, might be properly called "the alternating current negative characteristic," since it is not manifest in any way when a direct current or potential is applied to the device.

This, however, is not the only sort of negative characteristic or resistance reaction of which the electrode tube is capable.

In patent No. 1,393,594 to White (* * * Claimant's Ex. 261), a type of negative characteristic possessed by the three-electrode tube is shown, which results from the application of a direct current or potential to the grid of the device.

Fig. 2 represents a curve obtained by applying a negative voltage to the grid, and it will be seen that at point B,

where the negative voltage approaches zero, that the distance of the curve below the line marked "Grid Voltage" is much greater than at the point C, where the negative voltage is much greater. That is to say, increased voltage is associated with decreasing current, which is the condition referred to by Mr. Miller as the characteristic action of an arrangement to which the name of "Negative resistance" or name of "Negative Characteristic" applies.

Mr. White, in his patent, lines 32 to 50, page 1, brings out this action, as follows:

"In carrying my invention into effect, however, I employ a materially different principle. I have found that under [fol. 13[illegible]] certain conditions with devices of this type the current in the grid circuit may have a dropping characteristic, that is, as the voltage impressed upon the grid increases the current in the grid circuit will decrease. With the proper conditions for operation a circuit having current characteristic of the type described may be so organized that oscillations will be produced therein, the essential condition for the production of oscillations being that the circuit shall contain capacity and inductance and that the resistance of the circuit shall be less than $2\sqrt{\frac{L}{C}}$ where L represents the inductance and C the capacity of the circuit."

It will also be noted that Mr. White makes use of this particular characteristic for the purpose of generating oscillations in a manner which is quite different from the usual feed-back arrangement.

It is also to be noted that this is a characteristic possessed by tubes of relatively high vacuum, such as are used in every-day practice.

This I know from my own experience, and a statement by Mr. White, page 2 of his patent, lines 52 to 60, is indicative of the fact that the tube therein referred to was worked with a plate potential of 375 volts, which is a voltage that cannot be employed except with very good vacuum.

The particular negative characteristic just illustrated by Mr. White's patent is obtained as the result of applying a negative potential to the grid, but a similar negative characteristic is obtainable through applying a positive potential to the grid.

This is illustrated in the paper by Albert W. Hull, entitled "The Dynatron," a vacuum tube possessing negative resistance in the Proceedings of the Institute of Radio Engineers, February, 1918, Vol. 6, No. 1, pages 5 to 36, inclusive (* * * Claimant's Ex. 262).

This device is a three-electrode vacuum tube which requires merely that the potential applied to the grid shall be considerably higher than the potential applied to the plate, in order to develop the negative resistance reaction.

Fig. 3 shows a curve and the voltage increases from zero to 150 volts and more. It will be seen that, while the voltage is increasing from zero to the point A_0 that the current increases, after the fashion of any ordinary device having positive resistance, and from the point A_0 to the point 100, increasing voltage is associated with decreasing current, and between the points B and C_0 the current actually flows in a direction opposite to the applied voltage.

The device, therefore, between the points A_0 and C_0 is, as stated, just above the figure, a true negative resistance.

The arrangement described by Dr. Hull in his paper, forms the subject-matter of his patent No. 1,385,873, which was applied for June 5, 1916, and issued July 26, 1921.

While in physical construction this device appears different from the conventional three-electrode, this is due largely to the fact that the device as illustrated was in- [fol. 1342] tended to handle suitable power, and also because, due to the relatively high voltage applied to the grid, the conventional three-electrode tube construction would seriously overheat if used this way.

The same characteristic is, however, inherent in the conventional three-electrode tube, and is recognized by Dr. Van der Bijl in his book "Thermionic Vacuum Tube"; Fig. 75, page 153 (* * * Claimant's Ex. 263), shows a curve marked at its right-hand extremity Ep = 200. This curve from a point represented by about 15 volts to something over 35 volts, is a characteristic which is the same as that shown by Dr. Hull.

Still another manifestation of the negative resistance reaction of the three-electrode tube is to be found in a work entitled "Radio Engineering Principles" (by Lauer and Brown). On page 177 there is the following statement:

"If now the grid potential Eg is varied so as to increase the current Ip in the plate circuit, it is seen that

the resistance drop RIp in the external plate circuit will correspondingly increase. It then follows from equation (30) that, with the battery voltage EB remaining constant, the plate potential Ep will decrease. Conversely, if the current Ip is decreased by decreasing the grid potential, the plate potential Ep will increase. Thus, on account of the external resistance of the plate circuit, the plate potential is no longer constant, but is a function of the plate current, and varies in opposite direction to the latter. In other words, the tube, operated under such conditions, has a negative plate resistance r ction as defined in connection with the oscillating arc, Chapter V."

The circuit diagram which goes with this statement is Fig. 145, page 175.

Mr. Vaill: Counsel for claimant offers in evidence the following exhibits and requests that the same be marked as indicated:

"Weagant Sketch No. 4," "Claimant's Exhibit No. 259."

U. S. Patent to Pupin and Armstrong, No. 1,334,165, March 16, 1920, "Claimant's Exhibit No. 260."

U. S. Patent to White, No. 1,393,594, October 11, 1921, "Claimant's Exhibit No. 261."

Photostat copies of pages 5 to 11, inclusive, of the "Proceedings of the Institute of Radio Engineers for February, 1918, being the first seven pages of the article entitled "The Dynatron" by Albert W. Hull, as "Claimant's Exhibit No. 262."

Photostat copy of page 153 of the book entitled "The Thermionic Vacuum Tube" by Van der Bijl, as "Claimant's Exhibit No. 263."

Photostat copies of pages 175, 177 and 178 of the book entitled "Radio Engineering Principles" by Lauer and Brown, First Edition, Third Impression, 1920, as "Claimant's Exhibit No. 264."

(Said exhibits marked as requested.)

Q. 251. Will you please refer to Dr. Miller's answer to [fol. 1343] Q. 5 of his deposition, page 763 of the Printed Record, regarding the case of Hewitt *v.* American Telephone & Telegraph Co., and state whether you consider, from a technical standpoint, the devices and mode of operation disclosed in the Hewitt patents Nos. 781,001 and 781,002

("Defendant's Exhibits O and P"), are the same thing as the gaseous two-electrode tube demonstrated before Judge Mayer in the case of Marconi Wireless Telegraph Company of America *vs.* De Forest Radio Telephone and Telegraph Company? You may refer to any part of Judge Mayer's opinion which tends to confirm your understanding of the Hewitt patents.

* * * * * * *

A. The device illustrated in the Hewitt patents Nos. 781,001 and 781,002 is a very different thing from the vacuum valve of Fleming, both in structure and in usefulness. The Hewitt arrangement involves the use of a metallic vapor, viz., the vapor of mercury, and it does not have the hot electrode of the Fleming valve, and is not capable of providing a pure electron stream whose motion is unimpeded by the presence of gas, as is the case in a properly exhausted Fleming valve.

While Judge Mayer, in the part of his decision quoted by Mr. Miller, distinguishes the operation of this Hewitt device from that of the DeForest three-electrode tube, this distinction is not one which is applicable to the Fleming valve, since, as has been conclusively demonstrated by the tests which I made, the Fleming valve with only two electrodes, is capable of acting on the so-called relay principle.

Judge Mayer, in his opinion in the case referred to by Mr. Miller, makes a further statement which appears of interest at this point. On page 203, 272 F. R., he states as follows:

"Not only have these two patents been of no practical service in this art, as against the undoubted utility of defendant's devices, but it is plain that, giving them their fullest academic value, they have a different object, principle, mode of operation, and result from these which characterize the audion type of repeater used by defendant."

In considering and comparing the Fleming valve which has a vacuum of a high order, but not of the highest order such as is obtainable today commercially, and the Hewitt devices, there is a most important distinction to be made relative to the nature and function of the gaseous content of the evacuated tube.

In the Hewitt device, mercury vapor is the essential con- [fol. 1344] ducting element, and if not present, the device

ceases to exist, while in the Fleming arrangement the residual air or other gases which exhaustion has failed to remove, are not the essential conducting element, nor do they play any useful part in the operation of the device. They are, on the contrary, in the nature of an obstruction to the flow of the electron stream, and the more perfectly they can be removed, the more perfectly will the device operate.

In the Hewitt device a metallic vapor, viz., that of mercury, is the fundamental basis for the operation, while in the Fleming device an electron stream is the fundamental basis, and the less the electron stream is interfered with by the gas, the better it functions, and in the Fleming patent are explicit instructions for making this vacuum as high as possible.

Q. 252. Referring to Dr. Miller's answer to Q. 6 of his deposition, please state to what extent you may agree or disagree as to the fundamental operation of the two-electrode and three-electrode valves, as explained by him in connection with "Miller Drawing B"?

A. Mr. Miller gives, in his answer to Q. 6, an explanation of the action of the three-electrode valve which is quite in accordance with the usual technical viewpoint in the matter.

In his explanation of the two-electrode tube, he chooses to limit his explanation to the mere rectifying action possessed by the two-electrode tube. He does not, of course, show the two-electrode Fleming valve in the Fleming circuit, nor does he bring out the fact that this two-electrode valve is capable of functioning by means of an electrostatic field control of the electron stream.

In his explanation of the action of the three-electrode device, he shows clearly how the grid serves to control flow of electrons to the plate from the filament, but he omits to point out that a similar control is possible with the two-electrode tube by merely bringing it in the neighborhood of an external electrostatic field.

He also chooses, in his diagram, to show the Fleming valve without a so-called B battery, but while it is true that the Fleming patent does not happen to include such a battery, yet its use with a radio receiving device was well known in the prior art, and Fleming himself, in an article in the "Proceedings of the Royal Society of London," Volume 74, page 476, page 483, gives characteristic curves which clearly show the effect of the application of a battery poten-

tial between the plate and filament and in general discloses in these curves information which would lead to the use of such a battery for radio purposes.

I might mention also at this point that the Fleming valve used as a simple detector in receiving sets manufactured for commercial use, was provided with a means for applying a potential between the plate and filament.

Dr. Miller, in concluding his answer to Q. 6, sums up his explanation as follows (P. R., p. 768):

"This explains the remarkable amplifying property of the three-electrode vacuum tube which is due to the DeForest grid and which is responsible for the important position of the device in the electrical art."

[fol. 1345] With this conclusion of Dr. Miller's I disagree very emphatically, for the very simple reason that the remarkable amplifying property referred to is not, as he states, due to the DeForest grid, but is due to the fact that the electron stream of the Fleming valve can be controlled by an electrostatic field, releasing as the result of this control more energy than is consumed by the controlling action.

The DeForest grid inserted in the Fleming valve is the most efficient way of bringing the electrostatic field into such relation with the electron stream as is required by the conditions of operation, and is an invention which has proven to be of enormous importance.

Q. 253. Referring to Dr. Miller's answer to Q. 7 (P. R., p. 768), will you please comment upon the similarity of action of the vacuum tube when acting as an amplifier of radio-frequency oscillations and of audio-frequency currents?

A. Mr. Miller, in discussing this subject in his answer to Q. 7, has brought out the fact that owing to the lack of inertia of the electron stream, it was capable of responding equally well at high frequencies, or at low frequencies. This, of course, was the discovery of Dr. Fleming, who was the first to show that a vacuum tube with a hot and a cold electrode would operate when acted upon by high-frequency oscillations.

While it happens that the vacuum tube functions in this way with respect to all frequencies, it must be remembered that this is not a characteristic of any and all devices which

have the property of rectifying. Many rectifiers operate effectively on alternating voltages of low frequency, but are quite useless when employed with high frequencies.

Q. 254. Do you agree with Dr. Miller, in his answer to Q. 8 (P. R., p. 769), as to the contrasts which he has stated exist between the uses of the two-electrode valve and the three-electrode valve? If not, please state in what respects, and give your reasons.

A. Dr. Miller, in reply to Q. 8, states that the two-electrode valve can only be used as a rectifier, while the three-element device may be used as an amplifier, a combined amplifier and radio-frequency detector and oscillation generator, and a combined oscillation generator and radio-frequency detector. He, furthermore, refers to what he calls the "freak gas action" as being a useless laboratory "stunt," and of no practical utility.

From the facts disclosed by the recent demonstration which I made of the two-electrode tube, it is apparent that Dr. Miller, in making this reply, has quite overlooked the relay type of action of which the two-electrode tube is capable. These tests disclosed very clearly and unmistakably the fact that the two-electrode Fleming valve is an amplifier, combined amplifier and radio-frequency detector and oscillation generator, and a combined oscillation generator and radio-frequency detector.

In other words, it is capable of all the uses of which the three-electrode tube is capable.

Dr. Miller, on Printed Record, page 770, uses a definition of the word detector, which is limited in a particular [fol. 1346] way, viz., to the conversion of high-frequency current or voltage into another form.

In the Report of the Committee on Standardization for 1922 on behalf of the Institute of Radio Engineers, is given the definition of the word detector which is considerably broader in scope, and which carries the significance more usually associated with this word. This definition is as follows:

"Detector: That portion of the receiving apparatus which, connected to a circuit carrying currents of radio-frequency, and in conjunction with a self-contained or separate indicator, translates the radio-frequency power into a form suitable for operation of the indicator. This translation may be effected either by the conversion of the radio-

frequency power, or by means of the control of local power. The indicator may be a telephone receiver, relaying device, tape recorder, and so on."

It will be noted that the above definition conveys as its essential impression the fact that the detector is a device which, when acted upon by radio-frequency, translates this action into one capable of operating an indicator.

At the bottom of P. R., p. 773, Dr. Miller makes a differentiation between an amplifier and a detector and refers to the latter as distorting the radio-frequency.

As this word is capable, that is, the word "distorts," of misinterpretation, it seems important to point out here that this means merely that the valve is rectifying the incoming wave.

In the strictly technical sense, this statement is perfectly correct, but used in its ordinary significance it seems to infer that the valve is modifying the wave form of the incoming signal in an undesirable way; whereas, as a matter of fact, this modification, rectification or, as it is technically termed, "distortion," is the necessary action in order to make the incoming high-frequency oscillations usefully evident. When an incoming oscillation is rectified, one-half of it is cut off, but the sound resulting therefrom is not distorted.

Dr. Miller, in his further discussion under Q. 8, outlines the various uses of the three-electrode tube as amplifier, oscillator, detector, etc., and enlarges somewhat on the great importance which these things have in the radio art. Statements which are unquestionably true, but which fail to include any reference to the same inherent capabilities, as shown in my tests, of the two-electrode tube.

Q. 255. Please state to what extent, if any, you agree with Dr. Miller's statement regarding the similarity of action of the two-electrode vacuum tube and the arc.

A. Dr. Miller, in replying to Q. 11 (P. R., p. 781), brings out the action of the oscillating arc and the similarity between it and the oscillating Fleming valve demonstrated to Judge Mayer. He also quotes a statement of Mr. Waterman to the effect that the two-electrode tube does not in any way resemble the arc.

There seems to be some confusion which, I think, is readily cleared up. At the time that the demonstration [fol. 1347] was made to Judge Mayer, while we had suc-

ceeded in making the two-electrode tube oscillate, we did not know the mechanism which was responsible for the action.

Both Mr. Waterman and I were familiar with the characteristic of the arc variously known as "falling characteristic," "negative characteristic," but no tests which we were able to make at that time, succeeded in showing the existence of such a characteristic. Since that time, however, greater knowledge relative to the action of both the two-electrode and the three-electrode valve has been obtained, and I have succeeded in making tests which show the falling characteristic to exist in the two-element valve.

Mr. Waterman's statement, therefore, to the effect that the two-electrode tube did not in any way resemble the arc was perfectly true, since the two devices physically, and in their general action, are very different, while the fact that the two-electrode tube possessed the falling characteristic, which is also possessed by the arc, was quite unknown to any of the witnesses for the plaintiff, at that time.

As is usual in scientific work, more and more light is thrown on the action and capabilities of various devices, and so it happens that we understand better the action of the two-electrode tube demonstrated to Judge Mayer, and, in addition, the fact that this two-electrode tube is quite capable of acting in a manner entirely independent of the particular sort of negative characteristic which was responsible for the action of these tubes during these tests.

My recent tests show conclusively that the simple two-electrode tube operating by means of the electrostatic control, not only accomplish all that was shown in the tests before Judge Mayer, but, in addition, all of the characteristic action of the three-electrode tube.

Q. 256. Will you please refer to Dr. Miller's answer to Q. 12, and particularly the last paragraph thereof on P. R., p. 784, and state whether or not you agree with Dr. Miller's conclusions as to the possibility of oscillation when using a hard two-electrode tube?

A. Dr. Miller, in his reply to Q. 12, has apparently elaborated on the difficulties which he experienced in making a two-electrode "soft" tube oscillate, but, as is very often the case, in tests of this sort, he would have found it much easier to do had he followed the demonstration before Judge Mayer.

He also refers t attempts to make the hard two-electrode tube oscillate, and states that the failure experienced is to be expected, since this tube has a rising characteristic. It should be noted that the three-electrode tube also has a rising characteristic and yet, as Dr. Miller points out, is a generator of oscillation.

As a matter of fact, it is perfectly obvious that the tests made by Dr. Miller were not conducted in a proper way to demonstrate the fact that a two-electrode tube is capable of oscillating, but rather in a way which, in the light of the present-day knowledge of vacuum valves, had no possibility of working.

The hard vacuum two-electrode tube employed in the tests which I recently made would, if taken alone, show a rising characteristic; nevertheless, when used in the origi-
[fol. 1348] nal Fleming circuit and merely laid on a board in the vicinity of a coil or condenser of that circuit, which arrangement permitted the electrostatic field to operate, oscillations of great constancy and controlability were obtained.

Q. 257. Referring to Dr. Miller's answer to Q. 13 on page 784 of the Printed Record, will you please state from your knowledge of the tests before Judge Mayer, whether they showed that variations in the oscillations were present or not?

A. In the tests before Judge Mayer, one demonstration involved the reception of oscillations from a two-electrode oscillator by a three-electrode oscillating valve and the production in the telephone receivers of a heterodyne beat note. This particular method of detection is very sensitive to variations in the oscillation-frequencies, and were such present, a note of variable pitch would be heard.

I do not recollect that the note heard in the tests before Judge Mayer showed this variation, while I have a distinct recollection of having made tests with the same apparatus in which I noted particularly that the pitch of the beat note was quite constant.

Q. 258. Do you agree with Dr. Miller's statement in answer to Q. 16 (P. R., p. 785), regarding the necessity of gaseous two-electrode tubes having gas present to a definite degree in order to act as an oscillation generator?

A. This question, and Dr. Miller's answer thereto, seem based on the assumption that a two-electrode tube is capa-

ble of oscillating only by means of the gas action. Dr. Miller has already testified to the fact that he was unable to make the hard two-electrode tube oscillate. That was simply because he did not use it in the right way.

There are many ways of associating and connecting circuits with the three-electrode tube of such a nature that oscillations are not obtainable therefrom, and the same is, of course, true of the two-electrode tube.

As I have already pointed out, neither the two-electrode tube, nor the three-electrode tube can, without the association of proper circuits, give rise to oscillations. While, on the other hand, as I have shown in my demonstrations, the two-electrode tube in the proper circuit and association, oscillates very readily when the vacuum is so high that the residual gas plays no part in the action.

Q. 259. Please comment on Dr. Miller's answer to Q. 17 of his deposition (P. R., p. 786), referring particularly to "Miller Drawing F", used by Dr. Miller in explanation of his answer.

A. Dr. Miller, in answer to Q. 17, compares the operation of the three-electrode vacuum tube in the P X circuit and the Fleming valve in the circuit of the Fleming patent. He brings out, of course, his usual distinction betwen these two devices, viz., that while the Fleming valve is a detector, the three-electrode vacuum valve is an amplifier as well.

Of course, this fails to take into consideration the facts disclosed by my tests, viz., the use of the Fleming valve in the circuit of the patent with, of course, a battery added as a source of power, is also an amplifier.

In Fig. 2 of "Miller Drawing F" is shown a circuit of the [fol. 1349] De Forest patent No. 879,532 and a particularly interesting detail of this is the condenser f^3. It will be noted that there is no grid leak associated with this, after the fashion of Fig. 3, *r*, and in the present-day hard tube, such an arrangement is practically inoperative.

This is an interesting illustration of the fact that developments of an important invention often lead to radical changes in the operating associations.

When the three-electrode tube was first made, the vacuum was relatively low and the positive ionization was sufficient to act as a leak and prevent the grid from acquiring a negative charge.

With the modern high-vacuum tube, however, this internal condition no longer exists and an external leak is essen-

tial. In connection with Dr. Miller's explanation of the action of Fig. 3, he brings out the fact that the grid-to-filament circuit of the three-electrode tube is a rectifier, a fact which is essential to the operation of the device not only as a detector, but as is frequently lost sight of, to its action as an amplifier. In one way or another, regardless of what purpose the valve is used for, rectification takes place in the grid circuit and the use of a condenser in series with the grid is identical with the common use of such a condenser with the Fleming valve.

In the work by Dr. Fleming, entitled "Wireless Telegraphers Pocket Book", page 249, Fig. 36, there is shown a circuit diagram containing a condenser C_1 which circuit and condenser are substantially identical with the grid circuit j^2, h', j^3 of Fig. 2 of "Miller Drawing F". Such a circuit could not operate unless either the valve contained sufficient positive ionization, due to gas, to discharge condenser C_1, or else condenser C_1 was leaky.

I recollect having taken apart a commercial Marconi tuner having such a condenser in its circuit, and of being somewhat puzzled as to how it was operating.

I tried the valve in an external circuit, the same as that in the tuner, but using a different condenser, and it wouldn't work. This led me to investigate the condenser in the instrument and I found it to be leaky to an extent sufficient to render it operative.

Dr. Miller goes on to explain further in his answer to Q. 17, the operation of Fig. 3, which is the regenerative oscillating arrangement of the three-electrode tube, but he omits, of course, to point out that the two-electrode tube in the circuit of Fig. 1 (Drawing F), operates in precisely the same way as was brought out in my recent tests.

While Dr. Miller states that in the Fleming circuit the feeble-received oscillations must themselves supply the power to actuate the indicator and that in the three-electrode tube power is supplied by a B battery, he is imposing a restriction upon the Fleming arrangement for which, in my opinion, there is no justification, since the use of a battery with a detector of wireless waves was old and well known at the time of the Fleming patent and it is quite obvious that either the three or two-electrode vacuum valves must have some source of power attached to them [fol. 1350] if they are to generate oscillations, since neither tube, in itself, is a source of power.

On page 790 of the Printed Record, Dr. Miller makes the following statement:

"* * * On the other hand, the three-electrode device in the P N circuit as well as in the regenerative circuit constitutes essentially a different type of vacuum tube connected to two separate circuits neither of which performs the functions of the Fleming circuit. In the P N and regenerative circuits, while the received oscillations are increasing in amplitude, they produce rectified current pulses which charge the grid condenser, which charges leak away when the received oscillations are decreasing in amplitude or are not being received. Such rectification as occurs is not indicated in either circuit. Even if it were possible to observe this rectification it would not reproduce the audio-frequencies which the detector is intended to indicate. The rectification and flow of current is an incidental action of the tube in properly charging and discharging the grid condenser."

The first sentence of the above quotation contends that the three-electrode device is an essentially different one from the two-electrodes, which statement is quite contrary to the facts, as I have brought them out in my various answers, and it is also incorrect in stating that neither of these two circuits of the three-electrode device perform the function of the Fleming circuit. As will be seen by reference to the diagram, Fig. 1, "Miller Drawing F", the Fleming circuit performs the two essential requirements of a device used to detect radio signals. It impresses upon the sensitive device the received oscillations by means of the inductance *k*, and it contains the indicator I, which operates as the result of the change wrought upon these oscillations by the valve.

In the three-electrode tube with the P N circuit, these two actions are effected by two separate circuits, the oscillations being impressed on the vacuum valve by means of the circuit j^2, h', j^3, while the circuit I B contains the indicating mechanism operated by the action of the three-electrode device upon the oscillatory currents.

His further statement to the effect that such rectification as exists, is not observed, and even if it were possible to observe it, would not reproduce the audio-frequency variations which the detector is intended to produce, is so en-

tirely at variance with the facts that it seems as though Dr. Miller must have been confused in this answer.

When a circuit of the type shown in Fig. 3 is employed for the reception of signals, it is the rectification in the grid circuit which makes these signals audible, and without such rectification there would be no indication of the telephone receiver.

His further statement that the rectification is merely incidental in properly charging the grid condenser, is a case of "putting the cart before the horse" for the simple rea-[fol. 1351] son that the rectification is the fundamental action and the reason why the grid condenser is charged, so that the latter is merely the result of the former.

Were there no rectification in the grid circuit, this charging and discharging of the condenser in the manner explained by Dr. Miller, could not take place, and it constitutes one of the basic reasons for the operation of the three-electrode tube.

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Q. 260. Please refer to Dr. Miller's answer to Q. 18 (P. R., p. 791), and state to what extent you may agree or disagree with him as to his stated contrasts between the operation of the two-electrode tube and the three-electrode tube, when a battery is employed.

A. Dr. Miller has given an explanation of the action of a biasing battery which is similar to the explanation which I have given in the matter of imperfect electrical contacts, and I note that he includes the Fleming valve as a particular type of detector whose operation may be improved by the addition of a battery.

In referring to the Fleming valve he makes his usual statement to the effect that it is not an amplifier, a statement which, of course, is disproven by the results of the demonstration which I recently made, and which showed quite otherwise.

He confirms my statement to the effect that the use of a battery with an amplifier is obvious and necessary, in the following words at the top of P. R., page 793:

"The use of a battery with a detector is optional; the use of a battery with an amplifier is indispensable by reason of the law of conservation of energy."

As I have previously mentioned, the use of a battery with a detector was part of the teaching of the prior art; as, for instance, in its use with the coherer. In this particular use the action of the battery differed in an important sense from the ordinary detector, such as the crystal detector, because in general the amount of energy released by the incoming signal was considerably greater than that in the signal itself, as is shown by the fact that this released energy was capable of operating a relay device.

It so happens that I have done a considerable amount of work with relay devices operated by the modern three-electrode tube, and I know that a very substantial amount of amplification is necessary for the purpose. From this it follows that the coherer was a species of amplifier; another use of the battery with a detector of electrical oscillations which was prior to the Fleming patent, is shown in the patent to R. A. Fessenden, dated May 5th, 1903, No. 727,331 ("Plaintiff's Exhibit No. 82-5" in Marconi Wireless Telegraph Company of America *vs.* DeForest Radio [fol. 1352] Telephone & Telegraph Company). This is the liquid barreter and was an extremely sensitive detector.

The two illustrations given above, together with the paper by Dr. Fleming, giving the characteristic curves of the two-electrode valve are, in my opinion, sufficient justification for the employment of a battery with a Fleming valve. Another use of a battery with a detector of the electrolytic type is shown in patent No. 780,842, issued to F. K. Vreeland, patented January 24, 1905 ("Plaintiff's Exhibit No. 82-10," in the case of Marconi Wireless Telegraph Company of America vs. DeForest Radio Telephone & Telegraph Co.), while patent No. 755,840, dated March 29, 1904, to J. C. Bose ("Plaintiff's Exhibit No. 82-9" in the same case), discloses the use of a battery with a crystal detector.

In addition to the instruction contained in the prior art to use a battery, I may add that a rather extensive experience in the construction and use of the Fleming valve as a detector has brought out the fact that some of them work best when the oscillatory circuit is connected to the negative side of the filament, as shown in the Fleming patent; that others work best when connected with the positive side, while others, again, work best with a substantial amount of added potential.

In the ordinary commercial use of the Fleming valve as a detector, it was customary to supply a means for varying this potential.

In conclusion, I will sum up my remarks relative to Mr. Miller's answer to Q. 18 by stating that the Fleming valve may not only be improved as a detector by the addition of a separate battery, but may also be effectively used as an amplifier, when such separate battery is employed.

Mr. Vaill: Counsel for claimant offers in evidence printed copies of the patents referred to by the witness, and requests that they be marked as indicated:

No. 727,331, to R. A. Fessenden, May 5, 1903, "Claimant's Exhibit No. 265."

No. 780,842, to F. K. Vreeland, January 24, 1905, "Claimant's Exhibit No. 266."

No. 755,840, to J. C. Bose, March 29, 1904, "Claimant's Exhibit No. 267."

(The said exhibits marked as requested.)

Q. 261. Will you please refer to Dr. Miller's answer to Q. 19, particularly where he criticises Mr. Waterman's statement regarding "Plaintiff's Exhibit No. 110," with especial reference to the three tubes or bulbs T, T^2 and T^3 (P. R., p. 796-; also the statement contained in the first paragraph (P. R., p. 798), and also the statement following said paragraph, pointing out to what extent you may agree or disagree with Dr. Miller in said portions of his answer?

A. Dr. Miller's criticism of Mr. Waterman's statement is evidently based on the particular definition of the word "detector" which he has chosen to employ. If, however, the broader interpretation of the word detector, such as that given by the Standard's Committee of the Institute of Radio Engineers, and to which I referred in my answer to Q. 254, and which, incidentally, was the general understanding in the art until the development of the three-electrode valve technique narrowed its significance, Mr. Waterman's statement appears as anything but nonsensical.

[fol. 1353] As will be seen from a reference to the definition given, the function of the detector is to translate the energy of the incoming signal into a form of energy which will operate the indicator, and this translation may be accompanied by, or result in, the release of local energy.

The three detector bulbs referred to by Mr. Waterman operate in this identical way, since their action is to translate the effect of the incoming oscillations into a form of energy which will operate the telephone receivers, and this translation is accomplished through the release of local energy.

This action may be viewed also in another light, as follows:

In the early days of radio, a detector was any arrangement which would, when acted upon by an incoming signal, actuate some indicating device. In the present case, the particular signals with which this device is intended to be used, are of such intensity that three tubes are necessary to supply the required actuating power to the telephone receivers.

Dr. Miller, on page 798 of the Printed Record, states that the two-electrode valve is incapable of such tandem use, but his statement is, of course, entirely contradicted by the evidence of my recent demonstration with the two-electrode tube, which showed that a tandem arrangement of two-electrode tubes amplified in a large and satisfactory manner.

In a statement near the top of P. R., p. 798, Dr. Miller refers to an amplifier which had received signals thousands of times too weak to be received on a detector alone. Under such circumstances, the device which he calls a "detector," would cease to be a detector, because it would be entirely incapable of carrying out the fundamental purpose of a detector, viz., the translation of the energy of the incoming signal into a form which would operate the indicator. If the signal is thousands of times too weak to be received on such a device, it is obviously incapable, under those circumstances, of operating the indicator.

The whole amplifier arrangement being necessary for this purpose, it therefore becomes the detector.

Q. 262. Please refer to Dr. Miller's answer to Q. 20 (P. R., p. 799), and state if there is anything set forth therein regarding the four uses of a three-electrode tube that does not apply to a two-electrode tube.

A. There is no use referred to in Dr. Miller's answer to Q. 20 of the three-electrode vacuum tube which is not also a use which can be made of the two-electrode tube. This statement, of course, results from the tests which I recently

made, and which showed that the two-electrode tube was capable of all the uses here referred to.

Q. 263. Referring to Dr. Miller's answer to Q. 21 of his deposition (P. R., p. 805), please state what bearing, from a technical standpoint, the Edison patent No. 307,031, dated October 21, 1884, has upon the disclosure of the principle of the two-electrode vacuum tube as an instrument for use in radio telegraphy, and also please refer to Mr. Waterman's statement quoted by Dr. Miller, that in the three-electrode device:

"The cold element is divided into two."

In your answer please refer to any publications or patents which indicate that the prior workers in the art so considered the three-electrode tube, indicating, if you will, any examples of instances where the anode was so divided. [fol. 1354] A. The Edison patent No. 307,031 does not, in my opinion, teach anything relative to the use of the two-electrode vacuum tube in a radio system, nor does it disclose any arrangement of this tube which could be usefully employed in a radio system.

Not only is the arrangement disclosed in the Edison patent useless for any radio purpose, but so far as I am aware, the arrangement disclosed therein has no useful purpose of any kind, and has not formed a basis for any commercially used device.

What this patent actually discloses is a rectifying arrangement operating a galvanometer as, for instance, as is shown in Fig. 1. A further purpose of this disclosure is to cause the galvanometer by means of an auxiliary contact shown in Fig. 3, to close the circuit of a regulating mechanism, in order to control the voltage regulation of a generator.

It is to be noted further that the rectifying device of the Edison patent is disclosed in connection with a circuit in which the alternating electromotive force is of low frequency; that is, of the frequency ordinarily employed for lighting purposes. There is, therefore, no indication from the disclosures of this patent relative to the usefulness of the device, or high or radio frequency.

The nature of the Edison invention and its purpose are quite clearly set forth in the specification, for instance (lines 30 to 34, inclusive, page 1), Mr. Edison, after de-

scribing his discovery of what has been termed the "Edison effect," says:

"My invention consists in the utilization of this discovery for indicating or regulating variations in electro-motive force, or for affecting electrical apparatus in any desired manner."

And in lines 8 to 13, page 1, he sets forth the object of the invention as follows:

"The object of my invention is to produce an efficient apparatus for indicating variations of electro-motive force in an electric circuit, preferably for use in connection with systems of electrical distribution to show the changes in pressure in the various parts of the district."

Dr. Miller, on page 805, after quoting Mr. Waterman's description of the grid of the three-electrode tube as follows: "a bit of wire bent back and forth to form a sort of grid," introduces two quotations from Dr. Van der Bijl's book, as follows:

"'In fact, the insertion of the grid into the valve resulted in a device of tremendous potentialities—one that can justly be placed in the same category with such fundamental devices as the steam engine, the dynamo, and the telephone.'

"And on page 42 Van der Bijl states:

"'It is hardly necessary to say that the insertion of the grid has made the audion a device of immense practical importance and enabled it to perform functions that would otherwise have been impossible.'"

While there is no question whatsoever relative to the great importance of the grid, an importance which is justly emphasized in these two quotations, nevertheless, the fact [fol. 1355] should not be lost sight of that the two-electrode Fleming valve is the foundation on which this important structure has been reared and, as has been demonstrated in my recent tests, possesses inherently all of the characteristic capabilities of the three-electrode tube.

In considering Dr. Miller's comment on Mr. Waterman's statement relative to the cold element of the Fleming valve being divided into two parts, it appears to me to be of inter-

est to set forth a few facts of an historical nature, showing how the development of the vacuum valve took place.

Dr. Fleming knew of the Edison effect soon after its discovery by Edison and, in a paper entitled "On Electric Discharges between Electrodes at Different Temperatures in Air and High Vacua," published in the Proceedings of the Royal Society of London, Vol. 47, page 118, of 1890, he describes many of his experiments with apparatus similar to that of the Edison patent No. 307,031.

Later, in the Proceedings of the Royal Society of London, Vol. 74, 1905, beginning at page 476, Dr. Fleming published a paper on the same subject, but involving radio frequencies; showing many other experiments which he conducted, but mainly with the vacuum tube of his patent here in suit. On page 478 (Claimant's Ex. 269) of this publication, which is the one to which I have previously referred, Dr. Fleming illustrates in Fig. 1, three forms of the vacuum tube, referred to as *a*, *b*, *c*, respectively, and on page 477, he refers to this figure 1 as follows:

"The diagrams in fig. 1 show various forms of the arrangement. Diagram *a* shows a bulb with a single carbon filament surrounded by a metal cylinder, *b* shows one with two carbon filaments, and *c* a carbon filament and two insulated metal plates."

The latter illustration, to-wit, *c*, is particularly significant and pertinent to the question at present under discussion, for its shows the anode of the tube split up into two separate plates.

Although Dr. Fleming has not referred to these separate plates being attached to different parts of a radio-frequency circuit, yet it is obvious that such an action would be perfectly practicable and, when the development of this device was taken up by DeForest, it is evident that he recognized this fact. In his patents No. 841,387 (Claimant's Ex. 270) and No. 879,532 ("Defendant's Exhibit K"), of January 15, 1907, and February 18, 1908, respectively, there are illustrated two plates, for instance, Fig. 2 of the first-named patent and Fig. 2 of the second-named patent, arranged in a manner substantially identical with that of *c*, Fig. 1, of Dr. Fleming's article.

DeForest, in these two patents, splits up the cold element into two parts, to one of which he attaches the input cir-

cuit carrying the radio-frequency, and to the other of which he attaches the indicator circuit.

The recognition of this fact is also to be found in the "Hand-Book of Wireless Telegraphy and Telephony" by Eccles, on page 251, where a paragraph appears, as follows:

"The Double Anode Valve

"This form of valve, sometimes called the grid-and-plate valve, consists of a vacuous bulb containing a glowing filament as cathode and two other electrodes. One of these may be regarded as the plate in the valve described above, [fol. 1356] the other is a grid or perforated plate, which is fixed between the filament and the plate. * * *"

A similar recognition of this fact is to be found in the work by Van der Bijl, entitled "Thermionic Vacuum Tube." On page 145, the following statement is found:

"* * * This third electrode can, however, be of any form, since a controlling effect on the discharge can be obtained by so positioning a conductor with respect to the path of the discharge that potential variations applied to it will cause variations in the current flowing between cathode and anode. The controlling electrode may, for example, be in the form of *a plate placed on the side of the cathode opposite to that of the anode* or in the form of a wire or a plurality of wires galvanically connected and placed in the plane of the cathode parallel to that of the anode. * * *" (Italics mine.)

There are further points indicating that the grid and the plate are simply a splitting up of the plate electrode which are indicated by properties common to both; both elements, when connected to the filament circuit, give rectifying action, both control the flow of electrons from the hot filament in accordance with the potential impressed upon them, and both are commonly employed with a constant potential, that is to say, the plate with a positive potential and the grid with a negative potential.

The quotation by Dr. Miller from Van der Bijl, as follows:

"It is hardly necessary to say that the insertion of the grid has made the audion a device of immense practical im-

portance and enabled it to perform functions that would otherwise have been impossible."

is most significant, indicating, as it does, that the three-electrode tube was simply a two-electrode tube in which a grid was inserted.

Dr. Miller's reference to the Eccles publication, entitled "The Three-Electrode Thermionic Tube and the Revolution in Wireless Telegraphy" is, to a large extent, correct, but the following sentence brings out some facts of importance:

"That it has been a veritable revolution can be seen by comparing the common practice in wireless telegraphy of 1914 with that of 1919."

It is to be noted that the three-electrode tube was fully known in 1914 and that the revolution referred to was not the result of discoveries made during that time, but of the tremendous application and use of the discoveries already made.

In the quotation given from Dr. Van der Bijl's book on page 145, is a reference to the fact that the third electrode is capable of exerting a controlling action when it is not between the filament and the plate, and I have seen and tested a practically operative tube which I understand was made either in Germany or Holland, in which this arrangement of two plates, one on either side of the filament, was employed.

In considering the inventions of both Fleming and DeForest, it is important to bear in mind that neither of these [fol. 1357] gentlemen, at the time of application for their patents, realized the full capabilities of their devices such, for instance, as their use for the production of oscillations. It was some six or eight years after the DeForest patents had been granted before the facts relative to this action were known.

While this discovery was made first in connection with the three-electrode tube, the two-electrode tube both as demonstrated before Judge Mayer and in my recent demonstration in this case, have inherently this same capability, so that Dr. Miller's enumeration of the differences between these two tubes should be converted to an enumeration of the likenesses between them.

It is thus apparent that DeForest's discovery of the advantage of inserting a third electrode directly in a two-electrode vacuum tube, important though it was, was not alone the cause of the "revolution," but the discovery and development of the full capabilities of this device, such as that of oscillating.

These discoveries, however, are equally applicable to the two-electrode tube, as I have already shown.

It seems to me that Dr. Miller's quotation from Dr. Eccles may convey an incorrect impression of what Dr. Eccles actually meant. It must therefore be appreciated that the period referred to by Dr. Eccles from 1914 to 1919, was the exact period of the World War, and that owing to the necessity of complete and accurate transmission and reception between sections of the forces of the Army and Navy, and also between the Naval and Land forces, great inpetus was given to the development of the vacuum tube both for transmitting and receiving purposes.

It has been estimated that had not this impetus been given to the development of the vacuum tube under conditions of the World War, its final development would probably have been delayed at least ten years.

All of these facts above referred to, clearly indicate that the fundamental invention was that of Fleming, when he discovered that the electron stream between a hot and a cold electrode could be controlled by radio-frequency oscillations.

I think, therefore, it is quite clear that DeForest in his inventions of the three-electrode tube, divided the anode or positive electrode of Fleming into two parts and used one for the incoming radio-frequency oscillations, and the other for the operation of the indicating mechanism.

* * * * * * *

Dr. Miller has somewhat at length disagreed with Mr. Waterman's statements to the effect that the P N circuit is usable with the Fleming valve. His entire contention rests on his designation of the P N circuit as a three-termi- [fol. 1358] nal circuit and from this premise he proceeds to argue that a two-terminal device is not usable in the three-terminal circuit. In this argument he fails, however, to bring out the fact that the so-called P N circuit did not originate with the three-electrode tube, but was simply borrowed by DeForest from the prior art.

In "Weagant Sketch No. 5" (Claim‌ant's Ex. 273) is shown in Fig. 1 the circuit arrangement of Marconi patent No. 627,650, in which an input circuit consisting of a coil j^2 and a condenser K′ is shown. These two elements correspond to the coil j^2 and the condenser j^3 of "Miller Drawing F," Fig. 2. The condenser h', which is the tuning condenser in this circuit, of course is not shown in the Marconi patent, because the tuning of this circuit had not been accomplished at the date of this patent.

This difference, however, does not change the circuit with respect to the point at present under consideration.

On the right-hand side of Fig. 1 "Weagant Sketch No. 5," is shown a battery B, an indicator R and two choke coils C^1 and C^2. This circuit corresponds to the right-hand circuit of Fig. 2 of "Miller Drawing F," with the exception that the latter does not require the choke coils. Both circuits are for the purpose of supplying battery power to the detector, and for the purpose of containing the indicator.

A comparison of these two sketches shows at once that DeForest simply took the connection which runs from condenser K′ to the detector T and transferred it to the grid. After this transfer has been made, the circuit may be said to have three terminals which are separate and distinct, but in its original form two of the terminals coincided on the detector T of Fig. 1 of "Weagant Sketch 5." These two terminals are the connection from the condenser K′ and choke coil C″, to the detector T. While the terminal conditions of the two circuits naturally change when this circuit is transferred from a two-terminal device to a three-terminal device, the circuit itself and its function, which is to conduct the radio-frequency energy to the detecting tube, remains unchanged.

In "Miller Drawing L" the two-electrode tube is shown in the P N circuit, and I have reproduced it as Fig. 3 of "Weagant Sketch 5," and it will be noted that it corresponds very closely with Fig. 1 of "Weagant Sketch No. 5," differing only in the fact that, instead of two choke coils in the battery circuit, there is only *one*, indicated by L, and these two arrangements are alternative, and no difference in the action results from the use of one coil as compared to two.

Fig. 3 of "Weagant Sketch 5" represents an arrangement of the two-electrode tube in the P N circuit which I

have frequently used, while "Weagant Sketch No. 6" (Claimant's Ex. 274) is the same circuit with the choke coil L omitted, due to the fact that the telephone receiver I provided a sufficient choking effect without the employment of an additional choke coil. This circuit I have actually used a great deal, and it will be noted that it corresponds exactly with the circuit of Fig. 2 of "Miller Drawing F," with the exception that both the right-hand circuit and the left-hand circuit terminate on one electrode, instead of on two.

Dr. Miller's statement, as follows (P. R., p. 810):

> "The circuits are distinct and separate because they ter- [fol. 1359] minate respectively in the grid and plate terminals which are points having different potentials both steady and varying."

appears to make a distinction between the use of the P N circuit with the three-electrode tube with the same circuit used with the two-electrode tube, based on the fact that these two points in the three-electrode tube are at different potentials, while in the two-electrode tube they are at the same potential.

While this is true, it is not the result of any difference in the circuits, but merely a difference in the terminal conditions.

In connection with Fig. 3 of "Miller Drawing L," Dr. Miller states (P. R., p. 810):

> "The way Figure 3 is drawn, with the low frequency branch to the right of the bulb and the high frequency branch to the left, makes it appear that the separation has been made into two circuits. Figure 3a dispels this illusion: this is the same identical circuit as Figure 3 and shows that the separation has been merely into two branches which are in parallel between the points *a* and *b*."

It is the function of a circuit drawing to make as clear as possible all of the facts which such a drawing is capable of presenting. In Fig. 3 the arrangement of circuits clearly indicates the facts of operation, viz.: that the circuit at the left is for the purpose of putting the high frequency energy into the tube, while the circuit to the right is for the purpose of supplying the battery power to the detector and for containing the indicating instrument.

Redrawing the circuit as Dr. Miller has in Fig. 3a, merely results in a type of drawing which is much less clear than it might be, and which, to any but a skilled technical man, would be confusing.

I may state further that, were a drawing of these circuits presented to me in this form by an assistant engineer I should immediately require him to re-draw these circuits after the fashion of Fig. 3.

Of course, when the P N circuit is drawn as in Fig. 3a, its appearance differs markedly from that of the P N circuit attached to the three-electrode tube, as shown in Fig. 4, but, since the purpose and functioning of the circuit is identical in both cases, that is to say, the left-hand side puts the energy on the incoming signal into the valve, while the right-hand side supplies the battery power and the indicating mechanism; the drawings representative of the employment of this circuit with the two types of tubes should, as nearly as possible, have the same identity of appearance as they have in fact.

Examination of the two drawings Fig. 3 and Fig. 4 of "Miller Drawing L" shows, very clearly, without the necessity of a lengthy explanation in words, the point brought out by Mr. Waterman, viz., that the three-electrode tube constituted the splitting up of the cold electrode of the Fleming valve into two parts corresponding with the two functions, viz., the input of signal energy and the indication of the presence of that energy.

Dr. Miller's reference to the World War and the revolution in radio which accompanied it, suggests an analogy between the cause and the ultimate development of the World War, and the work of Dr. Fleming, and the subsequent development of the vacuum valve.

[fol. 1360] A single shot from an assassin was followed by a conflict which involved the whole world, and consequences of colossal magnitude.

The discovery by Dr. Fleming that an electron stream between a heated cathode and a cold plate or anode could be controlled by an applied radio-frequency voltage, was a fundamental work which led to a development which, at the present day dominates the whole radio art.

It is usually very easy, after an invention has been made, to see how it should obviously have followed from the work of someone else, but it was some eighteen to twenty years after the discovery of the "Edison effect," during

which time Fleming and others conducted extensive research work on this "effect," before the invention of the Fleming valve was made.

It is also interesting to note that some time and work was required before DeForest, working with the device of Fleming, hit upon the idea of splitting the anode into two parts, so as to separate one terminal of the input circuit from one terminal of the output circuit.

With this change in structure and the subsequent discovery of the capabilities of the resulting device, as well as the original device in the matter of producing oscillations, the final step was taken which has resulted in a complete revolution in radio technique. So complete is this victory of the development of the Fleming invention, that today the vacuum tube may well be termed the "heart" of radio.

Mr. Vaill: Counsel for claimant offers in evidence the following as exhibits, and asks that they be marked as follows:

"Proceedings of the Royal Society of London," Vol. 47, pages 118 to 126, inclusive, being the Fleming article on "Electric Discharge between Electrodes at Different Temperatures in Air and in High Vacua," received December 16, 1889, published under date of January 9, 1890, as "Claimant's Exhibit No. 268."

"Proceedings of the Royal Society of London," Vol. 74, pages 476 to 487, inclusive (article received January 24, 1905, read on February 9, 1905, published in Pamphlet No. 505, March 16, 1905; Volume 74 being published in April, 1905), article entitled "On the Conversion of Electric Oscillations into Continuous Current by Means of a Vacuum Valve," referred to by the witness in connection with De Forest Patent No. 823,402 ("Plaintiff's Exhibit No. 258"), in connection with his answer to Q. 252, and in connection with his answer to Q. 263, as "Claimant's Exhibit No. 269."

Printed copy of patent No. 841,387, January 15, 1907, to DeForest, referred to in answer to Q. 263, as "Claimant's Exhibit No. 270."

Photostat copy of page 251 of A Hand-Book by W. H. Eccles, entitled "Wireless Telegraphy and Telephony" (December, 1915), as "Claimant's Exhibit No. 271."

Photostat copy of page 145 of the book by H. J. Van der Bijl, entitled "The Thermionic Vacuum Tube" (First

Edition, Second Impression—1920), as "Claimant's Exhibit No. 272."

"Weagant's Sketch No. 5," as "Claimant's Exhibit No. 273."

"Weagant's Sketch No. 6," as "Claimant's Exhibit No. 274."

(Said exhibits marked as requested.)

Q. 264. Please state to what extent you agree or disagree [fol. 1361] with Dr. Miller's answer to Q. 22 of his deposition (P. R., p. 811), concerning "Plaintiff's Exhibit No. 85".

A. Dr. Miller's answer to Q. 22 consists largely of an explanation of the action of the particular amplifying device which is the subject of "Plaintiff's Exhibit No. 85," and which explanation does not, so far as I can see, add anything to similar explanations previously given. He takes occasion again to criticise Mr. Waterman's statement: "Three detector bulbs T^1, T^2, T^3, together constitute the detector," and to again make his particular distinction between a detector and an amplifier.

As I have already pointed out, this particular distinction is one which has arisen in the development of the technical literature of the three-electrode tube, and is one which was not made in the earlier days of radio.

As I have also pointed out, the definition of the word "detector" given in the Report of the Standards Committee of the Institute of Radio Engineers, is much broader in its scope and under this definition, when it is necessary to employ three vacuum tubes such as T^1, T^2, T^3, in the quotation above, this entire combination is properly considered to constitute a detector.

Dr. Miller, of course, makes as much as possible of the amplifying action of the three-electrode tube, but while this action exists, and is of great consequence, nevertheless, it is a property which was possessed by the first of all radio detectors, viz., the coherer and, of course, as I have shown by conclusive demonstrations, by the two-electrode vacuum valve.

Dr. Miller repeats, several times, his statement to the effect that the Fleming valve is not an amplifier and cannot amplify; to which, of course, I must reply that the facts as shown by actual demonstration, are quite otherwise, and the two-electrode Fleming valve is so good an amplifier

that, were it not for the fact that the three-electrode one is better, this two-electrode amplifying Fleming valve would be found in extensive commercial use today.

Dr. Miller's criticism of Mr. Waterman's statement that (P. R., p. 813):

"It should be understood that as a matter of fact not all bulbs, whether they have one cold element as shown in defendant's patent, or two, as in the so-called audion, are able to work efficiently enough to produce an amplification. The bulbs which will amplify have to be selected by trial."

is to the effect that the three-electrode tube always amplifies. Perhaps a brief statement of some of the facts relative to the manufacture of three-electrode tubes may, at this point, make clear the basis of Mr. Waterman's statement.

When the three-electrode tube was first made and sold as a commercial device, for a considerable period of time, these tubes varied so much in their sensitiveness that it was largely a matter of luck if the purchaser got a good one. Almost any of them would detect, but a considerable percentage was quite worthless as either amplifiers or oscillators.

In the present-day commercial manufacture of three-electrode tubes, it is customary to put them through a careful test and inspection before they are marketed, so that [fol. 1362] the tubes which appear on the market are practically all good tubes. This result, however, is accomplished through the rejection of a considerable percentage of the tubes manufactured, and I happen to know from my own personal experience in this work, that the percentage of rejection is sometimes as high as sixty per cent.

In the light of these facts it is quite evident that an amplifying valve has to have something besides the third electrode in order to make it work, and that something is proper construction, together with suitable exhaustion.

The commercial development of suitable methods for exhaustion has consumed some years of intensive work, and it is in the effectiveness of these exhaustion methods that much of the improvement of the three-electrode tube as produced today, over the earlier devices of the same sort, is due.

I think I may fairly sum up this whole matter by stating that both the two-electrode valve and the three-electrode

valve must be properly constructed and exhausted, if they are to function satisfactorily in accordance with present-day technical standards.

Q. 265. Please refer to Dr. Miller's criticism, in his answer to Q. 23 (P. R., p. 814), of Mr. Waterman's testimony regarding the use of a battery with a vacuum tube detector, and state whether you agree or disagree with Dr. Miller's conclusions.

A. Dr. Miller's answer to Q. 23 disagrees with Mr. Waterman's statement to the effect that Paragraph 208 of the "Navy Manual" of 1915 refers to a lamp detector and a battery and, in fact, he denies the correctness of this statement. His denial appears to be not only a contradiction of Mr. Waterman's statement, but of the fact relative to the three-electrode tube which he himself has admitted. That the three-electrode dector may be properly termed a lamp detector, seems most obvious, since, when the filament is heated to incandescence, it gives out a light; sufficient light, in fact, when a number of them are employed, to render working with the apparatus possible at night, without other illumination.

In so far as this device constituting a detector is concerned, we have only to remember that Dr. Miller has given a complete explanation of the detecting action of the three-electrode tube. That a battery was used with this detector is obvious, and I therefore find that I am in agreement with Mr. Waterman's statement relative to the paragraph mentioned.

Dr. Miller further comments to the effect that Mr. Waterman's statement gives rise to the inference that when a detector is supplied with a battery, it becomes an amplifying device. In so far as this inference applies to the two-electrode Fleming valve, it is entirely correct, as I have shown by my tests, and I do not gather from Mr. Waterman's statement any extension of such inference beyond the two-electrode tube.

Mr. Waterman's further statement, "either the two or three-element bulb can be used to amplify when used with a local battery," quoted by Dr. Miller on Printed Record, page 814, is entirely in accordance with the results of my recent tests and demonstrations, and Dr. Miller is quite mistaken in his denial of this statement. He is also mistaken in the following sentence, "The real feature of the

three-element bulb which enables it to be used as an amplifier is the grid-control member."

[fol. 1363] My tests have shown conclusively that the *real feature* of the vacuum tube amplifier is the electron stream originating in the hot electrode, and flowing to the cold electrode, and the inherent capabilities which this electron stream has of being influenced by a properly associated electrostatic field in such a way that the energy in the variations of the electron stream is greater than the energy which must be associated with it for the purpose of producing these variations.

Dr. Miller further differentiates, on P. R., p. 814, between detection and amplification, but the sort of detection which I must assume he refers to is that contained in the definition which he quoted from Dr. Eccles in answer to Q. 8 on P. R., p. 770, which detection is a particular and limited kind of detection, and signifies the conversion of a radio frequency into some other and usually, though not always, audible frequency. This particular significance is useful in a technical analysis of the operation of vacuum tubes, but is not detection in the broad sense of the word, as understood in the earlier radio art, or as defined by the definition contained in the report of the Standards Committee for 1922, of the Institute of Radio Engineers.

Dr. Miller carries this particular thought through his further comment at the bottom of Printed Record, page 814, relative to detection and amplification, and adds the further characterization to detection by calling it a "distortion." This statement is true only in the limited technical sense which he has chosen to employ, but it is quite untrue in the broad sense.

As will be seen from the definition of the Standards Committee, referred to above, detection may be, and in fact actually is, accomplished through the control of a local source of energy. This was the case in the original detector, viz., the coherer, and is also the case with the two or three electrode valves.

While amplification may be differentiated from detection on the narrow technical basis applied by Dr. Miller, nevertheless, in the broader sense detection may involve amplification. The original coherer was a species of amplifier, and its detecting action was accompanied by an amplification.

The two and three-electrode valves, either singly, or in cascade, act as detectors and amplification accompanies this detecting action.

Dr. Miller, on Printed Record, page 815, refers to the characteristic curve of an amplifier, and states that it should be as nearly a straight line as possible, which is perfectly true, in so far as the particular sort of amplification to which he refers is concerned, and so far as an ideal device is concerned. It so happens, however, that the characteristic curve of the three-electrode tube, while fairly straight over part of its length, is curved in other portions.

The requirement mentioned above, that the device should have a straight line, is important only for that kind of amplification which involves a reproduction in the output circuit of the wave-form of the electromotive force applied to the input circuit. This is the sort of amplification necessary for faithfully reproducing telephone speech, but it is not the only kind of amplification. There is another kind, of which the coherer is an illustration, in which the output energy may be much greater than the input energy, but quite unrelated to it in form, and in this instance the input energy is in the form of an oscillating current of high fre- [fol. 1364] quency, while the output energy is in the form of a direct current.

Q. 266. Please refer to the last paragraph of Dr. Miller's answer to Q. 24 (P. R., p. 817), and state whether you agree with him that the device of the Fleming and the three-electrode vacuum tubes have "nothing in common," when used in transmitting sets, giving your reasons for your answer.

A. I must first, in replying to this question, point out that Dr. Miller's statement, in the first part of this paragraph, is self-contradictory, and that actually it is not a denial of the demonstration before Judge Mayer of oscillations produced by the two-electrode vacuum valve, but an admission that such oscillations were obtained, and a criticism of the method of action and the operating condition.

I am unable to see any real basis which permits Dr. Miller to make even the criticism which he has, for the simple reason that he was not present during this test, and the difficulties, etc., which he himself experienced, form no proper criterion of the demonstration before Judge Mayer.

It is also evident from the concluding part of Dr. Miller's statement in this paragraph, that he was quite unaware of

the fact that the two-electrode Fleming valve with a high vacuum, could oscillate in the manner demonstrated in my tests. Because of the facts disclosed in this test, it is quite obvious that the three-electrode tubes referred to by Dr. Miller, have very much in common with the device of the Fleming valve, and in fact are simply an improvement over this device.

* * * * * * *

Q. 267. Do you agree with the statements made by Dr. Miller in answer to Q. 25 (P. R., p. 817), and Q. 27 (P. R., p. 819), regarding the importance of the Fleming valve in the radio art? Also please refer to Mr. Loftin's testimony on the same subject, as found in answer to Q. 39 of his deposition (P. R., p. 1149), and particularly that part of the answer referring to the "literature of the world on the subject of radio," giving examples of such literature which disprove Mr. Loftin's statement in this respect.

A. Both Dr. Miller and Mr. Loftin, in replying to questions relative to the use of the Fleming valve, merely express the fact that their own personal knowledge of the use of this device is negligible. It happens, however, that others are familiar with such use and have given testimony in this case which shows that the Fleming valve has occupied a position of considerable importance in the radio art. I refer to Messrs. Alexander E. Reoch, Charles J. Weaver, John J. Brennan and Gordon B. Rabbits.

[fol. 1365] As is usual in his replies, Dr. Miller refers to the Fleming valve as a detector only, and incapable of amplifying, and I must again refer to my demonstrations, which have clearly proven the incorrectness of this statement.

He also refers to it as a detector of inferior characteristics as compared with the crystal and to this I shall reply that in its stability, freedom from the effect of shocks, either electrical or mechanical, and its certainty of operation, the Fleming valve, neglecting for the time being, its amplifying property, and considering only its rectifying action, was a detector of outstanding merit.

It would appear from Dr. Miller's statements that he had personally never tested or used a good specimen of the Fleming valve, while on the other hand, I have made and used many of these devices which, when used as simple rectifiers, were equal in sensitiveness to the most sensitive

of all crystals, viz., the galena and which required no adjustments of any kind.

I might state further, that at one period, somewhere about 1915, as I recollect it, I was planning to replace all the crystal detectors in the service of the Marconi Company of America with the simple Fleming valve. The only reason why this plan was not carried into effect was the rapid development of the three-electrode device, including that particular type developed by the witness, having the controlling electrode on the outside.

I have no hesitation in stating that had this plan been carried out, it would have resulted in substantial improvement in the ship service of the Marconi Wireless Telegraph Company of America.

Statements relative to the inferiority of the Fleming valve as compared to the crystals, and considering the Fleming valve from the standpoint of its rectifying action only, and not including its amplification property, are based on a lack of knowledge of the real capabilities of this device, which capabilities I of necessity investigated most extensively, since the Marconi Company owned the Fleming patent and a good detector was essential to its business.

I note in connection with the last part of your question, that Dr. Zenneck, who has been referred to many times by the defendant in this case as a radio authority, in his book entitled "Wireless Telegraphy" (Claimant's Ex. 275), gives a diagram, Figs. 340 and 341, of the Fleming valve and associated circuits, the latter circuit showing an auxiliary battery E, which is the same thing as the "B" battery frequently referred to in this case.

Above the figures is the following statement:

"* * * But in the more recent form of Fleming's 'oscillation-valve,' the anode being a cylinder of carbon and the cathode a tungsten wire, this detector seems to have met all reasonable requirements as to sensitiveness and reliability. This is borne out by the fact that Marconi has been using it, in conjunction with an Einthoven string galvanometer, in his transatlantic stations."

This statement of Dr. Zenneck's brings out the fact that he was aware of a most important use for the Fleming valve, viz., in the Marconi transatlantic stations.

Dr. Miller cites various references on Printed Record.

[fol. 1366] pages 821 and 822, tending to show through the publications referred to therein, the insensitiveness of, and general uselessness of, the Fleming valve.

As it happens that I am very familiar with the development of both the two and three-electrode valves to the point of commercial usefulness, I am able to point out certain facts in connection with references of this kind, which I believe to be illuminating.

Both the two-electrode tube and the three-electrode tube, as constructed at first, were a long, long way from displaying the capabilities inherent in them. When the amplifying and oscillating action of the three-electrode tube were discovered, a tremendous incentive was uncovered for further investigation and development of this device.

To this end the great facilities of such organizations as the General Electric Company, the American Telephone and Telegraph Company and, to a lesser extent, of the American Marconi Company, were devoted and, after the expenditure of probably millions of dollars, the three-electrode tube was brought to its condition of present-day usefulness.

The original Fleming valve, however, partly because its amplifying ability was not recognized by most people, and partly because, in any case, it is not so good as the three-electrode tube, did not receive the attention, so far as I am aware, of any organization, except the American Marconi Company.

Due to the patent situation the two-electrode device was of great importance to this organization, and extensive development work, under my direct personal supervision, was undertaken for the purpose of determining to what extent the original Fleming valve was capable of realizing the benefit of the work which had been done on the three-electrode valve.

So far as I am aware, no one else in the world undertook anything of this kind, and the comments of the various writers referred to by Dr. Miller and Mr. Loftin, apply to odd specimens of the device which happened to come into these writers' hands. Had the three-electrode tube not been subject to a great development, I have no doubt that radio literature would be full of the same sort of disparaging remarks, because I know from extensive personal experience, that this device, in its early form as manufactured by DeForest, was a most unsatisfactory thing to use.

My problem, in taking up the development of the two-electrode tube, was the same as confronted the workers who developed the three-electrode device, viz., to secure maximum sensitiveness and uniformity in the results.

Following in the footsteps of the workers who developed the three-electrode tube, particularly in the matter of securing a good order of vacuum, I succeeded in improving the results obtainable with the two-electrode valve over those obtainable with the same device in its early form, by an amount quite equal to the improvement attained in the work on the three-electrode tube.

As a result of this work, the oscillating and amplifying capabilities of the two-electrode tube appeared, and I was also able to produce a two-electrode device for use as a simple rectifier and detector of sensitiveness equal to the best crystal, and which could be made uniform in results, due to [fol. 1367] the fact that suitable methods of exhaustion were employed.

When, therefore, a comparison is made between the operation of the two-electrode device and the three-electrode device, this comparison is of significance only when it is made clear whether or not, in both cases, the devices have the benefit of modern development in the technique of manufacture.

Dr. Miller (P. R., p. 822) refers to a paper by Dr. Austin relative to the sensitiveness of the Fleming valve, and states that his result indicates a sensitiveness ratio in favor of the crystal detector of from 2 to 10 in favor of the crystal. He neglects, however, to make any reference to Dr. Austin's explanation of the larger ratio, which is as follows, pages 538 and 539 of the paper referred to:

"For a wave length of 900 meters the perikon appeared to be at least ten times as sensitive as the vacuum valve. As there is no reason to believe that the vacuum detector would change its sensitiveness with frequency, this difference is almost certainly due to insufficient coupling."

This statement brings out the fact that the larger ratio in favor of the crystal detector expressed nothing whatever relative to the sensitiveness of the vacuum tube itelf, but is a figure which included a deficiency in the receiving apparatus at the longer wave lengths.

I might further point out that a test of this kind carried out on a few specimens of the Fleming valve, and having back of it simply ordinary scientific curiosity, rather than any constructive purpose, is quite without significance in determining the fundamental capabilities of a device like the Fleming valve. Only extensive investigations—such as that conducted by an industrial organization—is capable of developing this sort of fundamental information.

As illustrative of the significance, or otherwise, of tests of the sort referred to by Dr. Miller and reported in Dr. Austin's paper ("Defendant's Exhibit F-1"), I may call attention to some tests on the three-electrode tube, reported by Dr. Austin in this same paper on pages 540, 541 and 542, in which a sensitive ratio to the electrolytic detector of 1.5 is given.

In spite of the tremendous capabilities of the three-electrode device in the matter of amplification, oscillation and regenerative action, this particular test disclosed nothing which would warrant the use of the three-electrode tube.

I happen to know, furthermore, from my personal contact with Naval radiomen during this period, that on the basis of this test, the fact that the increase in sensitiveness over ordinary detectors was so small and the cost of the auxiliary apparatus, such as batteries, was considered so great, that the Navy declined to employ it in its own service until some two or three years later, when the remarkable capabilities of this device were discovered as the result of some *real* work done elsewhere.

In spite of Dr. Miller's inability to find any literature containing a favorable reference to the Fleming valve or any recognition on the part of technical writers of its [fol. 1368] usefulness, I find in the work entitled "Text Book on Wireless Telegraphy," Vol. 2, Valves and Valve Apparatus, by Rupert Stanley, Chapter 3, page 36, the following statement:

"Fleming Valve.—The first valve detector was patented by Dr. J. A. Fleming in 1904; it had no grid, and simply made use of the unilateral conductivity effect between a heated filament and a plate enclosed together in a vacuum. At that time it was considered so reliable and robust compared to crystal detectors that it was largely adopted on wireless installations by the Marconi Company."

And also on pages 37 and 38, two additional paragraphs:

"Fleming was the first to use the unilateral conductivity effects of an electron discharge, from a heated filament to a plate at a higher potential than the filament, so that his patent may be considered as the parent one of all valves used in wireless receiver circuits."

* * * * * * *

" 'AUDION' VALVE.—Dr. DeForest introduced a grid consisting of a perforated cylinder, or plate, between the filament and anode of what was virtually a Fleming valve, thus producing a relaying valve which he called an 'Audion.' In DeForest's circuit, which is shown in Fig. 13, the valve is used as a detector-relay; the small pulses of potential set up in the receiver circuit act on the grid and cause corresponding pulses of plate circuit current; these are also rectified by employing the valve at the proper point on its characteristic curve."

The first statement quoted indicates that because of the reliability of the Fleming valve, it was largely employed by the Marconi Company, which company had a greater number of radio installations than any other company that I am aware of.

The second paragraph quoted brings out the point to which I have repeatedly referred, viz., that Fleming was the first to disclose the utility of an electron stream or discharge, when influenced by radio oscillations.

The third paragraph brings out the author's point of view relative to the three-electrode valve being the result of the insertion in the two-electrode valve of the third electrode or grid.

I note also that he refers to the three-electrode valve as the "detector-relay," which expression is similar to some of my own statements, indicating that a detector may operate by the control of local energy, as well as by the mere conversion of the form of incoming energy into a form suitable for operating an indicating mechanism.

Mr. Vaill: Counsel for claimant offers in evidence photostat copies of pages 283 and 284 of book entitled "Wireless Telegraphy" by Zenneck, translation by Seelig, 1915, as "Claimant's Exhibit No. 275."

Also the title page and pages 36, 37 and 38 of the "Text-

Book on Wireless Telegraphy", Vol. II, by Rupert Stanley of 1919, as "Claimant's Exhibit No. 276".

(Said exhibits marked as requested.)

[fol. 1369] Q. 268. Referring to Dr. Miller's answer to Q. 28 (P. R., p. 823), please state to what extent you may disagree with Dr. Miller's statements regarding the art prior to the invention of the Fleming valve?

A. Dr. Miller, in reply to Q. 28, sets forth at length various scientific facts known to scientists prior to 1904, but he does not refer to any publication showing that anyone, prior to Fleming, made any use of the so-called "Edison bulb" as a rectifier for high-frequency oscillations.

His statement to the effect that any scientist could have predicted prior to 1904 that the Edison device would operate just the same at radio-frequencies as at audio-frequencies, omits entirely to bring out any evidence that any of these learned gentlemen did make such a prediction, in spite of the fact that at the time of the Fleming patent, radio telegraphy was some eight years old.

Fleming himself was familiar with the Edison device nearly eighteen or twenty years before he made his invention.

As is practically always the case, and as I have learned from a long experience with inventions, the mere existence of the fundamental scientific information necessary for the making of an invention, is without signficance in itself. To someone there must occur the idea which results in the utilization of such fundamental information for a useful purpose.

As an illustration of the fallacy of arguing anything in particular from the sort of information referred to by Dr. Miller, I will undertake to show, by the same method of reasoning, that the three-electrode tube was a perfectly obvious device to anyone skilled in the art after Fleming's disclosure. I have several times referred to the fact that an electrostatic control of an electron stream by a third element was old and well known in the art, and I produce herewith pages 237 and 238 (Claimant's Ex. 277) of Dr. Fleming's book, "The Principles of Electric Wave Telegraphy," published 1906, in which, in Figs. 31 and 32, there is shown a device known as the Braun tube.

Reference to Fig. 32 shows that this device, when acted upon by a high potential, produces a cathode ray, which is

another name for an electron stream, which travels from one end of the device, viz., the left hand end, to a screen at the opposite end.

Two exterior electrodes P, P, connected to an oscillating circuit, L, K, S, have impressed upon them the alternating potentials due to the oscillating circuit and the electrostatic field existing on these plates deflects the electron stream and enables the device to be used for delineating condenser discharge curve.

We have here in so far as fundamental scientific information is concerned, the elements of the three-electrode tube, viz., an electron stream and an electrostatic control thereof.

Following the particular method of reasoning involved in Dr. Miller's reply to Q. 28, would lead me to the conclusion that Dr. Fleming, having this particular information, would have immediately applied to his vacuum tube a third electrode capable of producing the electrostatic control.

That this particular method of reasoning has little relation to the actual business of making an invention is of course, shown by the fact that Dr. Fleming did not do anything of the kind but, on the contrary, it required another individual, viz., Dr. DeForest, to make this invention and [fol. 1370] carry out in a practical device, the fundamental electrical principles clearly shown in the Braun tube with electrostatic control.

Dr. Miller, in further statements in answer to Q. 28, refers to patent No. 328,687 to Valbreuze ("Defendant's Exhibit A-2"), as an illustration of the use of a rectifier in a radio circuit, but it would be more nearly correct to state that the Valbreuze patent discloses merely a paper-and-ink radio system.

I have constructed the device shown in this patent which, of course, does not involve any hot electrode and it is utterly useless for the purpose described. Dr. Miller's statement that the device is even more insensitive than the Fleming valve is hardly a correct expression of the facts, for the Fleming valve, properly constructed, is an extremely sensitive detector, while the Valbreuze arrangement is so insensitive that it is useless practically, and I may also add, has never had any practical use.

Mr. Vaill: Counsel for claimant offers in evidence photostatic copies of pages 237 and 238 of the book by Dr. J. A. Fleming, entitled "The Principles of Electric Wave Teleg-

raphy" of 1906 and requests that the same be marked "Claimant's Exhibit No. 277."

(Said exhibit marked as requested.)

Q. 269. Do you agree with Dr. Miller's conclusion, in his answer to Q. 29 (P. R., p. 828), that:

"* * * there is no connection whatsoever between the detecting function and the functions of amplifying or oscillating."?

A. Dr. Miller's statement, as I understand it, is directed to the three-electrode device, and I am quite unable to agree with it, because a little detailed consideration of the mechanism of the three-electrode tube shows that the same mechanism, viz., its rectifying action, is an important essential of the amplifying action and the oscillating action.

As Dr. Miller himself has pointed out, in his explanation of the action of the three-electrode tube, when an electromotive force is applied to the grid, the positive half of the cycle results in withdrawing more electrons from the hot cathode and thereby increasing the plate current, while the negative half cycle has an opposite effect, and results in the reduction of the plate current. This very action is rectification, because the rectifying action between either of the cold electrodes and the hot filament is the direct result of the fact that the electrons in the filament can only be drawn out by a potential in one direction and therefore, current can only flow in one direction.

If we now examine the action taking place during oscillation, we find that, as explained by Dr. Miller, the current variations in the plate circuit support the current variations in the grid circuit, a fact which is only possible as the result of the rectifying or detecting action of the tube.

A brief inspection of the mechanism shows why this is so. When a positive potential is applied to the grid the plate current increases, and when a negative potential is applied to the grid, the plate current decreases, due to the rectifying action of the grid, as explained above.

Let us, however, assume for a moment, that the grid [fol. 1371] works differently, viz., that either a positive or a negative impulse causes an increase in the current in the plate circuit; then it would follow that for one complete cycle on the grid, one-half being positive, and the other half

negative in the plate circuit, there would be two positive half cycles, which is analyzable into a direct-current component and an alternating-current component of twice the frequency of the electromotive force applied to the grid. This means that the output frequency of the tube under these conditions, would be double the input frequency and could not, therefore, support this input frequency and produce sustained oscillations.

Similarly, the device could not function as an amplifier which operated without distortion but, on the contrary, if it amplified at all, would change the frequency of everything put into it.

From the above simple analysis it is evident that the three-electrode tube, in acting as either an amplifier or oscillator, depends upon the fundamental rectifying action between the cold electrodes and the hot electrode.

Another and less technical way of stating this fact, is to simply point out that the three-electrode tube contains two rectifying circuits, that regardless of what particular manner in which the tube is functioning, it is employing those rectifying circuits and, furthermore, if it did not contain these rectifying circuits, it would be some other kind of a device.

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There are one or two additional points which illustrate the part played by the rectifying property of the three-electrode tube when it is used as an oscillator or an amplifier.

Dr. Miller has laid considerable stress on the fact that one of the outstanding features of the three-electrode tube is its ability to amplify without distortion, a property of immense importance in wire telephony and in the reception of radio telephony. It is customary in the operation of the three-electrode tube to impress upon the grid a negative voltage, commonly referred to as a biasing voltage, of such magnitude that when the grid circuit is operated upon by an incoming signal the positive half-cycle will not exceed the negative bias applied to the grid. This is necessary in order to reduce distortion to a minimum, for even though the characteristics of the tube may be a straight line, yet as soon as the grid goes positive, it begins to draw current and, as a result, introduces losses into the input circuit. It may therefore happen that, during

the rise of the positive half-cycle of grid electromotive force [fol. 1372] from zero to a maximum, the first half of it may operate on a grid which consumes no energy, while the second half of it, due to current flowing, may operate on a grid which consumes energy.

Such an action will at once resort in distortion and is, therefore, avoided in practice. It becomes very important, then, both from the standpoint of distortionless amplification and efficiency of amplification, to keep the grid negative, so that it may consume no energy, *but* this is possible only because the grid circuit is a rectifier, and when a negative voltage is applied, no current can flow through it so long as the positive half-cycle of the incoming voltage does not exceed this negative voltage.

It is thus seen that the rectifying characteristic on which the tube depends for detection, is also vital in obtaining good amplification.

Similarly when the tube is used as an oscillator, a negative bias is applied either with a separate battery or by the use of a grid condenser with a leak. The latter device functions by accumulating a negative charge from the tube's own oscillations and by adjustment at the rate at which this charge leaks off relative to the rate at which it accumulates, any desired negative voltage may be impressed on the grid.

It is thus seen that the rectifying or detecting function of the three-electrode tube has an additional important bearing on the performance of the three-electrode tube as an oscillator.

Q. 270. Have you read, and do you understand, the technical descriptions in the Fleming patent No. 803,684, of November 7, 1905, in suit, "Plaintiff's Exhibit No. 22", particularly as regards claims 1 and 37 thereof?

A. I have read the patent, and believe that I understand it, and the significance of the claims 1 and 37.

Q. 271. Have you read the deposition of Mr. Frank N. Waterman, given during the opening or prima facie proofs in this case, and if so, do you understand the same, and do you agree with Mr. Waterman's position regarding the nature of the invention of the said Fleming patent in suit, and the similarity of the devices and circuits used by the defendant on which infringement is claimed, with the devices and circuits of said Fleming patent?

A. I have read Mr. Waterman's deposition, and believe that I understand it, and I am in agreement with Mr. Waterman's position regarding the nature of the invention of the Fleming patent in suit, and I agree with Mr. Waterman in his opinion relative to the similarity of the devices and circuits used by the defendant, on which infringement is claimed, with the devices and circuits of the Fleming patent.

Q. 272. Will you please state briefly your understanding of the nature of the invention of Fleming patent No. 803,684, referring also to claims 1 and 37 thereof, stating from the technical standpoint whether or not the descriptions of the elements in said claims are confined to the exact devices described and illustrated in the specification and drawings of said patent?

A. Fleming patent No. 803,684 discloses an arrangement consisting of a cold electrode and a heated electrode enclosed within an evacuated glass vessel. It also discloses connections between the cold electrode and the heated electrode an indicating device in this circuit and a coil for associating this circuit with a wireless telegraph antenna; a battery for heating the hot electrode is also shown and described.

[fol. 1373] In the specification, Dr. Fleming points out that this arrangement is particularly adapted to the rectification of high-frequency currents, which are necessary in a wireless communicating system and that, in this respect, it differs from various well-known forms of rectifier.

In referring to the construction of the device, the patent is not limited to a particular arrangement or construction of electrodes, but states a number of alternatives as, for instance, lines 73 to 86, inclusive, page 2:

"In place of using a metal cylinder surrounding a carbon loop filament I sometimes use a number of carbon filaments. Some of these are heated by means of an electric current and become the hot conductor of the oscillation-valve and the others remain cold and form the cold conductor, or the metal cylinder may be replaced by a cylinder of meerschaum or the like having wound helically upon it a narrow ribbon of metallic foil. In those cases in which a larger alternating current has to be dealt with the hot conductor may be a rod of soft graphitic carbon held in suitable supports."

Dr. Fleming also directs particularly the exhaustion of the valve to a high degree and in line 96, page 1 to line 5, page 2, gives some instructions as follows:

"As a very high vacuum should be obtained in the bulb *a* and as a considerable quantity of air is occluded in the conductors, these should be heated when the bulb is being exhausted. The filament *b* can be conveniently heated by passing a current through it, while the cylinder *c* can be heated by surrounding the bulb *a* with a resistance-coil through which a current is passed, the whole being inclosed in a box lined with asbestos or the like. When, as hereinafter described, the cylinder *c* is replaced by any form of conductor which can be heated by passing a current through it, this method is usually more convenient than that just described."

This instruction is quite clear and indicates that Dr. Fleming intended that his valve should have a vacuum at least as high as was commercially obtainable at that date, and that the electron flow from the hot electrode to the cold electrode might be impeded as little as possible by the residual gases.

As I understand the Fleming patent, it discloses, for the first time, the use of an evacuated vessel containing a hot electrode and a cold electrode in a radio telegraph circuit.

This, of course, is a broad and fundamental disclosure, and one which has resulted in a tremendous development in the radio art and to my mind, obviously is not confined to particular details or methods of use.

Consideration of claims 1 and 37 shows, I think, quite clearly, the breadth of the invention. Claim 1, which reads as follows:

"The combination of a vacuous vessel, two conductors adjacent to but not touching each other in the vessel, means for heating one of the conductors and a circuit outside the vessel connecting the two conductors."

describes any arrangement employing a vacuum valve in radio, with which I am familiar, and claim 37, which reads as follows:

[fol. 1374] "At a receiving-station in a system of wireless telegraphy employing electrical oscillations of high

frequency a detector comprising a vacuous vessel, two conductors adjacent to but not touching each other in the vessel, means for heating one of the conductors, a circuit outside of the vessel connecting the two conductors, means for detecting a continuous current in the circuit, and means for impressing upon the circuit the received oscillations."

describes some millions of radio receiving-stations at present in use.

Q. 273. Referring to the demonstrations which you made on December 28, 1926, in the presence of counsel for defendant, showing that the two-electrode valve with a high vacuum would oscillate and amplify, will you please state, in accordance with your understanding of the matter from a technical standpoint ,whether or not the vacuum tubes used in said demonstrations, which comprised circular plates as anodes, separated lengthwise of the tubes certain distances from the respective filaments, were Fleming valves within the terms of the Fleming patent No. 803,684 in suit?

A. Yes, they certainly were, since, while not the literal construction shown in the diagram of said patent, yet they were unquestionably of a construction within the intent of the patent in the matter of variations in structure, as indicated in the quotation, page 2, lines 72 to 86.

I might also point out that the enclosed electrodes of these valves consisted of one hot electrode and one cold electrode.

Q. 274. Referring to Dr. Miller's answer to Q. 30 of his deposition (P. R., p. 828), will you please state whether or not the three-electrode vacuum tube referred to by Dr. Miller as "V T-1" ("Defendant's Exhibit F"), is within the technical description of the invention of the Fleming patent in suit, as stated in the specification of said patent, and in claims 1 and 37 thereof; also when said vacuum tube is used in radio receiving circuits, such as illustrated by the diagram of the "P N" circuit shown in "Plaintiff's Exhibit No. 83." Please give your reasons for your answer and state any personal experience you have had with the use of the V T-1 three-electrode vacuum tube?

A. The vacuum tube referred to by Dr. Miller is the "V T-1" happens to be a vacuum tube of the three-electrode type which I have used for many years, and I have no hesitation in saying that this vacuum tube employed in radio circuits is, in my opinion, completely described by the claims

of the Fleming patent, and its structure and use are of a nature disclosed by this patent.

You have referred in your question to the "P N" circuit, which is a circuit with which I am familiar, and is useful in this answer in showing how the claims of Fleming patent No. 803,684 read on this device. Considering claim 1, the first requirement is a vacuous vessel, which is met by the exhausted glass container of the "V T-1", while the second requirement is two conductors adjacent to, but not touching each other in the vessel; since the "V T-1" tube contains three conductors adjacent to, but not touching each other; it contains, therefore, the two required by this element of the claim.

Means for heating one of the conductors is the next element and this is shown by the battery B′ in Plaintiff's Exhibit No. 83," which illustrates the P N circuit employed with a three-electrode tube.

The final requirement in this claim is a circuit outside [fol. 1375] the vessel connecting the two conductors. In "Plaintiff's Exhibit No. 83" this requirement is met by the circuit from the plate P, through the battery B^2, the indicating mechanism R, to the filament.

It is also met by the circuit from the grid, through condenser j^3, inductance j^2, and the connecting wires to the filament F.

Considering claim 37 and the same diagram of "Plaintiff's Exhibit No. 83," the first requirement is a detector at a receiving station in a system of wireless telegraphy employing electrical oscillations of high frequency, and this is disclosed by the three-electrode tube P G F and all of the associated circuits of "Plaintiff's Exhibit No. 83." The claim also requires that the detector should comprise a vacuous vessel, which is the exhausted bulb T of "Plaintiff's Exhibit No. 83"; two conductors adjacent to but not touching each other in the vessel, which is met by the two conductors F G or F P of the three-electrode tube. Means for heating one of the conductors is the battery B′; the circuit outside the vessel connecting the two conductors, which is the circuit B B^2, the indicator R and the wires to the battery, for the circuit j^3, j^2 and the connecting wires from the grid to the battery B′. Means for detecting a continuous current in the circuit is the indicator R, and means for impressing upon the circuit the received oscillations is

supplied by the coil j^2 coupled to the coil j', which is in the antenna circuit.

This description and comparison is, of course, based upon the use of the three-electrode tube such, for instance, as the V T-1, in a radio communications system employing high frequency oscillations.

It happens that in my personal experience I have used the V T-1 tube in radio circuits of many sorts, including that known as the P N circuit, and have seen it employed in Government stations in a similar manner.

In Dr. Miller's reply to Q. 30 (P. R., p. 828), he makes a reference to the use of this three-electrode tube, that is, the V T-1, for the purpose of ground telegraphy, or as it was designated, T P S, and I might point out that while some of these V T-1 vacuum tubes undoubtedly were used for this purpose, from my knowledge of the methods of the Government in the purchase of vacuum tubes of this sort, I can say that it was not customary to specify the use which was to be made of the individual tubes; but, on the contrary, it was customary to buy the tubes under a general specification and to use them when received, for radio or any other purpose.

* * * * * * *

Q. 275. Referring to the last paragraph of your preceding answer, where you mention the purchase of vacuum [fol. 1376] tubes by the Government under "a general specification," I hand you a typewritten document consisting of four sheets and ask you what you recognize this to be.

A. I recognize this and the title indicates that it is "General Specification No. 2087 (October 24, 1918), Tubes Type V T-1." This is a specification issued by the United States Government for vacuum tubes to be used either in radio or T P S. It gives various details relative to the construction, performance and shipping of these tubes.

It will be noted that in paragraph 1-(a) it contains the following statement:

"These tubes will be called upon to perform three functions. They must operate as detectors of radio signals, as amplifiers for audio and radio frequency signals, and as oscillators in circuits used for heterodyne reception of radio signals."

On page 4 of this same specification is a paragraph which shows that these V T-1 vacuum tubes must pass an oscillator test, said paragraph is as follows:

"(f) *Oscillator Test.* The standard Signal Corps oscillator set, diagram of which is shown in drawing RL-A-176, shall be used for this test. All blue label tubes shall oscillate in this circuit over the filament voltage range of three and three-tenths (3.3) to three and nine-tenths (3.9) volts with the plate at plus seventeen (17) volts. No vacuum tubes shall be accepted as yellow label tubes that do not pass the oscillator test for blue label tubes."

Mr. Vaill: Counsel for claimant offers in evidence a photostat copy of U. S. Government "General Specification No. 2087" referred to by the witness in the preceding answer, and requests that the same be marked "Claimant's Exhibit No. 278."

(Said exhibit marked as requested.)

Q. 276. Referring again to Dr. Miller's answer to Q. 13 (P. R., p. 784), and the last paragraph of his answer to Q. 24 as found on page 817 of the Printed Record, will you please state from your knowledge of the demonstrations before Judge Mayer in the proceedings on the Supplemental Bill in the case of Marconi Wireless Telegraph Company of America *vs.* De Forest Radio Telephone and Telegraph Company, the length of time that the oscillations continued during such demonstrations and whether the action was such that it could not be reproduced if desired, and under similar conditions?

A. My recollection of the tests before Judge Mayer is that something like one-half hour was consumed in making these tests and that there was no difficulty experienced in getting the tube to oscillate. It is also my opinion, based on work done prior to the demonstrations, that the result of these tests were reproduceable.

Q. 277. In your answer to Q. 259, in which you had under consideration Dr. Miller's drawing F, you referred to the leakage away from the grid element of the three-electrode tube. Will you please explain a little more fully under what conditions an external leak is made necessary?

A. This external leak is made necessary whenever there is a grid condenser, such as f^3 of Fig. 2 or Fig. 3, "Miller Drawing F." In order that this condenser may not acquire a permanent negative charge.

[fol. 1377] I might point out in this connection that while it is necessary to employ the grid condenser and grid leak for detection purposes, that a hard three-electrode tube will detect without the use of these devices. When the three-electrode tube is used as a radio-frequency amplifier or as an audio-frequency amplifier, it is not customary or necessary to employ the grid condenser and leak.

Q. 278. Do you agree with Dr. Miller's answer to X Q. 78 (P. R., p. 837), regarding the variations in the electron flow in two-electrode and three-electrode tubes?

A. Dr. Miller, in replying to X Q. 78, states that in the three-electrode tube, potential variations are not impressed upon the electron stream, while they are in the two-electrode tube. His basis for so answering this question is found in his answer to X Q. 79, and rests on the fact that the electron flow does not take place between the cathode and the grid, and hence, potential variations are not impressed upon the electron flow.

There is no justification whatever for this particular presentation of the facts, except for the purposes of the answer to X Q. 78. While it is perfectly true that there is no current flow ordinarily between the grid and the hot cathode, nevertheless the grid is immersed in the electron stream which flows between the cathode and the cold electrode and whatever potential exists upon the grid, influences the space around it. Since the space around the grid contains the electron stream, the potential impressed upon the grid is, therefore, acting upon the electron stream.

In answer to X Q. 79, Dr. Miller states:

"The variations in flow in the three-electrode tube are caused by potentials which are impressed upon the grid of the tube."

This statement, in my opinion, is misleading in the sense that, having stated that the grid potentials were not impressed upon the electron stream, it could be argued from this statement that the important thing was to vary the potentials of the grid.

The actual facts, however, are that the variations of potential upon the grid, in themselves are of no consequence, but are of importance only as they affect the electron stream. The grid itself, while the most effective means for bringing about this control, is not the only way, and my recent tests

have shown that this control is obtainable when the third electrode is dispensed with altogether.

In answer to X Q. 79, Dr. Miller states:

"The action of impressing the potential variations upon the current flow is one which requires power."

Which statement, in my opinion, is misleading, since it is true only of the particular narrow interpretation which Dr. Miller chooses to use, of the significance of the words "impressing the potential variations upon" and which is limited to that particular case in which the electron stream flows from the hot electrode to the cold electrode upon which the potential is impressed. This happens to be the case in the three-electrode tube of the circuit between the plate and the hot cathode.

Q. 279. Will you please comment upon Dr. Miller's answers to X Qs. 82 and 83 (P. R., p. 838), as you have done in connection with X Qs. 78 and 79 (P. R., pp. 837 and 838)?

[fol. 1378] A. Dr. Miller, in replying to X Q. 82, while admitting that the electron flow from cathode to anode is increased when the grid is positively charged, qualifies his answer by stating that the positive charge on the grid does not directly produce the increased current flow. By this I understand him to mean that the increased current flow does not take place through the grid itself, which action would be undesirable if it did take place. He further states:

"It is merely the controlling effect of this charge upon the current flow between the anode and cathode that permits a greater current flow between these two electrodes."

Since the current flow between the cathode and the anode is the same thing as the electron stream, this quotation is a direct statement to the effect that the potential impressed upon the grid controls the electron stream, which is a complete contradiction of his answer to X Q. 78, as I understand it.

In his reply to X Q. 83, Dr. Miller denies that the difference between the influence of the control of the grid in a three-electrode tube and of the anode in the two-electrode tube is that one is indirect, and the other direct. His denial seems to be based upon his dislike of the use of the word control with both electrodes, and he reserves it specifically for the action of the grid of the three-electrode tube.

This reservation, however, in my opinion, is justified only in a narrow technical consideration of the three-electrode tube by itself, but in the broader consideration of the vacuum tubes of different types, it ceases to apply. In my opinion, the two-electrode tube used merely as a rectifier, and without consideration of the outside electrostatic field effect, involves control of the electron stream through the potential variations impressed upon the plate by the incoming oscillations.

Of course, as usual, Dr. Miller differentiates between the two-electrode and three-electrode devices on the ground that the latter is an amplifier, while the former is not, and with this distinction I am forced to disagree, since I have proven by actual tests that the two-electrode tube is an amplifier.

Dr. Miller concludes his answer to X Q. 83 with the following statement:

"This is not an amplifier and the variations in current would not be called control variations."

There is nothing about the meaning of the word "control," so far as I can see, which in any way limits its significance to amplification. On the contrary, this control may, or may not, be accompanied by amplification, and still be the cause of results indicated or made use of.

Q. 280. Referring now to Dr. Robert A. Milliken's deposition on behalf of the defendant, will you please consider his answer to Q. 12 (P. R., p. 845), and state wherein you may agree or disagree with the statements of said answer?

A. Dr. Milliken, in replying to Q. 12, first explains the action of the two-electrode valve when used as a rectifier and then explains the action of the three-electrode tube as a relay type of amplifier. He differentiates between the two devices in the following statement (P. R., p. 846):

"The essential difference between the three-electrode and the two-electrode tubes, for the communications art, is then [fol. 1379] that the well-exhausted two-electrode tube is simply a rectifier, and by virtue of that property can be used as a detector of signals, but it is not and can not be an amplifier, whereas the three-electrode tube was developed primarily for telephone amplification for the very reason that the two-electrode tube could not be so used, and the primary use of the three-electrode tube throughout the war was for the purpose of amplification and is that to-day."

In this statement there is contained a repetition of the same remarks so frequently made by Dr. Miller, in his deposition, to the effect that the two-electrode tube is not, and cannot be, an amplifier, a statement which, however, is completely at variance with the facts developed by my tests.

The concluding paragraph of Dr. Milliken's answer to Q. 12 is as follows (P. R., p. 847):

"The essential difference, then, in a word, between the two-electrode tube and the three-electrode tube, so far as the communications art is concerned, is that they were developed for entirely different purposes and actually fulfill completely different functions, the first being simply a rectifier and detector and incapable of performing either in a scientific or commercial way the function of amplification, while the second was developed for and now serves magnificently the purposes of distortionless amplification."

This statement is quite at variance with the facts as I know them relative to the two-electrode tube and the three-electrode tube. In the first place that the two-electrode tube is an extremely good amplifier I have already demonstrated, and in the second place, I am in a position to deny Dr. Milliken's statement to the effect that the two-electrode tube was developed solely for a purpose different from that of the three-electrode tube, for the reason that extensive work was done under my direction in the development of the two-electrode tube which showed, among other things, that this tube was a good oscillator and amplifier.

Q. 281. Please refer to Dr. Milliken's answer to Q. 20 (P. R., p. 849); also his answer to Q. 23 (P. R., 850), and comment upon his statements relative to what he terms "negative resistance".

A. Dr. Milliken, in his reply to Q. 20, has gone at some length into the particular sort of negative resistance which is possessed by the arc or the two-electrode Fleming valve, having a certain amount of gas present, but seems to have quite entirely overlooked some of the other types of negative resistance, which I have explained in my answer to Q. 250.

In his answer to Q. 23, he differentiates particularly between the gas-filled two-electrode tube of the Cooper-Hewitt type and the three-electrode tube, and denies that the three-electrode tube has a "negative resistance." In his denial

Dr. Milliken seems to have overlooked the above facts relative to the three-electrode tube, since, in my answer to Q. 250, several kinds of "negative resistances" are shown to be possessed by this device.

I might also add that Dr. Milliken, in his reply to Q. 23, holds an opinion relative to the three-electrode tube which [fol. 1380] is not only contrary to that of the witness, but is contrary to the point of view of such eminent and experienced experts in this art as M. I. Pupin and E. H. Armstrong.

Q. 282. Please consider Dr. Milliken's answers to X Q. 27-X Q. 29 of his deposition (P. R., p. 851), and in view of your experience with the manufacture of Fleming valves, will you please state whether you consider his answers correct.

A. My own experience with vacuum valves exhausted by ordinary methods indicates that the vacuum mentioned by Dr. Milliken in answer to X Q. 29, viz., 10^{-3} millimeters of mercury, is approximately correct. He states, in answer to X Q. 27, that the vacuum commercially obtainable at the date of the Fleming patent was 10^{-6} or 10^{-7} millimeters of mercury, and explains in answer to X Q. 28, that he bases his opinion in answer to X Q. 27 on the clean-up effect mentioned by Dr. Langmuir. He overlooks, however, the fact that Dr. Langmuir's statement is made with respect to an incandescent illuminating lamp, and that this lamp has no large metal surface in it which releases a considerable amount of gas. As the vacuum tube gets older this plate releases considerable quantities of gas, and this is an action which runs counter to the clean-up action of the filament.

I might also mention that I have made a great many Fleming valves exhausted to about the vacuum referred to, viz., 10^{-3} millimeters of mercury, which, after extensive use, showed a considerable improvement in the vacuum, but that this improvement never resulted, in any case under my own observation, in the vacuum becoming so good as 10^{-6} or 10^{-7} millimeters.

In answer to Q. 28½, Dr. Milliken refers to methods of obtaining a vacuum as low as 10^{-6} millimeters, but this, of course, was in a laboratory, and from my general knowledge of commercial processes of evacuation, I do not believe that this order of vacuum represented a commercial result at the time of the Fleming patent.

Q. 283. Will you please consider Dr. Milliken's answers to X Qs. 46-51, inclusive (P. R., p. 854), and comment thereon according to your knowledge of detection and amplification?

A. I differ, of course, with Dr. Milliken's statement contained in his reply to X Q. 46, that the Fleming valve is not an amplifier, since I have proven by practical demonstration that it is.

The remaining cross-questions referred to in your question seem to constitute an attempt on the part of the cross-examiner to find out whether or not Dr. Milliken thinks that reception of radio signals is possible without rectification, and his answer appears to indicate that such reception is possible.

I am quite unable to understand any basis for this reply of Dr. Milliken's for the reason that it is a matter of simple observation to determine that such reception without rectification in some form or other, is impossible.

In his answer, Dr. Milliken refers to rectification of the audio-frequency and I must point out that while an incoming radio wave may be modulated at a great many audio frequencies and contain these frequencies as components of the wave-form, nevertheless, the only frequencies coming into the receiving instrument are radio-frequencies, and the audio-frequency components of the radio-frequency waves [fol. 1381] are not extractable from the waves, except by a process which involves rectification in some form or other.

I must therefore state that Dr. Milliken is mistaken in his replies to the questions referred to.

Q. 284. Do you agree with Dr. Milliken's answer to X Q. 78 (P. R., p. 858), regarding the physical principles underlying thermionic tubes having two electrodes, being applicable also to similar tubes having three electrodes?

A. I note that Dr. Milliken states that both the two-electrode and the three-electrode tubes utilize pure electron discharges produced by heating a filament to a high temperature in an evacuated tube. He differentiates between them, however, by referring to the control of the electron stream by changes in potential upon the grid, and the fact that, in his opinion, the Fleming valve is without this kind of a control. This is a differentiation of the sort with which I have disagreed on numerous occasions in the course of this deposition, for reasons based upon the performance of the Fleming valve in actual practical tests.

I therefore understand both the three-electrode and the two-electrode vacuum valves to employ the same underlying principles.

Q. 285. Please now refer to Dr. Milliken's answer to X Q. 79 (P. R., p. 859), state to what extent you may agree or disagree with him.

A. I must, of necessity, disagree with this statement for the simple reason that the application of any particular potential to the grid of a three-electrode tube results in the same change in the current flowing in the plate circuit, as would be produced by another and greater potential to the plate circuit itself.

This is, as a matter of fact, one way of expressing the fact that the device is an amplifier. For after all, that is all an amplifier is, viz., a device which has currents flowing in its output circuit whose magnitude is that which would result from the direct application to the output circuit of a potential which is greater than that actually applied to the input circuit.

* * * * * * *

When, in the three-electrode device a potential is apllied to the plate circuit directly, that is, the plate to filament circuit, the resulting current flow is proportional to the applied electromotive force and the resistance of the tube, and the energy involved is simply the energy which is supplied by the exterior source of power, whatever it may be.

When, however, the plate is split into two parts, one part serving for the application of the power in the form of a direct current, and the other serving for the input of the high-frequency energy, there is a marked change, and the energy which is then involved in the variations of the [fol. 1382] current of the plate circuit, is greater than the energy in the form of oscillations which are applied to the second cold element. This is, of course, one action which makes the device useful, since this action means that the device is an amplifier.

When Dr. Milliken, in his reply to X Q. 79, states:

"It certainly cannot be the equivalent in its result, else the grid might be dispensed with, and two-electrode tubes used as distortionless amplifiers, for example."

he does not reply to the question, but merely states that if the device were used in this way, it would not be an ampli-

fier which, of course, is self-evident from the explanations which have been given of the action of the three-electrode tube and the correct answer to X Q. 79 is "Yes".

Q. 286. Please refer to Dr. Milliken's answers to X Qs. 80 and 81 (P. R., p. 859), and state to what extent you may disagree with his answers.

A. Dr. Milliken, in answer to X Q. 80, which is a question directed to the three-element valve, answers on the basis of two of these electrodes, and states that there cannot be a direct causal relation between the applied potential and the current produced by it. He then qualifies this statement by admitting that interference with this causal relation by some third agency is possible, although, even in this case, he denies the result would be a negative resistance.

Of course, the three-electrode tube has three electrodes, and even if any two of them failed to disclose a negative resistance characteristic, this result would not be of particular significance, provided that the three taken in combination did possess a negative resistance.

Of course, in my answer to Q. 250 I have shown that the grid and filament alone may possess a negative resistance characteristic, and that this has been employed by White in his patent, 1,393,594, and I have also shown that the three electrodes, taken together, display a negative characteristic in various ways.

Dr. Milliken's concluding sentence, in his answer to X Q. 80, seems to express his particular and very limited definition of negative resistance as follows:

"At any rate, this is something quite distinct from the negative resistance of an arc which is due to copious ionization of gas or vapor molecules."

This seems to indicate rather clearly that Dr. Milliken is satisfied to differentiate the negative resistance of a three-electrode tube from that of an arc on the basis that the latter is due to copious ionization of the gas vapor.

Of course, in the hard three-electrode tube there is no vapor which is ionized, any more than there are two arcing carbon electrodes. The devices are distinct and different, but this fact is neither of consequence nor interest, since we are concerned here merely with a particular sort of electrical characteristic and not the nature of the device which possesses it.

As has already been very clearly brought out in my answer to Q. 250, a negative resistance characteristic may result from a variety of causes, and the only possible reason for attempting to tie it up with the arc is to secure a basis for differentiating between the soft vacuum tube which acts [fol. 1383] similar to the arc, and the hard vacuum tube, which has additional features of behavior.

Q. 287. Please refer to Dr. Milliken's answers to X Qs. 82-85, inclusive (P. R., pp. 859 and 860), and state whether, in view of your experience in the radio art, there was any difficulty in understanding the author's terminology and definitions of the quoted paragraph in X Q. 82 by one skilled in the art at the date Dr. Milliken testified, to-wit, December, 1923, said quoted paragraph forming a part of "Claimant's Exhibit No. 264", introduced during your deposition.

A. So far as I can see, no particular difficulty is involved in understanding the author's terminology and definitions.

Mr. Miller, in his testimony on pages 776 to 780 of the Printed Record, has a great deal to say about negative resistances and the particular action referred to in the Lauer and Brown book is explained by Dr. Miller at the bottom of page 776 and the top of page 777 as follows:

"In the case of such devices as have a falling characteristic or negative slope, when the current through the device increases, the opposing voltage decreases, when the current decreases the opposing voltage increases."

This statement by Dr. Miller is almost identical with the statement by the authors of the Lauer and Brown book and brings out the fact that when an increase in current flow through a device is associated with a decrease in voltage, and *vice versa*, the device is referred to as a "negative resistance", or is said to have a falling characteristic.

Q. 288. Please now refer to the deposition of Major Joseph O. Mauborgne, given on behalf of the defendant, and state to what extent you may disagree with his answer to Q. 15 as found on pages 1222-1224, inclusive, of the Printed Record, in which answer please refer to "Defendant's Exhibit H-5", and state to what extent the circuit No. 3 of this exhibit involves the apparatus and circuits of the patents in suit in this case.

A. The first part of Major Mauborgne's reply to Q. 16 consists largely of a brief reference to the work of Marconi,

Branly, Popoff, Tesla and Lodge, all of which has been covered in detail in my replies to various questions relating to the Marconi and Lodge patents. He next refers to early work by the Signal Corps of the United States Army and their attempts to establish communication over one hundred miles in Alaska.

While Major Mauborgne states, near the bottom of Printed Record page 1222, that some of the Signal Corps attempts to work in Alaska involved Marconi apparatus, yet his quotation from the report does not include a reference to Marconi apparatus, which leaves much uncertainty in my mind as to whether or not Marconi apparatus was actually used.

That Marconi apparatus was capable of signaling over the required distance would appear from the paper by Capt. H. B. Jackson, entitled "On some Phenomena affecting the Transmission of Electric Waves over the Surface of Sea and Earth." Proceedings of the Royal Society of London, 1902, Volume 70, page 254. This paper contains a table entitled "Table No. IV" which gives distances over which working was had and included amongst these distances is [fol. 1384] one of 135 miles and another of 105 miles, both of which are greater than the requirement of the Alaskan situation.

In considering a report of the nature of that of the Chief Signaling Officer, referred to by Major Mauborgne, relative to the state of the art at this time, I am obliged to recognize some facts, gained as the result of long experience in dealing with the United States Government in radio matters.

I have learned, as the result of this experience, that it is common practice for the Government personnel, after becoming familiar with the work of commercial companies through the purchase of their apparatus, to condemn all of the purchased apparatus and proceed with the development of something which they then call their own.

I have usually been forced to the conclusion that the real reason for this procedure has been the desire of the Government Bureaus to get hold of some interesting work and I have in general found that the apparatus so developed by the Government Bureaus involved little or nothing that was essentially new, but, on the contrary, employed the devices, patents, etc. of the commercial companies.

An illustration of this sort of thing, I think, is to be found in the arrangement used by Capt. Wildman, shown in the "Electrician" of October 20, 1905. The diagram of connections contained in this article appears to involve both the Marconi and Lodge patents. There is a closed circuit consisting of two condensers marked "Capacity", a spark-gap and a portion of the coil, marked "Linking Coil". There is also an antenna system involving an elevated conductor, a portion of the linking coil, a second coil and an arrangement marked "Wire Net", which is merely a form of what is known as the counterpoise earth connection.

The descriptive article specifically states that the closed circuit and the open circuits were in resonance, which fact was determined by the hot wire ammeter, and from this fact and the description just given, this arrangement is evidently nothing more than that of the Marconi tuning patent, and it includes the Lodge variably-acting inductance coil.

It is furthermore perfectly obvious that all Wildman did was to employ the Marconi arrangement with a considerable amount of power. The specification specifically refers to 3 Kilo-watts which, at that time, was a very substantial power to use in radio.

I am therefore unable to find that the work of Wildman added anything new to the knowledge of the radio art, or that it can be considered in any way as a particularly material contribution to the development of the art.

Further evidence of the capabilities of the Marconi apparatus in the matter of distance working is contained in Mr. Marconi's testimony (P. R., p. 504), wherein he testifies relative to the working between the station at Poldhu and the Steamship Philadelphia in 1902, over a distance of 2,099 miles.

Similarly on pages 571 and 572 of the Printed Record, Mr. Marconi testified relative to the transmission of messages from Canada to England in 1902. The distances referred to in Mr. Marconi's testimony are very great, compared to the distance which had to be covered in Alaska and it is rather difficult to see any legitimate basis for [fol. 1385] assuming that the Signal Corps had to improve on Marconi's work, in order to carry out their required communication system.

I note a diagram in "Defendant's Exhibit H-5" which is a photostat copy of blueprint No. 3, Wiring Diagram,

Massie Wireless Telegraph Company, of apparatus furnished on Signal Corps Order No. 3428, as installed at Fort Gibbon, Alaska, 1908. The transmitting diagram of this station is substantially the same as that of the Wildman arrangement, referred to above, with the exception that all of the antenna inductance is contained in a single coil. This coil serves as inductance for both the open circuit and the closed circuit, and as a means of linking the two circuits together: that is, as a direct coupling, and it is quite obviously a tuned coupled circuit arrangement of the sort disclosed by Marconi in his tuning patent. This drawing was introduced by Major Mauborgne in answer to Q. 8.

In further developing his historical treatment of the development of radio, Major Mauborgne refers to the introduction of detectors of the electrolytic and crystal types and the extension of their use all over the world. He neglects, however, at this point, to refer to the invention and development of that detector which was destined to lay the foundation for the whole of the present day radio. I refer, of course, to the Fleming valve, which Dr. J. A. Fleming gave to the world in 1904.

While Major Mauborgne does refer to the introduction of continuous wave telegraphy in 1913 and the introduction of the three-electrode vacuum tube, about which, as he himself states, the whole radio art of today revolves, he again neglects to point out the fact that all of this development was made possible by the discovery and invention of the Fleming valve.

He makes a further statement to the effect that the advance made by Marconi over his predecessors cannot in any way be compared with the advance made by the introduction of the three-electrode tube, but his statement in this regard is not at all impressive to me, for the simple reason that Marconi is the father of radio, and that, regardless of what the subsequent developments of radio may be, they do not detract from the credit due Marconi for his original conception and execution, but, on the contrary, merely add to his undying fame.

I do not think that any work in the development of a great art, no matter how important it may be, can ever be considered in the same light or as of equal importance with that work which gave birth to the art, and I must, therefore,

disagree with Major Mauborgne in his assignment of relative values to the work of Marconi and others.

Q. 289. Although you have already fully treated the subject-matter of Major Mauborgne's answer to Q. 17 (P. R., p. 1224) of his deposition, I will ask you to refer to the concluding paragraph of his answer and state briefly in what respects you differ from his conclusions.

A. I must, of course, disagree with Major Mauborgne's statement to the effect that the two-electrode tube is not inherently capable of either oscillating or amplifying, since I have shown so conclusively that it possesses both of these capabilities.

Major Mauborgne describes somewhat at length various tests which he made, most of which failed to give any results in the matter of oscillating, but I am afraid that these tests are not particularly impressive, for the simple reason [fol. 1386] that it is a very easy matter to conduct tests which are unsuccessful, and when once it is shown that a thing can be done, the only significance of unsuccessful tests is that they indicate a failure on the part of the experimenter to appreciate the correct way of getting results.

I note that on Printed Record, page 1230, in concluding his answer, Major Mauborgne refers to a gas filled tube of the Fleming type, a characterization to which I much take exception, since a tube exhausted in accordance with the instructions of the Fleming patent would have a very good vacuum, and would be anything but "gas filled".

Mr. Vaill: Counsel for claimant offers in evidence a photostat copy of the title page and the subsequent pages of the "Proceedings of the Royal Society, London", Volume 70, 1902, beginning page 254, which contain an article "On some Phenomena affecting the Transmission of Electric Waves over the Surface of Sea and Earth", by Capt. H. B. Jackson, and requests that the same be marked "Claimant's Exhibit No. 279".

Also a photostat copy of the letter to the Editor of the "Electrician" of London, by Capt. Leonard D. Wildman, dated September 30, and published in the "Electrician" (London) on October 20, 1905, as "Claimant's Exhibit No. 280", and requests that the same be so marked.

Mr. Vaill: I also offer in evidence a photostat copy of the title page of the Proceedings of the Physical Society of

London, Vol XIV of 1896, together with photostat copies of pages 187 to 242, inclusive, containing an article by J. A. Fleming, entitled "A Further Examination of the Edison Effect in Glow Lamps" (Read March 27, 1896), being a copy of "Plaintiff's Exhibit No. 18" in the suit of the Marconi Wireless Telegraph Company of America *vs.* De Forest Radio Telephone and Telegraph Company in the Southern District of New York, on Fleming patent 803,684, as "Claimant's Exhibit No. 281".

(Said exhibits marked as requested.)

* * * * * * *

Q. 290. Referring further to the use of the two-electrode Fleming valve in connection with a battery potential on the plate or anode, please give any further instances of prior art publications which show that such a use was well known.

A. The British patent to Brown & Neilson, No. 28,955, of 1896, discloses an arrangement involving the use of a coherer and in Fig. 9 this coherer is shown connected with a telephone receiver, and the specification refers to it on page 7, lines 24 to 33, as follows:

[fol. 1387] "* * * We find that a Telephone Receiver used in this way, in conjunction with a Coherer, having preferably but not necessarily, carbon granules, forms a very sensitive receiving set for detecting very weak radiant energy as when coming from a long distance, *etc.*, because the Telephone is able to appreciate far more minute and temporary (but sudden) changes in the resistance of the Coherer than are required to vary a battery current sufficiently to operate any other telegraphic instrument. Used in this way also no restoring vibrator is required for the coherer, or at most only an arrangement to give a few momentary taps at long intervals, but this can be conveniently done by hand. * * *"

This patent indicates not only that a battery was used with a detecter prior to the Fleming patent in suit, but also that the coherer was used with a telephone receiver and without a tapper.

In "Claimant's Exhibit No. 281", pages 234 and 235 (Claimant's Ex. 281), experiments 24 and 25 bring out certain effects resulting from the use of a battery with a Fleming valve. This exhibit is a paper by Dr. Fleming in the

"Proceedings of the Physical Society of London," Vol. 14, 1896. Experiment 24 illustrates the fact that the single battery cell causes an appreciable current to flow through the experimental form of the Fleming valve, provided that the cold electrode is made positive with respect to the filament, while in experiment 25, twenty-five cells were connected in series, and it was found that when the negative pole of the combination was attached to the cold element there was no greater current flow than was obtained with the single cell; whereas, when the positive pole was connected to the cold element, the current flow was much greater than with a single cell.

These two experiments bring out two essential facts relative to the device, first, that it conducts in one direction only; secondly, that a greater current flowed through it when the potential was increased.

In "Claimant's Exhibit No. 269", which is also a paper by Dr. Fleming in the "Proceedings of the Royal Society of London", Vol. LXXIV, 1905, page 483, there is shown a curve which is an extension of experiment 25 of "Claimant's Exhibit No. 281" (which is Fleming's 1896 article), and in which the complete relation between applied potential and current flow through the Fleming d vice is exposed, and curves are plotted which illustrate this relation.

It will be noted that these curves are of irregular form and their shape is such as would indicate to one skilled in the art that the application of some particular potential to the device might result in an improvement in its action as a rectifier. There is indicated, for instance, on the curve between the points 10 and 20 a curvature of the sort which is common to detectors.

Judge Mayer, in his decision reported in 236 F. R., page 942, as found on page 952, makes the following and interesting comment relative to the use of a battery with a Fleming valve:

"The local battery is used to bring the lamp detector to the sensitive point of its characteristic curve, and the potentiometer is the simple and effective device which, varying the local battery, accomplishes the task. Nearly all prior art detectors were used in this way—the coherer of [fol. 1388] Marconi and Lodge; the microphone of Hughes and Branly; the electrolytic of Fessenden, Vreeland and others; and the crystal of Bose. Plaintiff's Exhibits 77 and 82.

"The use of the local battery to locate the sensitive point on the characteristic curve was well known and accepted as of Fleming's time, and, as appears by his 1905 lecture, was fully understood by him."

Mr. Vaill: Counsel for claimant offers in evidence a photostat copy of British Patent 28,955 of 1896, to Brown & Neilson, referred to in the last answer as "Claimant's Exhibit No. 282".

(Said exhibit so marked.)

* * * * * * *

[fol. 1389] Q. 293. Referring to Dr. Miller's answer to Q. 18 of his deposition (P. R., p. 791), regarding the operation of the two-electrode valve without a biasing battery, as compared with is operation when a biasing battery is used, and also contrasting the same with the three-electrode tube, will you please refer to any patents which indicate that the operation of a two-electrode tube and a three-electrode tube is similar in the respects mentioned?

A. British Patent No. 13,518, A. D. 1908, (Drawings Reproduced Opposite as part of Claimant's Ex. 287) discloses an arrangement due to Dr. Fleming, in which there is a radio receiving circuit including a Fleming valve, to which is applied variable potential between the anode and the cathode, derived from the heating battery. The mechanism for varying this potential is a potentiometer and is illustrated in the drawing by *j*, *k*. This potentiometer allows of the application of a biasing potential to the Fleming valve which enables detection to take place at the most sensitive point of the characteristic curve.

This is an arrangement frequently, in fact, generally, employed with the commercial sets manufactured by the British Marconi Company.

In British Patent No. 887 of 1907 (Drawings Reproduced Opposite as part of Claimant's Ex. 288) and the corresponding United States Patent No. 896,130 of August 18, 1908, there is shown an arrangement of the Fleming valve in a radio receiving set, the particular feature of which is the telephone transformer shown in the diagram of the United States patent as *n*, whose primary circuit is included in the valve circuit, and whose secondary circuit includes the telephone receivers, shown at *s*.

This particular arrangement of the telephone transformer was frequently employed in Marconi apparatus in

connection with the potentiometer of patent No. 13,518 of 1905, above referred to, and when so employed the battery potential used for biasing the Fleming valve does not flow through the telephone receivers, and this particular use is evidence of the fact that the effect of the battery potential is to increase the sensitiveness of the Fleming valve and not to affect the telephone receiver.

Mr. Vaill: Counsel for claimant offers in evidence photostat copy of British Patent No. 13,518 of 1908, to J. A. Fleming as "Claimant's Exhibit No. 287."

Also photostat copy of British Patent No. 887 of 1907, to G. Marconi as "Claimant's Exhibit No. 288."

Also printed copy of United States Patent to G. Marconi, No. 896,130 of August 18, 1908, as "Claimant's Exhibit No. 289."

(Said exhibits were so marked.)

Q. 294. Referring to Mr. Mauborgne's statement (P. R., p. 1231), in answer to Q. 18, that he had unsuccessfully examined publications regarding statements as to the two-electrade tube being capable of oscillation, can you refer to any publications which Mr. Mauborgne apparently over- [fol. 1390] looked?

A. In the work entitled "Themionic Vacuum Tube" by Van der Bijl, on pages 269, 270 and 271 Claimant's Ex. 290), there is a description and a circuit diagram, Fig. 151, of a two-electrode oscillating arrangement. Dr. Van der Bijl outlines the mathematical conditions which must be fulfilled in order ~~that the two-elect~~rode device shall operate and on the top of page 271 he gives his conclusions as follows:

"From this we see that in order to obtain sustained oscillations from a device having only two electrodes, it is necessary that the device shall have a negative resistance."

The arrangement shown and described by Dr. Van der Bijl is capable of oscillating at radio frequencies.

I note, also, that British Patent No. 104,566 of 1916 to Brown Claimant's Ex. 291), discloses a two-electrode valve connected in circuits designed to produce oscillations and an arrangement which, according to the specification, will produce oscillations of frequencies corresponding to the voice frequencies.

The principle of operation of this device is different from that of the arrangement described by Van der Bijl, in that it depends upon a variation of the filament temperature and not upon a negative resistance.

It is noted that the British patent to Brown, No. 104,566, of 1916, is referred to on page 200 of the book by Leggett, entitled "Wireless Telegraphy" of 1921, from which "Defendant's Exhibits N, G-5 and I-6" were taken, and also on page 53 of the book by Scott Taggart, entitled "Thermionic Tubes" of 1921, from which "Defendant's Exhibit M-5" was taken.

Mr. Vaill: Counsel for claimant offers in evidence photostat copies of pages 269, 270 and 271 of Van der Bijl's book "Thermionic Tube" of 1920, as "Claimant's Exhibit No. 290."

Also photostatic copy of British Patent No. 104,566 of 1916, to Brown, as "Claimant's Exhibit No. 291."

(Said exhibits so marked.)

Q. 295. Referring to Mr. Loftin's statement in the middle of page 1149 of the Printed Record that:

"I have read a large part of the literature of the world on the subject of radio and do not recall a single reference to the practical use of the two-electrode tube."

Can you give an instance of such a reference in the literature which Mr. Loftin apparently overlooked?

A. In the work entitled "The Principles of Electric Wave Telegraphy and Telephony" by J. A. Fleming, 3rd Edition, 1916, page 891, I find the following statement relative to the use of the Fleming valve:

"Using his liquid microphone as above described and a Fleming glow lamp oscillation valve as a receiver, Prof. Vanni achieved the feat of speaking clearly by radiotelephony from Rome to the Island of Ponza (120 km.), to Maddelena (260 km.), to Palermo (420 km.), to Vittoria (600 km.), and finally to Tripoli (1,000 km.). The timbre or [fol. 1391] quality of the voice is said to be reproduced with great accuracy."

It is noted that "Defendant's Exhibit U" was taken from this same Fleming book.

The 1,000 kilometers referred to in the above quotation is a distance somewhat over 600 miles.

Mr. Vaill: Counsel for claimant offers in evidence a photostat copy of page 891 of the book by J. A. Fleming, entitled "The Principles of Electric Wave Telegraphy and Telephony", 3rd Edition, 1916, as "Claimant's Exhibit No. 292."

(Said exhibit so marked.)

Q. 296. On page 1039 of the Printed Record, near the top, Mr. Loftin has stated as follows:

"In the actual example above I have shown that if the interlinkage is such as to result in a coupling of 3 per cent., for all practical purposes, there may be considered to be but one frequency."

And at the bottom of page 1157 and the top of page 1158 of the Printed Record, in answer to XQ. 72, he states as follows:

"In the open-gap type of system, when the coupling is loosened to the neighborhood of 3 per cent., it is found in practice that ordinary receiving apparatus will detect but one frequency, and in practical operation such a coupling is commonly known as loose, though couplings less than 3 per cent. are sometimes employed for extreme selectivity."

And in answer to XQ. 109 on Printed Record page 1166, he states:

"It is my opinion that to use the Marconi apparatus to accomplish the object set up in the manner set up, as I have quoted in my answer just previous, that the coupling should not exceed 3 or 4 per cent."

Will you please give an instance in the literature of practical operation of an open spark-gap with which greater coupling than this was used?

A. In the September 8th, 1911 "Engineering", there is an article entitled "Recent Developments in Radio-Telegraphy" by Prof. G. W. O. Howe (* * * Claimant's Ex. 293), relative to some experiments with coupled circuits having a local spark-gap.

In Fig. 7 a number of curves are shown which illustrate the resonance curves resulting from the use of various couplings, and curve C is representative of a good wave form and as shown by the table which is a part of Fig. 7,

resulted from a coupling of 7.8 per cent. This figure is considerably in excess of the figure given by Mr. Loftin and quoted in your question.

The article also contains an interesting statement illustrative of the quenching action of the open gap, to which I have frequently referred. This statement is as follows:

"In the experiment to which Fig. 7 refers, the spark-gap was exposed to an air-blast, so that whenever the amplitude [fol. 1392] of the current in the primary oscillating circuit was small, there was a tendency for the air-blast to remove the column of hot air and metallic vapour, and thus extinguish the spark. This will obviously occur at a moment when all the energy is oscillating in the aerial, and will prevent it surging back into the primary circuit. The aerial will now continue to oscillate at its natural frequency until the energy is all radiated or dissipated. In Fig. 7 not only was there an air-blast, but the electrodes were comparatively large, and the spark-gap short, so that no part of the spark was far removed from a large mass of cold metal tending to cool the heated air and condense the metallic vapour, and thus lower the conductivity of the spark."

Mr. Vaill: Counsel for claimant offers in evidence a photostat copy of page 336 of "Engineering" of September 8th, 1911, article by Prof. G. W. O. Howe, as "Claimant's Exhibit No. 293."

(Said exhibit so marked.)

* * * * * * *

Q. 297. Referring to your answer to Q. 288 of your present deposition, in which you consider Major Mauborgne's answer to Q. 16 of his deposition, in which he quotes from the report of the Chief Signal Officer of the Army for the year 1904, regarding instruments purchased from the Lodge-Muirhead Wireless Telegraph Co. (Ltd.) of Great Britain, and of others, will you please give any instances which have come to your attention showing the transmission of commercial wireless telegraph messages over a distance greater than one hundred miles, referred to in the report as the distance of transmission required in Alaska?

A. In the work entitled "A Handbook of Wireless Telegraphy" by J. Erskine-Murray, D. Sc., 1907, there is a de-

scription of the Lodge-Muirhead system and some information relative to a practical installation of this system, which shows that it was employed over distances of more than three hundred miles.

On page 163 (* * * Claimant's Ex. 294), Fig. 94 shows a map and on this map is a dotted triangle, and the apices of this triangle indicate the location of three radio stations between which communication was to be maintained. The following quotation from pages 162 and 163 indicates the nature of the operations:

"Towards the end of August 1904 operations commenced for linking up the Settlement of Port Blair in the Andaman Islands with the general telegraph system of India by establishing wireless telegraph stations at Port Blair and at Diamond Island, which is already connected by a short cable [fol. 1393] to the mainland. The distance to be spanned is 305.2 miles. Between Port Blair and Diamond Island is situated Table Island, on which is a lighthouse and close to which ships pass on the voyages to Rangoon and Colombo, and Calcutta to the Straits. It was therefore decided to take the opportunity of establishing a station on Table Island which it was hoped would prove useful in the shipping and to the Meteorological Department. This distance from Diamond Island to Table Island is 130.2 miles, and from Table Island to Port Blair 177.5 miles. A map

(Fig. 94)

is attached (Fig. 94) which shows the positions of the stations."

The success of the installation seems to be indicated by the following statement on page 167:

"Since April statistics were kept of the traffic exchanged; and a copy of the records for the months of April, May and June is appended. The installation was only opened in a limited degree for State traffic, and the actual number of messages tended and dealt with in April was only fifty-five sent from Port Blair, and thirty-nine received for Port Blair, but a great deal was sent in addition to these messages, and the traffic during the month amounted to nearly 10,000 words, and more could easily have been done had it been required. It will be noticed there were no interruptions during the month of April. During the month of May traffic exchanged amounted to nearly 14,000 words.

The traffic during July, August, September, October, and November has amounted to 18,542, 21,286, 21,408, 27,398 and 26,938 words of actual messages. More could, of course, have been dealt with had it been tendered."

In evaluating the performance of the Lodge-Muirhead system, referred to in the above quotation, it is of importance to note that this work took place in a tropical climate and was, therefore, under much more adverse circumstances than the conditions of operation to be found in the Alaskan situation, where the transmission was over Norton Sound, from the neighborhood of Nome to St. Michaels, according to the report of the Chief Signal Officer.

In a paper by G. Marconi, communicated by Dr. J. A. Fleming, to the Royal Society of London (Proceedings, Vol. 70, 1902, pages 341, 342 and 343), entitled "Note on a Magnetic Detector of Electric Waves, which can be employed as a Receiver for Spaced Telegraphy", read June 12th, 1902, there is a description of a type of detector known as a magnetic detector.

After describing the detector, the paper gives some information relative to its use in actual radio-communication, as follows:

"This detector has been successfully employed for some time in the reception of wireless telegraphic messages between St. Catherine's Point, Isle of Wight, and the North Haven, Poole, over a distance of 30 miles, and also between Poldhu, in Cornwall, and the North Haven, over a distance of 152 miles, of which 109 are over sea and 43 over high land."

[fol. 1394] This quotation shows clearly successful radio telegraphic communication over a distance considerably greater than that of the Alaskan situation, at a date prior to the date of the report by the Chief Signal Officer, relative to the failure of commercial apparatus to prove satisfactory.

Mr. Vaill: Counsel for Claimant offers in evidence a photostat copy of pages 162, 163 and 167 of "A Hand Book of Wireless Telegraphy" by J. Erskine-Murray, of 1907, giving transmission by the Lodge-Muirhead system, as "Claimant's Exhibit No. 294."

Also a photostat copy of pages 341, 342 and 343 of the Proceedings of the Royal Society of London, Vol. 70, of

1902, containing an article on the Magnetic Detector by G. Marconi, as "Claimant's Exhibit No. 295."

(Said exhibits marked as above.)

Q. 298. Will you please compare the structural and operating features of the VT-1 three-electrode tube, referred to and produced by Dr. Miller during his deposition as "Defendant's Exhibit F", with the tubes held to be an infringement of the Fleming patent No. 803,684, in the case of Marconi Wireless Telegraph Company of America *vs.* De Forest Radio Telephone and Telegraph Company, in the Southern District of New York, which tubes have been offered in evidence in the present case as parts of "Plaintiff's Exhibit No. 82", which were also part of "Plaintiff's Exhibit No. 2" in the DeForest case, and state whether said VT-1 tubes of "Defendant's Exhibit F" are similar or differ in any substantial point?

A. The VT-1 tube produced by Defendant's expert is fundamentally identical in structure and mode of operation with the three-electrode tube of "Plaintiff's Exhibit No. 82." Both devices consist of an evacuated glass vessel enclosing a filament which can be heated to incandescence by a battery, a cold electrode in the form of a plate and a second cold electrode in the form of a grid interposed between the hot and cold electrodes.

Both of these devices are capable of the usual uses of the three-electrode tube, viz., detecting, oscillating and amplifying. Structurally they differ in the details of manufacture, the VT-1 having all three electrodes of larger dimensions than the three-electrode tube of "Plaintiff's Exhibit No. 82" and also the BT-1 tube is so constructed that part of the plate element and part of the grid element is on each side of the filament.

The vacuum in the VT-1 tube is of a somewhat higher order than the vacuum of "Plaintiff's Exhibit No. 82."

Q. 299. Please state, according to your knowledge of prior litigation relating to the Fleming patent in suit, No. 803,684, whether or not any alleged additional anticipatory matter has been introduced by the Defendant in the present case, that was not in evidence in the prior litigation.

A. No new evidence relating to prior inventions has been introduced by the Defendant in this case, which was not in prior litigation.

* * * * * * *

[fol. 1395] Q. 300. You have already stated, in answer to Q. 243 of your present deposition, that you were present and testified in the suit of Marconi Wireless Telegraph Company of America vs. DeForest Radio Telephone and Telegraph Company on the Fleming Patent No. 803,684 during the supplemental trial and demonstrations before Judge Mayer, as set forth in "Claimant's Exhibit No. 171" in this case. Did you testify in the original trial in that case and to what extent were you present during the trial?

A. I was present throughout the greater part of the original trial, and took part in this trial in the suit of the present claimant against the DeForest Company on the Fleming patent, and testified therein.

Q. 301. The opinions of Judge Mayer in the District Court and of the Circuit Court of Appeals of the Second Circuit, refer to the "P N detector", and Judge Mayer's opinion (236 Fed., 942, 956), in the case referred to in the previous question, contains illustrations entitled "Simplified Circuits of 'P N' Detector", and also one entitled "Fleming & DeForest Compared". What similarity exists between the P N detector referred to and illustrated in the circuits of the opinions above mentioned and the P N circuit mentioned by Dr. Miller in answer to Q. 17 of his deposition, and which you and Mr. Waterman have each referred to in your depositions in this case?

A. The P N circuit referred to by Dr. Miller, Mr. Waterman and myself is the circuit of the P N type of audion detector referred to and illustrated in the decision of the Court in the Marconi-DeForest case, and which was held in that case to be an infringement of claims 1 and 37 of the Fleming patent 803,684.

Q. 302. In what respects, if any, do the contentions of Defendant's Experts in the present case differ from those of Defendant's Experts in the Marconi Co. vs. DeForest Co. suit in respect to the P N type of audion detector and its circuits, as compared with the circuits shown in the Fleming patent in suit?

A. The contentions of the Defendant's Experts in respect to the P N audion in this and the Marconi-DeForest case are substantially the same, as appears from the record and the opinion of the Court in the latter case.

Q. 303. Judge Mayer, in his memorandum opinion filed July 11, 1917, in said case of Marconi Co. vs. DeForest Co., holding that the decree and injunction filed and granted in

the original case covered and included the DeForest amplifier (see "Claimant's Exhibit No. 171", page 72), refers to some audion detector amplifier sets sold by the DeForest Company. According to your knowledge of the proceedings in that suit, please state whether or not any of those sets were in evidence and tested during the trial of that case. If so, please state how they were tested and with what results.

A. There was in evidence at the original trial as "Defendant's Exhibit A-4" a combination P N audion, ultra-audion and two-step amplifier employing three audion tubes. [fol. 1396] The circuit arrangement, when the P N circuit was used, was substantially the same as "Plaintiff's Exhibit No. 110", Fig. 88-A from the Navy Manual of 1915. This apparatus was tested before Judge Mayer at the original trial. Signals were received on the telephone by inserting the telephone plug in the three different sockets of the set. When the telephone plug was inserted in the first socket, the signal was detected and amplified by the first tube; when the telephone was placed in the second socket, the signal was then detected and further amplified by the second tube, and when the telephone plug was placed in the third socket the signal was detected and still further amplified.

Q. 304. Referring further to the use of local batteries in detectors for radio frequency signals prior to the invention of the DeForest three-electrode vacuum tube, will you please refer to any further instances showing such use, and also indicate, by appropriate diagrams, how the Fleming valve may be substituted in prior detecting circuits without other substantial change in such circuits?

A. I produce a drawing (Claimant's Ex. 298) showing diagrams taken from nine patents in which a battery is used with a detector; accompanying these diagrams is a corresponding diagram showing a Fleming valve inserted in the place of the detector illustrated in the diagram of the patent.

The patents referred to, from which these circuit diagrams are taken, are as follows:

Lodge, No. 609,154, Fig. 3, issued August 16, 1898, on application filed February 1st, 1898, "Plaintiff's Exhibit No. 20".

Marconi, No. 627,650, Fig. 1, issued June 27, 1899, on application filed January 5, 1899, "Defendant's Exhibit B-6"; also the same patent, Fig. 2.

Branly, No. 796,880, Fig. 1, issued August 8, 1905, on application filed September 4, 1902.

Fessenden, No. 727,331, Fig. 1, issued May 5, 1903, on application filed April 9, 1903, "Claimant's Exhibit No. 265".

DeForest & Smyth, No. 716,203, Fig. 1, issued December 6, 1902, on application filed September 1, 1900, "Defendant's Exhibit X-4".

Hogg, No. 763,894, Fig. 3, issued June 28, 1904, on application filed February 9, 1903.

Bose, No. 755,840, issued March 29, 1904, on application filed September 30, 1901, "Claimant's Exhibit No. 267".

Vreeland, No. 780,842, Fig. 4, issued January 24, 1905, on application filed August 1, 1904, "Claimant's Exhibit No. 266".

All of these patents were in evidence in the case of Marconi Wireless Telegraph Company of America *vs.* De Forest Radio Telephone & Telegraph Company, above referred to.

Mr. Vail: Counsel for claimant offers in evidence the patent to Branly, No. 796,880, of August 8, 1905, as "Claimant's Exhibit No. 296".

Also patent to Hogg, No. 763,894, of June 28, 1904, as "Claimant's Exhibit No. 297".

The chart produced by the witness is offered in evidence as "Claimant's Exhibit No. 298", and it is requested that said exhibits be marked accordingly by the Notary.

(Said exhibits so marked.)

[fol. 1397] Q. 305. Will you please refer to, and quote from, any technical publications where the authors state their opinions of the identity between the Fleming valve and the so-called DeForest audion detectors?

A. The first publication to which I shall refer is the Bureau of Standards Bulletin No. 140, containing an article entitled "The Comparative Sensitiveness of Some Common Detectors of Electrical Oscillations," by Dr. Austin, physicist in charge, United States Naval Wireless Laboratory. This is contained in "Defendant's Exhibit

F 1". At page 538 of this publication he first refers to the Fleming vacuum valve detector and describes its construction. On page 539 he shows the connections. On page 540 he refers to the audion and shows a diagram of the so-called P N circuit. On page 541 he states as follows in the first complete paragraph:

"The action of the detector appears to be as follows: The heated filament F is charged negatively by the battery B and a steady stream of negative electrons flows from the hot filament to the cold plate producing a direct current through the telephone. When an oscillatory electromotive force is impressed on the circuit F L⁴ G, the negative current can flow from F to G inside the bulb and not in the reverse direction, just as in the Fleming detector. The grid G thus becomes negatively charged and as it lies between F and P the direct current through the telephone is decreased as may be shown by placing a galvanometer in the telephone circuit."

Next I desire to refer to the U. S. Government publication entitled "Manual of Wireless Telegraphy for the Use of Naval Electricians", by Lieut.-Commander S. S. Robison. This was published in 1909. I invite particular attention to the paragraph on page 102, under the heading "Vacuum Tube Rectifiers", which is as follows:

"151. It has long been known that under certain circumstances negative electricity can be made to flow from an incandescent electrode to a cool one more easily than in the opposite direction. Prof. Fleming has taken advantage of this fact to construct a rectifying detector for electrical waves. This is essentially an incandescent electric light bulb into which a second electrode, in the form of a plate or of a cylinder surrounding the filament, is introduced. The filament is kept at a white heat by means of storage cells. If then the receiving circuit is connected to the filament and the cold electrode, the high frequency oscillations are able to pass only in one direction; i. e., they are converted into direct currents, which may be made to act upon a galvanometer or telephone. The rectifier can be made somewhat more sensitive by applying an external electromotive force in series with it and the telephone."

"Dr. DeForest has also devised a detector called the audion, which rests apparently upon the same general principles."

In the 1913 edition of the Navy Manual which has been offered in evidence as "Plaintiff's Exhibit No. 78", I refer [fol. 1398] to pages 133 and 143. Page 133 shows the so-called Marconi and Fleming diagrams, and I invite attention to the statement on page 143 under the caption "Vacuum Tube Detectors", particularly the first two sentences, which state:

"The two forms in use are Marconi *valve* detectors and the DeForest audion. This detector invented by Fleming *is a rectifier* permitting the passage of current in one direction only."

Explanation of unilateral conductivity is then made and the figures of the diagrams described. Figure 84, which is referred to, is to be found on page 134.

Referring now to the 1915 edition of the Navy Manual, which is also a part of "Claimant's Exhibit No. 78", I invite attention to pages 133, 134 and 143, which contain similar statements, but are somewhat more elaborated.

The next publication to which I refer is the article by Major Edwin H. Armstrong, published in the "Electrical World" of December 12, 1914, entitled "Operating Features of the Audion". This article is "Claimant's Exhibit No. 230". Major Armstrong goes into the details of the operation at considerable length and he points out in the first column of page 1149:

"The audion is essentially an electron relay: that is, the exhaustion is carried to such a point that the amount of gas present is exceedingly small, and the current between the hot and cold electrodes is entirely thermionic, the absence of gas making impossible the presence of positive ions."

In this paragraph Major Armstrong says that the action, which is the essential or fundamental action, is the thermionic or electronic conduction. On page 1151 Major Armstrong speaks of the simple or fundamental action of the device: namely, its valve action. I will refer first briefly to page 1150, at the bottom of the first column, where he says, referring to Fig. 7:

"The action is much the same as in one of Prof. Fleming's methods of using the valve."

Then, passing to the first column on page 1151, he says:

"To make use of the 'valve' action between hot and cold electrodes for the detection of high frequency oscillations a connection as shown in Fig. 13 is used.

"In this case a condenser C is inserted somewhere in the circuit between the grid and filament to prevent the flow of a continuous current between them, and the grid is therefore left free to assume a potential determined by its position with respect to the filament and wing. Usually this will be somewhere near the center of the operating part of the curve of Fig. 1; that is, near point P. Now the action for incoming oscillations, as far as the closed oscillating circuit, filament, grid and condenser C2 are concerned, is identical with the rectifying action of the Fleming valve."

The next publication to which I refer is "The Electrician" for November 21, 1913, containing the editorial pages where [fol. 1399] the editor reviews the paper by Dr. DeForest under the title "The Audion as a Relay". This paper was presented by Dr. DeForest before the Institute of Radio Engineers, and describes the audion as a Fleming valve with an additional electrode. The last two sentences of the editorial are particularly interesting as being a comment upon Dr. DeForest's attitude toward Prof. Fleming's process valve. They read:

"In passing, we must confess we have no affection for the name 'audion' to which exception was taken at the time it was introduced, and we think that Dr. DeForest might be more generous in his acknowledgment of the work of Dr. J. A. Fleming. Our readers generally will probably agree that the audion, although differing widely from the Fleming valve, is an offshoot of it."

All of these publications were in evidence, together with several others, in the case of Marconi vs. DeForest Co. on the Fleming patent and the Court of Appeals of the Second Circuit in its decision on appeal, referred to the editorial in "The Electrician," as above quoted.

Mr. Vaill: Counsel for claimant offers in evidence a photostat copy of pages 102 and 103 of the "Manual of Wireless Telegraphy for Use of Naval Electricians," by Lieut.-Commander S. S. Robison, for the year 1909, and requests that the same be marked "Claimant's Exhibit No. 299."

(Said exhibit marked as requested.)

Mr. Vaill: Counsel for claimant offers in evidence the first and second pages of the November 21st, 1913, issue of "The Electrician" (London), and requests that the same be marked "Claimant's Exhibit No. 300."

(Said exhibit marked as requested.)

Q. 306. Referring to your answer to Q. 275 of your present deposition, in which reference is made to "Claimant's Exhibit No. 278," which is Government Specification entitled "General Specification No. 2087" (October 24, 1918) "Tubes Type VT-1," please state what difference in the use of the tubes referred to as "blue label tubes" and "yellow label tubes" was intended by the Government.

A. The following sentences 1 (a) of the specification of "Claimant's Exhibit No. 278" state as follows:

"This specification covers blue label and yellow label tubes. Blue label tubes are those that are free from all mechanical defects and pass all tests herein specified. Yellow tubes are those tubes which are slightly imperfect but which are suitable for training purposes in the United States. No vacuum tubes shall be accepted as yellow tubes which do not give reasonable assurance of satisfactory operation in any kind of service to which they may be put."

These sentences show that the Government divided the tubes into classes; that is, first grade and second grade, the blue tubes or first grade tubes being intended for general [fol. 1400] uses, while the yellow tubes or second grade tubes, were intended for training purposes only.

Q. 307. Dr. Miller states, in answer to Q. 25 of his deposition (P. R., p. 817), that the circuits the Government has used, and as shown in "Plaintiff's Exhibits Nos. 113, 114 and 115" are the so-called regenerative circuits, which utilize the amplified radio-frequency currents discarded in the P. N. circuit through the medium of the tickler or feed-back coil and enabled the operator to obtain signals twenty or thirty times louder than by use of the ordinary P N circuit, this result being made possible because of the amplifying property which he alleges to be possessed only by the three-electrode vacuum tube.

Dr. Miller also states that the tickler or feed-back coil can be used only with the three-electrode vaccum tube. Please state to what extent you may disagree with Dr. Miller's answer in these respects, and what is your per-

sonal experience with the use of the feed-back or tickler coil in connection with the two-electrode tube of the Fleming patent in suit.

A. The so-called regenerative circuits which Dr. Miller states can only be used with the three-electrode tube, are usable with the two-electrode tube.

In the recent demonstrations which I made and in which the oscillating property of the two-electrode tube was shown, the circuit and arrangement used constituted a form of regenerative circuit. I have also used with the two-electrode tube a circuit which is the same as the circuit of "Plaintiff's Exhibit No. 114," with the exception that the grid leak and condenser of this exhibit and the connecting wire thereto, were omitted. When used in this way, the coil j^2 is placed close to the glass tube of the two-electrode device. I produce a sketch (Claimant's Ex. 301) showing these connections.

In this sketch I have used the same designating letters that are used in "Claimant's Exhibit No. 114," and it will be noted that the grid leak L, and the stopping condenser h^3 and the connecting lead are missing. This, of course, is because there is no grid. In the actual arrangement of the apparatus the upper end of the coil j^2 is placed as closely as possible to the space between the cold plate P and the filament F.

Under these circumstances, the electrostatic field existing at the terminus of this coil influences the electron stream in the valve, and this, in conjunction with the feed-back action between the coils j^3 and j^2, results in the production of sustained oscillations.

Mr. Vaill: Counsel for claimant offers in evidence the sketch produced by the witness as "Weagant Sketch No. 7" and requests that the same be marked "Claimant's Exhibit No. 301."

(Said exhibit marked as requested.)

[fol. 1401] Q. 309. Will you now please refer further to Dr. Miller's answer to Q. 20 of his deposition (P. R., p. 799), as it relates to "Plaintiff's Exhibit No. 109," referring to your knowledge of the so-called DeForest "ultra-audion" circuit as being applicable to the two-electrode high vacuum Fleming valve? Also please state your opinion as to what bearing this may have upon the question of whether the

three-electrode valve when used in the "ultra-audion" circuit comes within the technical description of claims 1 and 37 of the Fleming patent in suit.

A. The "ultra-audion" circuit of "Plaintiff's Exhibit No. 109" can be used with the two-electrode tube. I have used this circuit with the two-electrode tube and I produce a sketch (Claimant's Ex. 302) illustrative of this use.

In drawing this sketch I have used the same designating letters as are used on "Plaintiff's Exhibit No. 109." It will be noted that condenser j^3 of this exhibit and the connecting wires thereto, are omitted, since, in the two-electrode tube the grid is missing and there is no necessity for this connection. I have marked this sketch "Weagant Sketch No. 8." The upper part of the coil j^2 in this arrangement is placed close to the walls of the two-electrode tube T, at a point between the plate P and the filament F. Under these circumstances, the electrostatic field existing on this coil affects the electron stream between the two electrodes of the two-electrode tube, and this action, in conjunction with the feed-back action, due to condenser C, results in the tube generating sustained oscillations.

In my use of this arrangement I have received continuous oscillations and produced the well-known beat note, which is the usual indication that the tube is oscillating.

Dr. Miller, in his deposition, in answer to Q. 20, has expressed the opinion that the "ultra-audion" circuit arrangement of the three-electrode tube was a use of the three-electrode tube of which the two-electrode tube was incapable, which statement is not in accordance with the results of the tests which I have made, and which, as stated above, show that the two-electrode tube is entirely capable of operating in the ultra-audion circuit.

It is my opinion that the ultra-audion arrangement of the three-electrode tube comes within the scope of claims 1 and 37 of the Fleming patent in suit.

Mr. Vaill: The sketch produced by the witness as "Weagant Sketch No. 8," is offered in evidence as "Claimant's Exhibit No. 302" and it is requested that it be so marked.

(Said Exhibit marked as requested.)

Q. 310. Referring further to Dr. Miller's answer to Q. 20 (P. R., p. 799), as it relates to "Plaintiff's Exhibit No. 116," will you please state your opinion as to whether the

three-electrode valve, when used in the circuits illustrated [fol. 1402] in said exhibit, when correctly drawn, as testified to by Mr. Clark in answer to Q. 78 of his deposition (P. R., p. 1507), comes within the technical description of claim 1 of the Fleming patent in suit, in view of your present knowledge of the oscillating property of the high vacuum two-electrode Fleming valve, in addition to the oscillating property of the valve, as testified to by Mr. Waterman in answer to Q. 2 of his deposition of December, 1922, in this case (P. R., pp. 259-261)?

A. "Plaintiff's Exhibit No. 116" shows four circuit arrangements in which the three-electrode tube is connected for the purpose of generating oscillations for radio-transmitting purposes. While these circuits differ in detail, their object and result is the same, viz., to enable the three-electrode vacuum tube to produce electrical oscillations of the sort suitable for radio transmission.

As has already been testified to by Mr. Waterman, and further proven by tests which I have made in this case, the two-electrode vacuum valve also possesses the capability of producing oscillations suitable for radio transmission. "Plaintiff's Exhibit No. 116," therefore, does not disclose any fundamental mode of operation of the three-electrode tube which is not also possessed by the two-electrode tube, and I am of the opinion that the arrangements disclosed in this exhibit come within the technical description of claim 1 of the Fleming patent in suit.

Q. 311. Referring again to "Plaintiff's Exhibit No. 85," three-stage amplifier used by the Navy, will you please state how the operation of such amplifier, when combined with an audion detector circuit, as indicated in the evidence of Mr. Clark, referred to by Dr. Miller, compares with the operation of the apparatus of "Plaintiff's Exhibit No. 110," as previously set forth by you herein?

A. Dr. Miller, in his answers to Qs. 19 and 22 of his deposition, criticizes Mr. Waterman's treatment of "Plaintiff's Exhibit No. 85," because he disagrees mainly to a consideration of "Plaintiff's Exhibit No. 110," which I have already considered. This treatment of "Plaintiff's Exhibit No. 85" by Mr. Waterman is fully justified by the testimony of Mr. Clark, who stated that the amplifier of "Plaintiff's Exhibit No. 85" was used by the Navy with an audion detector tube. Mr. Clark does not state, as a matter of fact, that the use of this amplifier was customary with a crystal

detector, although he does state that it had been used to some extent with a crystal detector.

My consideration of "Plaintiff's Exhibit No. 110," in which I give reasons for the arrangement therein shown as coming within the Fleming patent, applies with equal force to "Plaintiff's Exhibit No. 85," when used with an audion detector, or a crystal detector. In other words, the whole combination of a detector with the amplifier of "Plaintiff's Exhibit No. 85" constitutes as a whole a detector, as previously set forth by me.

Q. 312. In Q. 17 (P. R., p. 786), Dr. Miller was asked to compare the operation of the two-electrode valve detector in the circuit of the Fleming patent with the operation of the three-electrode vacuum tube or audion in the P. N. circuit of "Plaintiff's Exhibit No. 83." Will you please state wherein you differ with Dr. Miller in his conclusions in answer to that question, giving your reasons for your [fols. 1403-1404] opinion?

A. Dr. Miller, in his answer to Q. 17, distinguishes between the operation of the three-electrode tube in the P N circuit, which he states is essentially the same as "Plaintiff's Exhibit No. 83," and the two electrode device on the basis that the three-electrode tube is an amplifier, while the two-electrode tube is not.

As I have already shown by my demonstration, the two-electrode device is also an amplifier and Dr. Miller's distinction, therefore, is not in accordance with the basic facts. He also, at the top of page 791 of the Printed Record, expresses the opinion that in the Fleming circuit the power actuating the indicator must be less than the power of the received signals, while in the P N circuit this power may be greater than that of the received oscillations.

This, of course, is merely another way of stating that the three-electrode device is an amplifier, while the two-electrode device is not and, as already pointed out, the facts of actual operation are otherwise.

Q. 313. When, where and under what circumstances did you first receive wireless telegraph signals with the two-element bulb acting as an oscillating heterodyne, and what kind of wireless signals were received?

A. My recollection is that I first used a two-electrode Fleming valve as a heterodyne receiver of continuous oscillations while I was experimenting with the two-electrode valve for the purpose of determining its capabilities. My

recollection is that this was during the latter part of 1914, or the early part of 1915. These experiments were conducted at the laboratory of the Marconi Wireless Telegraph Company of America at Aldene, New Jersey, and I remember that signals were received from a continuous wave station at San Francisco, California.

Q. 314. From your personal experience and expert knowledge of the radio art, can you state about the time when it first became known that vacuum tubes had the capacity to oscillate and whether it was in using such tubes as detectors that such capacity to oscillate was first discovered?

A. My first knowledge of the fact that a vacuum tube would oscillate was derived from experiments which I conducted in February of 1914, with the three-electrode vacuum tube, during which time I discovered that the vacuum tube would oscillate. The vacuum tubes, when this discovery was made, were being used as receivers of wireless signals. I later found out that this same property had been discovered some time in 1912 or 1913 by other workers, and it is my understanding that the three-electrode tube, when this discovery was made, was being used as a detector of wireless signals.

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[fol. 1405] Cross-examination.

By Mr. Edwards:

X Q. 315. Do you own, or have you owned, any stock, securities or obligations of the claimant, Marconi Wireless Telegraph Company of America?

A. I have at times owned a few shares, but do not at the present time.

X Q. 316. For how long has it been since you have owned stock of the claimant?

A. As near as I can recollect, I sold my Marconi stock about the time the Radio Corporation was organized; some time in 1919, I think.

X Q. 317. Are you a stockholder in the Radio Corporation?

A. I own a few shares of the stock of the Radio Corporation.

X Q. 318. That corporation took over the business and physical assets of the claimant company, did it not?

A. It did.

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[fol. 1406] XQ. 332. Will you refer to your diagram, "Claimant's Exhibit No. 225" opposite printed page 1527 of the record? Do you consider that this diagram, as it stands, and not accompanied by explanation, would indicate to one skilled in the art that the two-electrode tube is placed close to the condenser R?

A. As is usual in wiring diagrams, the physical arrangements and their relation to one another, are not particularly indicated. The diagram as it stands, does not show the actual physical relation existing between the condenser R and the two-electrode valve *a*.

XQ. 333. May we assume that your answer to the last question is in the negative?

A. The answer appears to me to state clearly that the wiring diagram of "Claimant's Exhibit No. 225" does not show the physical relation or proximity of the two elements referred to in your question.

XQ. 334. Can you answer either of my last two questions categorically?

A. The categorical answer to XQ. 332 is that the diagram does not indicate that the two-electrode tube is placed close to the condenser R.

XQ. 335. The diagram referred to could have been to so indicate proximity of condenser R and two-electrode tube, could it not?

A. The diagram could have been so drawn that condenser R and the two-electrode tube would have been in proximity to each other.

XQ. 336. Is it not a fact that you could not have demon-
[fols. 1407-1408] strated the capability of the hard or high vacuum two-electrode tube to oscillate, unless you placed the condenser R sufficiently close to the tube to influence the electrostatic field of the tube?

A. The same result would have been secured if the top portion of the coil *m* were placed close to the two-electrode tube.

XQ. 337. It is not a fact that you could not have made the hard or high vacuum two-electrode tube to either amplify or oscillate in any circuit, unless you had placed either a condenser corresponding to R, or an inductance corresponding to *m*, sufficiently close to the tube to influence the electron space in the tube?

A. As I have pointed out in my testimony relative to the demonstration of the amplifying and oscillating action of

the two-electrode high vacuum tube, the association of a suitable electrostatic field on either a coil or a condenser, or connecting wires, was necessary for the action of the device.

X Q. 338. Will you answer my last question categorically, if you can do so?

A. The answer which I have already given seems to be as near a categorical answer as I am able to make.

X Q. 339. Then may I take it that you answer to X Q. 337 is in the affirmative, and if so, that I can pass on to another subject?

A. The answer is in the affirmative in so far as the particular circuit arrangement used in my demonstration is concerned. I am not able to either affirm or deny the statement contained in your question, in so far as other circuits may be concerned.

X Q. 340. It was also necessary, was it not, for you to employ in your demonstration a battery in the plate filament circuit of the tube, for example, the battery P in "Claimant's Exhibit No. 225", opposite P. R., page 1527, and the battery unmarked, but indicated at 180 volts in "Claimant's Exhibit No. 227", opposite P. R., page 1529?

A. With the particular tube used in the demonstration, the batteries referred to in your question were necessary.

X Q. 341. You do not find any such batteries shown in the Fleming patent in suit, do you?

A. I do not find a separate battery for applying a voltage between the space between the plate or cold electrode and the filament or hot electrode. There is, however, a battery shown for the purpose of heating the filament, which is also capable of being used for the purpose of applying a potential between the cold electrode and the hot electrode.

X Q. 342. If, in your demonstrations, you had omitted the battery P in "Claimant's Exhibit No. 225" and the 180-battery in "Claimant's Exhibit No. 227", how would you have adapted the filament battery *h* in "Claimant's Exhibit No. 225", 12 volts in "Claimant's Exhibit No. 227", to create a difference of potential between the plate and the filament?

A. A difference of potential between the plate and filament in "Claimant's Exhibit No. 225" could have been created by omitting battery P, and connecting the circuit containing the telephone *l*. to the positive side of the fila-

ment *b*; similarly, in "Claimant's Exhibit No. 227" a difference of potential could have been created by omitting the battery marked 180 and substituting simply a direct-connection between the points marked "Plus" and "Minus", these particular plus and minus points being these indicated in the diagram by the figures 180 and the arrows.

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[fol. 1409] X Q. 366. In your answer to Q. 265 you say that your tests have shown conclusively that the *real feature* of the vacuum tube amplifier is the electron stream originating in the hot electrode and flowing to the cold electrode, etc. Are we to understand from this that unless there is an electron stream originating in the hot electrode and flowing to the cold electrode, the tube cannot amplify?

A. It is correct to say with reference to either the two-electrode or three-electrode valve, that the electron stream [fol. 1410] flowing between the cold electrode and the hot electrode is essential in order that the device may amplify.

X Q. 367. Do you agree that in order to have an electron stream flowing from the hot electrode, or filament, to the cold electrode, or plate, suitable for amplification, there must be a difference of potential between the filament and the plate?

A. Yes, I agrge.

X Q. 368. Do you find any statement in the Fleming patent in suit, to the effect that there is, or should be, such difference in potential?

A. The patent indicates that the device is to be used for the reception of radio signals, which radio signals produce a difference of potential between the cold electrode and the hot electrode.

X Q. 369. Can you answer my question with a simple affirmative or negative?

A. Yes, I can state that the Fleming patent in suit gives instructions which, when followed, result in the application of a potential between the cold and hot electrodes.

X Q. 370. Will you read any statement that you can in the specification of the Fleming patent in suit, indicating that there is, or should be, any difference of potential between the filament and the plate, other than the potential of the incoming signal itself?

A. There is no instruction that I am aware of, in said patent, directing the application of a potential other than that due to the signal between the two electrodes.

X Q. 371. It is therefore a fact, is it not, that, in order to carry out your so-called demonstrations of amplifying and oscillating two-electrode tubes, you were forced to create a difference of potential between the filament and the plate, and that you do not find any statement in the Fleming patent in suit that there is, or should be, such difference of potential?

A. In the demonstrations which I made, it was necessary, as I believe I stated in my direct testimony, to employ a source of potential between the cold electrode and the hot electrode, in the form of a battery. It is also correct to say that the Fleming patent in suit does not specifically direct the employment of this particular battery.

X Q. 372. Is not the statement in my X Q. 371 substantially correct?

A. Yes, it appears to be correct.

X Q. 373. Do you find any statement in the Fleming patent in suit to the effect that the potential of the plate controls the flow of electrons?

A. The question as it stands is not quite definite, but I assume "by potential of the plate" is meant difference of potential between the plate and the hot electrode, which is the only potential on the plate having a physical significance. The patent, lines 53 to 60, inclusive, states as follows:

"I have discovered that if two conductors are enclosed in the vessel in which a good vacuum is made, one being heated to a high temperature, the space between the hot and cold conductors possesses a unilateral electric conductivity, and negative electricity can pass from the hot conductor to the cold conductor, but not in the reverse direction."

This is a statement to one skilled in the art that an electrical current will flow through the device in one direction, upon the application of an alternating potential.

X Q. 374. Do you find any statement in the Fleming pat- [fol. 1411] ent in suit descriptive of an electrostatic field, or other means, for controlling the electron stream in the manner in which you used such electrostatic field to control the electron stream in your so-called demonstrations?

A. I do not find an instruction directing the employment

of an electrostatic field in the particular way in which I employed it in my demonstrations.

X Q. 375. Do you find the application of an electrostatic field, or other means, for controlling an electron stream, in substantially the same way as you used it in your so-called demonstrations described in the Fleming patent in suit?

A. I do not find the particular method of using the electrostatic field which I used, described in said Fleming patent.

X Q. 376. Do you find described in said Fleming patent any means for controlling an electron stream analogous to the means employed by you for controlling the electron stream, in your so-called demonstrations?

A. Yes, I think I do.

X Q. 377. Will you read on the record any part of the Fleming patent in suit which describes such means?

A. Fig. 1 of the patent is a good description of such a means, since it shows a circuit between the hot and cold electrodes, containing an inductance coil associated with an antenna circuit, radio signals set up electrical currents in the antenna, which induce potentials in the coil *k*, and the result of these potentials is to set up an electrostatic field between the hot electrode and the cold electrode, which varies in the same manner as the signal current, and which, as the result of the inherent and well-known characteristics of this device, produce variations in the flow of electrons from the hot to the cold electrode. This is an electrostatic control of the electron stream which appears to me to be analogous to the method employed in my tests.

X Q. 378. Do you mean to say that the coil *k* of Fig. 1 is located so close to the tube that the electrostatic field created thereby would act upon the electron space in the tube in the same way that the electrostatic field of the coils or condensers located adjacent to the tubes acted upon the electron stream in such tubes in your so-called demonstrations?

A. No, I do not.

X Q. 379. Will you, then, explain how the electrostatic field between the hot and cold electrodes in Fig. 1, is varied by the potentials in coil *k*?

A. The variation results from the fact that one terminal of this coil is connected to the hot electrode and the other terminal is connected to the cold electrode.

X Q. 380. And the incoming signal is relied upon to create the same difference of potential between filament and plate as exists in the signal itself, is it not?

A. The signal is responsible for the variable potential applied between filament and plate.

X Q. 381. That is to say, the difference in potential between filament and plate could be no greater than the difference in potential in the incoming signal current itself in Fig. 1; is not that the fact?

A. That is not quite a correct statement; it is correct in so far as the variable potential is concerned, but due to the fact that coil *k* has one terminal connected to one side of the battery *h*, near the point *e*, there is also a fixed difference of potential existing between the two electrodes, so that, in the actual use indicated by Fig. 1 of the patent, the total potential will always be the sum of this fixed potential and the variable potential.

[fol. 1412] X Q. 382. Is it not a fact that in Fig. 1 the fixed potential created by the battery acts in opposition to the difference of potential in the signal itself, so that the total potential is, in fact, the incoming variable potential minus the fixed potential?

A. No, that is not quite correct, because the fixed potential is in one direction only, and the signal potential, since it is alternating, is in both directions; consequently, one-half of the incoming oscillation adds to the potential of the battery, while the other half subtracts from it.

X Q. 383. In preparing for your demonstrations, or otherwise, did you set up any of the hard vacuum tubes in the circuit of Fig. 1 of the Fleming patent in suit, to ascertain if any of the tubes would amplify or oscillate in that circuit?

A. The circuit actually used in the demonstrations was substantially the circuit of Fig. 1, the variations therefrom being simply such variations as one skilled in the art would naturally make, and consisted of the insertion in the antenna circuit *n, m, o* of a variable condenser for convenience in tuning and demonstrating. This circuit, of course, was also used in preparation for the demonstrations.

X Q. 384. Will you answer my question in the affirmative or negative, if you can do so?

A. Yes; my answer is affirmative.

X Q. 385. Meaning the identical circuit as shown in Fig. 1 of said Fleming patent?

A. In my understanding of the term "circuit", yes.

X Q. 386. Do you admit that the hard vacuum tubes used by you in your demonstrations would not amplify or oscillate if connected in the circuit shown in Fig. 1, without additions or modifications?

A. Before answering the question, I shall have to ask one, viz.: Does your question mean without additions to the circuit, or without additional elements contained in the circuit? There is a distinction there, to the technical man, which makes this question necessary.

X Q. 387. I have in mind particularly, without adding other means; such, for example, as a means for creating an electron stream by creating a difference of potential between filament and plate, and means, such as additional electrostatic means, for controlling the electron stream between filament and plate.

A. I believe that I can now answer the question by stating that the two-electrode tube will oscillate in the identical circuit shown in the Fleming patent in suit, when a source of potential such as the battery, is included in the circuit between the hot and cold electrodes. I do not consider that the arrangement of coil or condenser (particularly the former), adjacent to this electron space of the tube, constituted any additional means not shown in the patent, but merely a particular arrangement of the elements disclosed in the patent.

X Q. 388. But you admit, do you not, that in order to make your demonstrations, you were forced to do at least two things not shown in Fig. 1? First, the insertion of a battery in the circuit between the hot and cold electrodes and, second, the location of the coil *k* close to the tube?

A. I would not say that I admit it, but rather, that I affirm it, since I clearly stated, as I recollect it, both of these facts in my demonstrations.

X Q. 389. Is there any commercial use of the two-electrode tube controlled by an electrostatic field, as set up in your demonstrations?

A. None that I know of.

X Q. 390. When did the Marconi Company begin the commercial use of the quenched-gap?

A. About 1912, as I recollect.

[fol. 1413] X Q. 391. Since that time the major part, or substantially all of its installations, have employed the quenched-gap, have they not, down to the present time?

A. I can only answer that question to about 1920, and it is my recollection that most of the ships' sets installed between 1912 and 1920 were of the quenched-gap type. The quenched-gap transmitter was not employed in a considerable number of the Land Stations. Since 1920 I have not been in touch with the subject, and have no information.

X Q. 392. What type of transmitter spark-gap was employed in the Marconi land stations up to the time you left?

A. A number of them had rotary gaps.

X Q. 393. When did the Marconi Company begin the commercial use of the three-electrode vacuum tube?

A. My recollection on that is uncertain, but I believe it was prior to 1917.

X Q. 394. Can you say approximately how long before 1917?

A. Not earlier than 1914.

X Q. 395. And to what extent was the three-electrode vacuum tube in commercial use by the Marconi Company in 1920?

A. It was in use, I believe, in most of the land stations, and was beginning to be used on shipboard.

X Q. 396. Was the use of the lateral or local circuit, as indicated, for example, in the Marconi patent 763,772, begun during, or prior, to the time you were connected with the Company?

A. It was in use prior to my connection with the Marconi Company.

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[fol. 1414] X Q. 399. Referring again to Fig. 1 of the Fleming patent in suit, if a battery be inserted in the circuit between the plate and filament, will the statements in your answers to X Qs. 380 to 382, inclusive, still apply?

A. Yes.

X Q. 400. Will you refer to the "Plaintiff's Exhibit No. 85", drawing of DeForest Audion Amplifier, opposite page 225 of the printed record? Assuming an alternating voltage to be applied at the terminals X Y, what, if any, change will be made in this incoming voltage or current between the input terminals X Y and the telephone R?

A. That will depend, of course, on a great many things, such as the frequency of the applied electromotive force, the construction of the transformers and the characteristics of the particular tubes employed. The question is so

general that I have difficulty in answering it and, possibly, it might be simplified if the sort of change you are interested in inquiring about were specified.

X Q. 401. How will the frequency of the incoming current affect the operation?

A. It will affect it, because the drawing indicates transformers with iron cores, and transformers of this kind will pass a certain order of frequency; that is, a relatively low one, in an efficient manner, while the higher frequencies will not be passed so efficiently. They may also introduce distortions.

X Q. 402. Assuming the transformers to be adapted to pass such frequency as is received, and assuming the tubes to be hard tubes what, if any, change is made in the incoming signal or voltage between the input X Y and the telephone R?

A. Several changes take place: first, the intensity of the applied electromotive force is increased with each stage; secondly, in the arrangement shown there will be a distortion of the applied e.m.f., resulting in the production of a direct-current component in the output of each tube, this direct-current component being increasingly greater from the first tube to the third tube; there will also be produced harmonics, or multiples, of the applied e.m.f.

X Q. 403. If the incoming signal or frequency is a low frequency telephone conversation, for example, do the harmonics and distortion referred to in your last answer, tend to interfere with, or make clearer, the telephone conversation?

A. It may do either; depending on the characteristics of the transformer and the receiver. For example, the transformers may pass rather poorly the higher frequency com- [fol. 1415] ponents of the voice, which failure may, in part, be compensated for by the production of the harmonics; on the other hand, if the transformers are well designed for this purpose, and the telephone receiver equally responsive over the whole ranges of voice-frequencies, then the presence of the harmonics may have a detrimental effect; this condition, however, is one which is realized only with great difficulty in practice.

X Q. 404. Assuming the drawing to represent a low-frequency amplifier, do you find in it any suggestion that either of the three tubes is to perform any rectifying function?

A. Yes, of course; the tubes are three-electrode vacuum tubes and the space between either the cold electrode tubes and the hot electrodes is a rectifier, and the whole action of the tubes hinges around this fact.

XQ. 405. To the extent that rectification occurs in any of these three tubes, still assuming it to be used as a low-frequency amplifier, for telephone currents, for example, does, or does not, the rectification tend to improve or impair the reception in the telephone?

A. That is a difficult question to answer, for the reason that the three-electrode device operates as it does, because inherently it is a rectifier, and the question of whether or not rectification improves, or injures, the transmission of speech, can only be answered by stating that it is present, regardless of whether the transmission is good or poor.

XQ. 406. Now refer to "Plaintiff's Exhibit No. 110", opposite printed page 227 of the record, and assume that high-frequency radio signals are received on the antenna A E. Do you agree that the first tube T will act to rectify the high frequency and create a low-frequency component in the plate circuit of the first tube?

A. I certainly agree that the first tube acts as a rectifier, but whether or not it creates a component of low frequency in the output, depends upon the nature of the applied electromotive force; consequently, I cannot agree with that part of the question, unless the input electromotive force is more completely defined.

XQ. 407. Assuming the incoming wave to be a modulated, continuous wave, how do you understand the first tube T will function?

A. It will function in several ways; first of all, it would repeat in the output circuit the incoming modulated electromotive force; it would give rise to harmonics of that electromotive force and also to frequencies corresponding to the frequencies of modulation.

XQ. 408. Now, do you agree that the object and purpose of the remaining two tubes is to pass on the modulated frequency with increased intensity?

A. That is a portion of their purpose, but their fundamental purpose is to make evident in the telephone receiver R, the presence and modulation of the high-frequency radio currents existing in the antenna, A, J', E; in other words, in combination with the first tube, they function to detect the presence of a high-frequency radio current.

XQ. 409. I am trying to find out if the word "detect", as used by you in your deposition, and particularly in your last answer, has any significance other than enabling sounds in the telephone R to be detected, and to that end, whether or not in the last answer, you mean anything more than that the first tube has acted to rectify the incoming signal, and the last two tubes to intensify the rectified signal in the form it exists in the grid circuit of the second tube?

[fol. 1416] A. I am using the word "detect" in the sense in which it has been employed for many years in radio, and in the sense in which it is used in the definition of the word detector, given at one time by the Standards Committee of Radio Engineers, and given in my direct deposition. Under this significance, the complete arrangement of "Plaintiff's Exhibit No. 110" constitutes a "detector", since its purpose is to make evident the presence of high-frequency radio oscillations by means of sound in the telephone receiver.

XQ. 410. I do not think that you answer my question and I will therefore try it in another way: Assume, for example, that there is applied to the input circuit, the grid circuit, of the last tube T^3, a low-frequency telephone current, but that the energy is not sufficient to enable a person to hear sound in the telephone receiver R. Under those circumstances do you understand that the device is acting as a detector?

A. Assuming that your question supplies the entire information necessary for its answer, it would then appear that the last tube was not performing *any* useful function; and I would not be able to say from this information alone, whether or not it was a detector.

XQ. 411. What is the significance of the reservation which you make in answering the last question?

A. The significance is that the information given in your question imputes an inoperative device, and it is not possible to apply a characterization.

XQ. 412. In what respect would such a device be inoperative, other than that the listener would not hear sound?

A. It would be inoperative because the purpose of the arrangement shown in "Plaintiff's Exhibit No. 110" is to produce a sound in the telephone, and if it fails to do this, it is not working.

XQ. 413. Now, assume that without any other change,

the energy of the received telephone conversation at the input of the tube is increased until a listener does hear sound in the telephone; under those circumstances is the device acting as a detector?

A. That will depend upon the source of the applied e.m.f. If it be derived from the decomposition of a radio frequency into audio components, then, under my understanding, it would be acting as a detector; in other words, it would be part of a mechanism by means of which a frequency so high as to be inaudible to the human ear were acted upon to produce frequencies which could be heard. On the other hand, if the applied e.m.f. is an audible one, and not derived from a radio frequency, I would say that while the device itself, viz., tube T^3 of Plaintiff's Exhibit No. 110" is a detector, under these circumstances it was not operating as a detector under my understanding of the term.

X Q. 414. Then, if the low-frequency telephone current applied to the input of the tube is derived from a telephone circuit, as I understand you, the tube is not operating as a detector; am I right?

A. That is correct; understanding, of course, that the tube itself is still a detector in its inherent capabilities.

X Q. 415. But the same tube, if deriving low frequency from a tube such as the first tube in "Plaintiff's Exhibit No. 110", which acts to create the low frequency from the incoming high frequency, is acting as a detector; is that correct?

A. Yes; that is to say, it is then part of the detecting mechanism, since it is assisting in the process of rendering the inaudible high-frequency radio oscillations audible.

[fol. 1417] X Q. 416. Now, assume, for example, that the antenna and the first tube of "Plaintiff's Exhibit No. 110" constitute a radio receiving station at New York, for example, receiving trans-atlantic wireless communications, and that in place of the second tube, T^2, and its grid and plate circuits, there is a telephone line receiving the signals as they exist in the plate circuit of the first tube and extending to another location, Chicago, for example, and that the telephone line is inductively connected to the third tube, T^3, at the distant location, Chicago, for example, under those circumstances is the tube T^3 acting as a detector, or not?

A. It would seem to me that it was still a part of the detecting system, serving the general purpose of making the radio-frequency wave satisfactorily audible.

X Q. 417. Please let me remind you that I did not ask you whether or not the tube would be part of a "detecting system", but that the question asks if, under these circumstances, the tube is acting as a detector within your definition.

A. I thought I had made clear my point of view, namely, that I considered the whole system to constitute the detector; that I considered the individual tube itself to be a detector, and functioning in the manner specified in your question, to be performing its portion of the detecting action.

X Q. 418. What portion of the detecting action do you understand that the tube T^3 is performing under these circumstances?

A. It is assisting in the process of making the original radio frequency oscillations audible.

X Q. 419. Can you not explain what particular part or function this tube performs in the process of making the original radio-frequency oscillations audible?

A. Certainly; it is increasing the audio-frequency applied to it and derived from the radio-frequency signals.

X Q. 420. In your answer to Q. 225 you say that practically the entire British Merchant Marine was equipped with the open-gap transmitter, and that it was employed on most of the Naval ships of the World, with the exception of the German Navy. Have you personal knowledge of these facts?

A. I presume I may say that I have personal knowledge of these facts, since, during the period when I was Chief Engineer of the Marconi Company, I possessed extensive sources of information relative to the use of radio in the world generally.

X Q. 421. But you do not pretend to have actually visited the entire British Merchant Marine or most of the Naval ships, do you?

A. No, certainly not.

X Q. 422. From such information as you had, do you understand that practically the entire British Merchant Marine was, during all of the period concerning which you have testified, equipped with apparatus made and sold by the British Marconi Company?

A. Yes, I understood that to be a fact.

X Q. 423. And that since in or about 1912, these same ships have, in increasing numbers, been provided with quenched-gap apparatus?

A. If that is a fact, I was not aware of it.

X Q. 424. Did you send, or attend, at either transmitting or receiving stations the alleged transmission of messages "some 6600 miles", referred to in your answer to Q. 225?

A. No, I did not.

X Q. 425. What, if any, personal knowledge have you of the alleged fact that war communication was maintained [fol. 1418] between England and Russia, as well as ships at sea at great distances from England, as stated in your answer to Q. 225?

A. I have first-hand personal information on that point, resulting from the fact that I personally listened to large numbers of such messages being transmitted.

X Q. 426. Where were you when you heard these messages?

A. At various places; for instance, Belmar, New Jersey, Lakewood, New Jersey, Miami, Florida.

X Q. 427. In your answer to Q. 225 you referred to a station of the United Fruit Company, at New Orleans. Do you recall that there was some trouble or complaint in connection with that station?

A. Yes, lots of trouble.

X Q. 428. To such an extent that the United Fruit Company refused to pay for it, did it not?

A. That I don't know.

X Q. 429. Do you make any question as to the fact that in the alleged demonstrations before Judge Mayer, such oscillations by the two-electrode vacuum tube as was demonstrated to Judge Mayer, was due to the presence of residual gas in the tube?

A. I don't know that I would say that the oscillations were due to the gas, but simply that the residual gas played a part in the action.

X Q. 430. Will you go so far as to say that the tubes would not have oscillated in the circuits as demonstrated before Judge Mayer, if the tubes had been high-vacuum tubes not containing residual gas?

A. I cannot say this as the result of actual experience, although I believe that it is probably true.

XQ. 431. You mentioned in your direct testimony, if I recall correctly, that the tests before Judge Mayer occupied about half an hour; am I correct in this?

A. I am not sure just what I stated in my testimony, but that corresponds with my recollection at the moment.

XQ. 432. For how long a period did the tube continue to oscillate without interruption during these tests?

A. I do not seem to recollect.

XQ. 433. In your answer to Q. 245, you state that in the demonstrations before Judge Mayer the essential thing was simply to demonstrate that the two-electrode tube had the capability of generating high-frequency oscillations, and that it was your understanding that this was in accordance with the wishes of the Court. Please state how your understanding of the wishes of the Court was acquired.

* * * * * * *

Mr. Edwards: The complete sentence referring to this matter in the answer to Q. 245, is as follows:

"It was, furthermore, my understanding that this was in accordance with the wishes of the Court and that the question of reliability, or otherwise, was not involved."

* * * * * * *

A. My information, as I recollect it, was received from either Mr. Betts of the firm of Sheffield & Betts, or Mr. Waterman.

XQ. 434. You have no personal knowledge that the Court had any wish, of any kind, have you?

A. I received no direct personal information from the Court.

* * * * * * *

[fol. 1419] XQ. 435. When you gave testimony in the Kilbourne & Clark case, did you bring to the attention of the Court where, in your testimony, the criticisms which you have set forth here against the Lodge Fig. 4 and the Telsa demonstrations which you set forth in your deposition in this case, particularly in your answers to Qs. 208 to 211, inclusive?

A. It is my recollection that I testified in the Kilbourne & Clark case relative to experiments performed with apparatus arranged in the manner of Fig. 4 of the Lodge patent, and also that I testified relative to experiments performed with the so-called Tesla coil. I did not, in that case, testify

as an expert, but as a fact witness, and do not recollect having given testimony relative to the Lodge and Tesla patents, and the disclosures therein.

X Q. 436. Do you mean that you did not, in the Kilbourne & Clark case, express your opinion as an Expert as to whether or not the Lodge Fig. 4 and Tesla apparatus demonstrated by Prof. Pickard, corresponded with the disclosure of the Lodge and Tesla patents respectively?

A. It is my recollection that I identified the apparatus demonstrated by Mr. Pickard with the Lodge Fig. 4, with the exception of the coil *k*, and that I pointed out that this particular coil had not been constructed by Mr. Pickard in accordance with my understanding of the instructions of the patent.

With respect to the so-called Tesla apparatus, I recollect that I pointed out that it had not been constructed in accordance with the instructions of the patent and was not being used in a manner corresponding to my understanding of the instructions of the patent.

X Q. 437. Have you made any criticisms of the Lodge Fig. 4, or the Tesla apparatus, in this case, which you, or Mr. [fol. 1420] Waterman did not point out to the Court in the Kilbourne & Clark case?

A. I do not recollect, so far as my own testimony is concerned, having pointed out relative to the actual tests of Fig. 4 of the Lodge patent made in the Kilbourne & Clark case, and the so-called Tesla apparatus demonstrated in the same case, which I did not point out in my testimony in the Kilbourne & Clark case. I have, of course, made many references and statements relative to these two arrangements in my present testimony, which I do not think were contained in my Kilbourne & Clark testimony. I do not recollect Mr. Waterman's testimony sufficiently well to be able to answer the question.

X Q. 438. Can you enumerate briefly the substance of each of the references, statements or criticisms which you have made in this case relative to the Lodge Fig. 4, or the Tesla patent, which you believe were not contained in the Kilbourne & Clark testimony?

A. No, I don't think that I could do that without a detailed comparison of my testimony in the two cases.

X Q. 439. As I understand your testimony, your principal criticism of the Lodge Fig. 4, as set forth in your

answer to Q. 208, is that if coil k were constructed in accordance with information to be found in the Lodge specification or drawings, it would short-circuit the condensers j, j, and, therefore, make the transmitter inoperative; am I correct in that understanding?

A. No, I don't think that you are, because while I have pointed out in my answer to Q. 208 that the coil k constructed in accordance with any information I can find in the patent, renders the device inoperative; as I read my answer to Q. 208, that is only a portion of my criticism of Fig. 4 and the balance of my reply brings out the facts which I consider equally important.

X Q. 440. The criticism that coil k would short-circuit the condensers is based upon your understanding that coil k should be a high-frequency coil of a very few turns, is it not?

A. My understanding of the construction of coil k is based on what the drawing Fig. 4 shows, since, as I recollect, there is no other information in the patent, except a statement to the effect that coil k should permit the thorough charging of the condensers, which statement implies that coil k should offer a negligible opposition to the flow of current from the induction coil a into the condensers j.

X Q. 441. I am not asking you as to the correctness of your belief; but, rather, whether it is not a fact that your criticism that coil k would short-circuit the condensers is based upon an understanding that coil k should be a high-frequency coil with very few turns?

A. The question contains an expression "high-frequency coil", which is somewhat indefinite in meaning, but I will state that it is my understanding that the patent directs that coil k should be constructed of relatively few turns.

X Q. 442. In your answer to Q. 209 you say that on substituting for the coil k as set up by Prof. Pickard, a coil made as nearly as possible like the disclosure of the patent, the device was found to be entirely inoperative. Will you describe what the coil which you considered to be "as nearly as possible like the disclosure of the patent", was like?

A. The answer to that question will probably be found in my testimony in the Kilbourne & Clark case. I do not, at the moment, recollect the exact details, except that it was a small coil with a few turns of fine wire.

[fol. 1421] X Q. 443. Was there any finding by the Court

that the device with this substituted coil was entirely inoperative?

* * * * * * *

A. I do not recollect the Court's opinion sufficiently well to answer the question.

X Q. 444. Who did find the device to be entirely inoperative?

A. The device was tested, as I recollect it, by Mr. Waterman and myself, in the presence of representatives of the defendant in the Kilbourne & Clark case, and also in the presence of expert engineers appointed by the Court and designated as "Assessors".

X Q. 445. Did the assessors find that the device with this substituted coil was entirely inoperative?

A. I do not recollect their report, but presume it is to be found in the Kilbourne & Clark record. I recollect most distinctly, that the arrangement was inoperative under these conditions, and this fact must have been evident to those present.

X Q. 446. In your answer to Q. 227, you refer to three types of transmitters involved in the Kilbourne & Clark case; will you state in each of these transmitters how many swings of current there were in the primary circuit between the time the spark-gap closed and the time it opened again?

A. No measurements were made upon which an exact answer to your question can be based. The Achilles set was of a type commonly and extensively used, and I can state that, based upon my experience with similar apparatus, I believe that about ten oscillations took place in the closed circuit for each spark. Oscillograms were made by Dr. Chaffee, and in his testimony, P. R., p. 2971 in the Kilbourne & Clark case, he states that these tests disclosed at least two and a half complete oscillations. From my knowledge of the method of making these tests, and also from the manner in which the Simpson transmitter tuned, I believe that there were two or three times as many swings as the oscillograph showed. No tests that I am aware of were made upon the Thompson transmitter, but I believe that the number of swings would be somewhere between five and ten.

X Q. 447. Referring to the Marconi patent No. 763,772, do you agree that if, in the system set forth in that patent, the spark-gap is a plain gap, a tight coupling between the

closed circuit and the antenna, it would result in the transmission of a double frequency from the antenna?

A. As I have already pointed out in my direct examination, the production of a complex wave-form involving two or more frequencies, is not determined by the coupling or spark-gap alone, but depends upon the resistance of the circuit as well.

If the antenna circuit is of low resistance, then there will be produced the double frequency form of wave; if the circuit is of high resistance, there will *not* be produced the two humps in the resonant curve. In this statement I am assuming that some particular close coupling is employed which is greater than the open spark-gap can be used with, and cut off at the end of the first series of oscillations in the closed circuit.

X Q. 448. Is the "resistance" to which you refer, ohmic resistance?

A. Yes.

X Q. 449. Does ohmic resistance in a tuned circuit act [fol. 1422] to vary the frequency of the current?

A. To a very slight extent, it does, although this amount is so small that it is usually neglected.

X Q. 450. The effect of ohmic resistance in the antenna, as referred to in your answer to XQ. 447, would be to change the form of the wave by flattening it out, rather than to change the frequency of the wave or waves, would it not?

A. The effect of the resistance in the antenna circuit is two-fold. It has the effect of broadening or flattening the resonance curve of the antenna circuit in the manner implied by your question, but its effect on the production of the double frequency with some particular degree of coupling, is due to a different action, viz., that the energy transferred from the closed circuit is consumed, thereby reducing the amplitude of the current in the antenna circuit and, coincidentally, the power of the antenna circuit to re-ignite the spark-gap by means of retransferring energy back to the closed circuit. This is the action which makes possible the employment of a closer coupling with a high-resistance antenna, when the antenna resistance is high, than is possible when the antenna resistance is low.

X Q. 451. The effect of resistance is to reduce the efficiency of the system, is it not?

A. That depends upon the particular sort of efficiency which is meant. If, by this term is meant the ratio of the

power input into the oscillating circuit containing the spark-gap to the energy radiated by the antenna, and if the resistance referred to is any resistance other than that of radiation resistance, then the efficiency is lowered. If, however, under these circumstances, the resistance is that kind of resistance known as "radiation" resistance, then the converse is true, and the greater the value of this resistance, up to some limiting point, of course, the more efficient the arrangement becomes. Of course, there are other standards of efficiency besides the one I have assumed in my answer; for instance, the common criterion of practical usefulness might be considered, and this is the radiation of the maximum amount of power from the antenna with a reasonable wave-form. Due to the quenching action of all types of spark-gaps, on this basis it may happen that the insertion of resistance into the antenna circuit resulting in a broad single-hump resonance curve, gives a result which is as good as that obtained with a lower resistance, and the necessity of employing looser coupling which results therefrom.

On this basis, therefore, the insertion of resistance in a antenna of a radio system similar to Fig. 1 of Marconi patent 763,772, may not result in a decrease of efficiency.

X Q. 452. Will you explain what you mean by "radiation resistance"?

A. By "radiation resistance" I mean the capability which an open antenna system such as that of Fig. 1 of Marconi patent 763,772 has of radiating energy out into space. This capability is an energy-consuming action and is, therefore, analogous to a resistance, and is commonly measured in the same electrical units as a resistance.

X Q. 453. To the extent that ohmic resistance is present in the antenna, it tends to make the antenna a poor radiator, does it not?

A. No, that is not correct. The presence of ohmic resistance, and by that I assume you mean any resistance except radiation resistance, has no effect whatever upon the radiating power of an antenna. It consumes energy, and for this reason makes it necessary to supply greater power to the antenna, in order to radiate a given power from the [fol. 1423] antenna, but it does not affect the antenna itself, in so far as this radiating capability is concerned.

X Q. 454. To put it another way: It does not vary the frequency to which the antenna is tuned; but does reduce

the energy delivered to the antenna and thereby reduce the energy sent out by the antenna, does it not?

A. The statements in your question are not quite technically correct. The insertion of resistance in ~~the~~ antenna circuit, assuming that we are not concerned with an associated circuit containing a spark-gap, does not materially affect what is commonly known as the natural frequency or period of the antenna circuit.

With respect to the power applied to the antenna, the presence or absence of resistance is not a controlling element, but it is a controlling element in determining the percentage of this power which can be usefully radiated.

X Q. 455. So that increasing the ohmic resistance in the antenna reduces the energy radiated, does it not?

A. That question is not answerable unless other conditions, such as the associated circuits from which the antenna derives its power, are specified.

X Q. 456. Other parts of the system remaining the same, is it not a fact that increasing the ohmic resistance reduces the amount of energy transmitted by the system?

A. Again I must point out that the question does not supply sufficient information to enable me to answer, for the simple reason that with one type of power supply to the antenna, the insertion of resistance under practical operating conditions would not necessarily, decrease the power radiated or the efficiency of the arrangement as a radio transmitter; whereas, with another type, it would decrease it.

X Q. 457. It is a fact, is it not, irrespective of the type of power supplied, ohmic resistance to the extent that it is present in the antenna, tends to dissipate energy?

A. Ohmic resistance in an antenna circuit will always consume or dissipate energy.

Re-direct examination.

By Mr. Vaill:

R. D. Q. 458. In view of opposing counsel's reference to one example of your testimony on which his objection was entered as not being competent, I will refer you to your answer to Q. 224 on direct examination (P. R., p. 1682), where you quoted from the testimony of Dr. Zenneck in the Kilbourne & Clark case, and will ask you whether or

not you personally heard Dr. Zenneck give the testimony which you quoted in said answer.

* * * * * * *

[fol. 1424] A. I was present and heard Dr. Zenneck give the testimony referred to.

R. D. Q. 459. What other case did this testimony of Dr. Zenneck's relate to?

* * * * * * *

A. This testimony was originally given in the suit of the *Marconi Wireless Telegraph Company of America* vs. *The Atlantic Communication Company.*

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[fol. 1425] R. D. Q. 462. In answer to X Q. 446 you stated that oscillograms were made by Dr. Chaffee and in his testimony in the Kilbourne & Clark case he states that these tests disclosed at least two and a half or three oscillations. Will you please state what connection you had with such tests and your personal knowledge of what took place as to methods of procedure and results?

* * * * * * *

A. I was present during these tests and familiarized myself with the apparatus and methods employed in the making of these tests.

R. D. Q. 463. In your last answer you apparently neglected to state your personal knowledge of the results of the tests conducted by Dr. Chaffee; will you please complete your answer?

A. I observed the results obtained by Dr. Chaffee in these tests and noted that they were the results referred to in his testimony, viz., that at least two and one-half oscillations were present in the closed circuit of the Simpson transmitter.

* * * * * * *

Last question by the Notary: Do you know of any other matter relative to this claim?

Answer: No.

Deposition Closed

[fols. 1426-1427] *Deposition of Wilson Aull, Jr., for Claimant, taken at New York, N. Y., on the 9th day of June, A. D.* 1927.

Direct examination.

By Mr. Vaill:

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[fol. 1428] Q. 74. Dr. Miller, during his answering testimony for defendant, stated in answer to Q. 20 of his deposition (P. R., p. 801), referring to the method of reception by the use of the ultra-audion receiving circuit and the regenerative circuit with inductive coupling of "Plaintiff's Exhibits Nos. 109 and 114" respectively, as follows:

"It is apparent, therefore, that this important method of reception is made practically possible solely by reason of the oscillating property of the three-electrode vacuum tube."

Will you please state to what extent you can confirm Mr. Weagant's testimony in answer to Q. 309 (P. R., p. 1771), concerning the use of a two-electrode vacuum tube in the ultra-audion circuit similar to that of "Plaintiff's Exhibit No. 109"?

A. I have set up apparatus similar to that shown in the "Weagant Sketch No. 8," "Claimant's Exhibit No. 302," with which I used a two-element electron tube as shown at T. I also had a filament rheostat connected between the filament heating battery and the filament, so that I could regulate the temperature of filament F. Tube T was so placed that it was near the high potential end of the secondary tuning inductance j_2.

With this arrangement and with proper adjustment of the filament rheostat I had no difficulty in setting up oscillations and receiving C-W signals. I also received whistles caused by the beating of incoming broadcast signals with the locally generated oscillations by the well-known heterodyne action. The beat-note varied with the tuning of variable con-[fol. 1429] denser *h'*. When tuned to a broadcast station in varying the temperature of filament F by means of the rheostat, oscillations could be controlled or stopped altogether. Radio telephone signals were loudest when the circuit was adjusted so it was not quite oscillating; or, if continuous wave-signals were desired, they could readily be received by putting the circuit into the oscillating condition.

* * * * * * *

[fol. 1430] Q. 77. Dr. Miller, in his testimony on behalf of defendant, stated in answer to Q. 4 of his deposition on page 763 of the Printed Record, that:

"Crystals are incapable of generating oscillations, though a great number of crystal contacts have been and are being utilized as radio detectors."

Will you please state what experience you have had in connection with the reception of radio signals when the crystal detector has produced oscillations, and also as to the capabilities of the crystals to produce oscillations. In giving your answer will you refer to one or two publications which your experience may confirm?

A. It is well known in the art that crystal contacts having negative resistance characteristics may be used to generate oscillations in much the same manner that an electric arc is used for this purpose.

I have done some experimentaing with crystals used in circuits similar to those usually employed with the oscillating arc. Such methods have been pointed out in quite a number of publications, including Eccles' "Hand Book of Wireless Telegraphy and Telephony," 1918, page 259; "QST," March, 1920, page 44, and "Radio News," January, 1925, page 1166.

I have experimented with the circuits shown in "QST" and Fig. 3 of Dr. Pickard's article in "Radio News," and have obtained beat notes from a local broadcasting station, upon suitable adjustment of the crystal and potentiometer setting controlling the battery voltage. I obtained this result, using a galena crystal with a spring wire contact, and with a zincite crystal with a steel point.

These beat notes were, of course, due to the beating of local oscillations with the incoming telephone carrier wave. The correct adjustment of crystal and battery voltage is not easy to obtain and in my experience the adjustment did not hold long, but the intermittent beats set up, proved the existence of locally generated oscillations without doubt.

[fols. 1431-1432] Mr. Vaill: Counsel for claimant offers in evidence as one exhibit containing references to Dr. G. W. Pickard's work with Oscillating Crystals, photostat copies of page 44 of the publication "QST" for March, 1920, page 1166 and a portion of page 1270 of the "Radio News" for

January, 1925, and requests that the same be marked "Claimant's Exhibit No. 310."

(Said exhibit marked as requested.)

Mr. Vaill: Opportunity is extended to opposing counsel, for his own convenience and satisfaction, to attend and witness a repetition of the tests made by this witness and testified to in answer to Qs. 71 to 77, inclusive, of the direct examination.

Cross-examination.

By Mr. Edwards:

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[fols. 1433-1434] X Q. 93. Have you ever succeeded in generating oscillations with a crystal, other than one having negative resistance characteristics?

A. As far as I know, I have not. In generating the oscillations, it is almost impossible to tell just what the crystal characteristics are, except for one adjustment, the adjustment at which the characteristic curve is taken. It is theoretically impossible to obtain oscillations from a device such as a crystal or arc, unless it has a negative resistance or falling characteristic.

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Last Question by the Notary: Do you know of any other matter relative to this claim?

Answer: No.

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[fol. 1435] *Deposition of John V. L. Hogan, for Claimant, taken at New York, N. Y., on the 31st day of August, A. D. 1927.*

First Interrogatory by the Notary: State your name, your occupation, your age and your place of residence, whether you have any, and if any, what interest direct or indirect, in the claim which is the subject of inquiry; and whether, and in what degree, you are related to the claimant?

Answer: My name is John V. L. Hogan; age, 37; residence, Forest Hills, Long Island, New York; I have no

interest, direct or indirect, in the matters at issue and am not in any way related to the claimant.

Direct examination.

By Mr. Vaill:

Q. 1. What has been your training and experience in the art of Radio Engineering?

A. I have followed closely the literature and practical art of Radio Telegraphy and Radio Telephony continuously since I began as an amateur in 1904, or earlier, and I have carried on much experimental, research and engineering work in that field. I have designed, manipulated, constructed and investigated a great variety of apparatus for radio telegraphy and radio telephony.

I have directed and performed experimental investigation in the development of radio receivers in general, especially with regard to tuning and the prevention of interference, as well as of transmitting apparatus of various types, including numerous forms of spark and continuous-wave transmitters.

Work at my own stations, prior to 1906, brought me in touch with Dr. Lee DeForest, and for about six months in 1906 and 1907, I worked with him as his personal assistant at his laboratories in New York. In 1907 I continued my investigations in radio telegraphy and telephony, the tuning of receivers and transmitters, etc., both at the commercial radio station at Bridgeport, Connecticut, and at my own station. This work led to several patent applications of mine.

In January, 1908, I carried on an extensive series of tests of some special tuning devices for interference-prevention that I had devised, making use of the tower of the Times Building in New York, and receiving messages direct from Porto Rico, Cuba, the Canal Zone, etc. Beginning in the Fall of 1908, and continuing until 1909, I took courses in Electrical Engineering at the Sheffield Scientific School of Yale University, and was there awarded honors in Physics and Mathematics. During this period my experimental work was continued in the graduate research laboratories of the Scientific School and at my home station; data concerning the investigations then made have been published by me in my patent specifications and elsewhere. A considerable portion of my experimental work

at that time was directed toward the solution of tuning problems.

After an interview with Prof. Fessenden in December, [fol. 1436] 1909, I joined the staff of the National Electric Signaling Company and proceeded at once to its high-powered station at Brant Rock, Massachusetts. There I was put in charge of the long distance radio telegraphic tests made between that plant and the United States cruisers Birmingham and Salem. Until the Fall of 1921 I was continuously associated with the National Electric Signaling Company and its successor, the International Radio Telegraph Company, first as Telegraph Superintendent, then (in February, 1911), as Chief of the Operating, Erection and Inspection Department (from September, 1914), as Chief Research Engineer, Commercial Manager, and finally, as Manager. My work for the Company involved design, criticism, investigation, manipulation and manufacture of practically all forms of apparatus used in radio telegraphy and telephony.

In the Fall of 1921 I opened my own office as Consulting Engineer, and since that time have specialized in Radio. Throughout my contact with the field I have kept in close touch with the literature of the art, including the patents, as it has been published. Numerous technical papers and discussions written by me have been published in the Electrical World, the Electrician (of London), the Proceedings of the Institute of Radio Engineers, the Jahrbuch der Drahtlosen Telegraphie und Telephonie, etc.; and I have presented papers on radio subjects before meetings of the American Institute of Electrical Engineers, the Institute of Radio Engineers, the Society of Wireless Telegraph Engineers, the second Pan-American Scientific Congress (at Washington), at the Sheffield Scientific School and at Johns-Hopkins University, as well as before other organizations. A number of patents have been issued upon my inventions in Radio Telegraphy and Radio Telephony, both in the United States and abroad. I am a Fellow and Past President of the Institute of Radio Engineers, and served as Chairman of that body's Standardization Committee for a number of years. I am a member of the American Institute of Electrical Engineers, the American Association for the Advancement of Science, and other scientific and technical organizations. I have been called to Wash-

ington several times by Secretary Hoover, to serve in his Radio Advisory Conferences.

In general, I am familiar, by personal investigation and experience, with the design, manufacture and use of many types of radio transmitters and receivers. I am a practical radio operator and for some years held a Government license as such. I have written and had published by Little, Brown & Company, of Boston, a book on Radio, entitled "The Outline of Radio," which has gone into a second revised edition. Since 1908, I have made a close study of the patents in the art and science of radio-communication, and have been called upon to testify as a technical and practical expert in various litigations.

Q. 2. Have you read the Marconi tuning patent, No. 763,772 in suit, and do you understand the same, particularly with reference to the inventions recited in the transmitter claims thereof, numbers 1, 3, 6, 8, 11 and 12, the receiver claims thereof, numbers 13, 14, 16, 17, 18 and 19, and the combined transmitter and receiver claims hereof, numbers 10 and 20?

A. I have studied the patent, and believe that I understand it.

[fol. 1437] Q. 3. Briefly state the nature of the inventions referred to in the last question and the position such inventions occupied in the radio art at the date of Mr. Marconi's invention thereof which, for the purposes of this case, is that of the Marconi British patent No. 7,777, of April 26, 1900.

A. The inventions of the Marconi tuning patent relate particularly to the association of four tuned circuits in a wireless signaling system. Two of these tuned circuits are normally at the transmitter and two at the receiver. Certain of the claims of the patent (in particular Nos. 1, 3, 6, 8, 11 and 12) cover the transmitting station, certain others (Nos. 2, 13, 14, 16, 17, 18 and 19), the receiving station, and still others (Nos. 10 and 20), the entire system, including both stations.

Taking up the transmitter first, we find the arrangement typified by Fig. 1 of the patent, and clearly described in the specification. The chief elements of this transmitter are, first, the closed circuit comprising a spark-gap *c*, a condenser *e* and an inductance coil *d* (which in Fig. 1 constitutes the primary coil of a coupling or oscillation transformer) and, second, the open circuit comprising the an-

tenna f and A, the loading coil g, the secondary coil d' and the earth connection E. With reference to the closed circuit, the three important elements c, d and e may be of any type, there being no limiting statements or characterizations of them in the patent that would tend to limit their construction to any one of the usual designs. This circuit is described at page 2, line 9 as an "approximately closed circuit", as (line 10), "being a good conserver", and (line 15), "as persistent oscillator". The latter ("persistent oscillator") is carefully defined at page 2, lines 15 to 20 as "an electrical circuit of such a character that if electromotive force is suddenly applied to it and the current then cut off electrical oscillations are set up in the circuit which persist or are maintained for a long time". This means, technically, that the closed circuit may be tuned or be resonant to continuing trains of oscillations which are persistent except for the rate at which energy is transferred to the antenna or open circuit to be radiated. In the next part of this same sentence, beginning at page 2, line 1, such a closed or persistently oscillating circuit is contrasted against the antithetical type of circuit "which very quickly imparts the energy of electrical oscillations to the surrounding ether in the form of waves". Thus it is clear that this first circuit c, d, e, in the condenser of which the electrical energy of the transmitter is stored or conserved prior to its emission in space waves, must be one having low or negligible radiation power, so that, by itself, it will constitute "a persistent oscillator" or, in other words, a highly resonant circuit.

The elements of the transmitter open or antenna circuit of Fig. 1, comprising f, A, g, d' and E, have been referred to above. This circuit is defined at page 2, line 10 of the specification as an "open circuit", at lines 11 and 20 as "a good radiator" of wave energy and at line 21, as I have already quoted.

The Marconi invention of this patent presented an element found by Marconi to be essential (as has since been verified by many other investigators), viz., the tuning together of the two circuits, open and closed, at the transmitter. This tuning feature made a tremendous impression upon the art, and aided the radio worker of the day [fol. 1438] in overcoming his outstanding problems of increasing the distance of reliable transmission and increas-

ing the "localization" of signals or the selectivity of radio systems required to operate concurrently in the same general neighborhood. By following the law laid down by Marconi and stated at page 2, lines 118-129 of the patent, viz.:

"The capacity and self-induction of the four circuits—i. e., the primary and secondary circuits at the transmitting-station and the primary and secondary circuits at any one of the receiving stations in a communicating system—are each and all to be so independently adjusted as to make the product of the self-induction multiplied by the capacity the same in each case or multiples of each other—that is to say, the electrical time periods of the four circuits are to be the same or octaves of each other"

it was at once found that the transmitting station not only could be made more powerful than ever before (resulting in vastly greater transmission distances), but also that there was produced a radiator of purer waves better adapted for the "localization" of messages or syntonic interference—free operation.

Referring next to the receiving system, which is exemplified by Fig. 2 of the patent, we find the same two coupled circuits tuned together. The antenna-to-ground circuit *f'*, A, *g'*, *h*, *j'*, E is defined at page 1, line 51 of the specification as "a good absorber of electrical oscillations". This is the characteristic of an open circuit used for reception, just as "good radiation" is the characteristic of such a circuit used for transmission. The coupled secondary circuit which includes the secondary coils *j*², the loading inductance coils *g*² and the tuning condenser *h'*, is shown as of the closed highly resonant or persistently oscillating (*i. e.*, substantially non-radiating and non-absorbing) type. The two circuits, open and closed, are tuned together as instructed at page 2, lines 118-129 of the specification quoted above. This tuning, and the arrangement of these circuits, enables the user to attain a high degree of accuracy in the tuning of the complete apparatus at a transmitting station and at one or more receiving stations.

As a practical matter, it has repeatedly and consistently been found that such tuning together of the open and closed circuit at a receiver permits the attainment of results which, in point of distance, selectivity and reliability of

reception, are vastly superior to any that could have been had by the use of any prior art apparatus.

Considering both the transmitting station and the receiving station as constituting a complete system, Marconi points out that the four tuned circuits (two at each end), are to be adjusted to the same frequency of resonance. The matter which I have already quoted from page 2, lines 118-129, and the following down to page 3, line 17 of the specification, covers this point. By such four-circuit tuning the advantages of distant-station selective intercommunication, which I have already covered in some detail as to the transmitter and receiver individually, are combined. The result was the production of a radio telegraph system far superior to anything that had previously existed, and marking so great and so nearly universally recognized an advance as to warrant the greatest respect for the invention of this patent.

Marconi himself, at page 1, lines 20 to 66, inclusive, of [fol. 1439] this specification, sets forth how the four-circuit tuning provides a system showing great improvement over the earlier Marconi re-issue patent No. 11,913 (which was the first to show a practical wireless telegraph system). In the re-issue patent only the single open radiating antenna circuit of the transmitter and the single open absorbing antenna circuit of the receiver were tuned together. This gave a relatively small degree of "localization" of the messages, that is to say, only a broad tuning of one station to another (in general not sufficient to meet the interference conditions of even twenty years ago), and also imposed very definite limits upon the transmission distance and reliability of the normal station. The four-tuned circuit system of the later patent 763,772, involving coupled tuned circuits, whether used completely at both stations or merely at either the transmitter or the receiver, resulted in the exceedingly substantial improvements that I have set forth. As to the transmitter, the improved system of the tuning patent not merely made possible the effective use of greater transmitting powers, but it avoided insulation difficulties that, in the system of the re-issue patent, often interfered with, or completely prevented effective transmission in wet or stormy weather. Further, the four-circuit tuning patent system had the important advantage over the "plain aerial" system of the re-issue patent that it removed the spark-gap from the res-

onant radiating circuit and thereby eliminated from that circuit a large part of the damping of the oscillations in the aerial. Thus a larger portion of the energy of the oscillating current is radiated efficiently, rather than being partly absorbed in the spark-gap resistance. In this way the aerial circuit is made a better radiator than that of the re-issue patent. As to the receiver, the provision of the coupled closed tuned circuit added greatly to the resonant selectivity of the system, thus not only minimizing interference, but increasing the signal response of a suitable detector and indicator apparatus.

Summarizing, the tuning system of the later patent 763,-772 established in the art what has been generally recognized as a great practical advance over the plain aerial system of the re-issue patent, as well as over such other improvements as had been adopted by the practical workers in the field up to that time.

Q. 4. In your last answer you have referred to some of the advantages of the inventions of the Marconi tuning patent over Marconi's plain antenna or re-issue patent: please consider this subject further in connection with "Defendant's Exhibit K-6", being a reprint of the lecture by Marconi at the Royal Academy of Science, Stockholm, 1909, in which Marconi refers to his plain antenna and coupled tuned circuit inventions and their efficiency. Will you please state from your knowledge of the practical art, to what Mr. Marconi was referring in comparing the efficiency of the two systems, particularly beginning at page 24 of his lecture?

A. The portion of this paper at page 24, contains the following statement by Mr. Marconi:

"A remarkable fact, not generally known, in regard to transmitters is that none of the arrangements employing condensers exceed in efficiency the plain elevated aerial or vertical wire discharging to earth through a spark gap, as [fol. 1440] used in my first experiments—Figs. 2 and 3 (*ante*). I have recently been able to confirm the statement made by Professor Fleming in his book, 'The Principles of Electric Wave Telegraphy', 1906, page 555, that with a power of 8 watts in the aerial it is possible to communicate to distances of over 100 miles. I have also found that by this method it is possible to send signals 2,000 miles

across the Atlantic with a smaller expenditure of energy than by any other method known to myself.

"The only drawback to this arrangement is that unless very large aerials are used, the amount of energy which can be efficiently employed is limited by the potential, beyond which brush discharges and the resistances of the spark gap begin to have a deleterious effect."

Here Mr. Marconi emphasizes very clearly the power limitation which is inherent to the original system of the Marconi re-issue patent 11,913. With ordinary antenna systems of the plain aerial type only a relatively small amount of power can be employed, and the signals will therefore travel only a relatively short distance in spite of the rather high electrical efficiency given to the system by its radiating qualities. On the other hand, when the coupled tuned system of Marconi patent 763,772 is used, even a small antenna can be made to send out powerful signals. Of course, if one has a very large antenna, its capacity may be sufficient so that even the old system will permit use of enough power to bridge quite long distances, such as the 2,000 miles cited by Marconi in that part of his lecture that I have quoted. But when the same large antenna is used with the coupled tuned system of the later patent, the useful power may be still farther and greatly increased, and then both the distance and the reliability of communication will be correspondingly multiplied. This is additional to the advantages of not requiring such perfect antenna insulation, of minimizing the heat-losses that occur because of the spark-gap resistance when there is a spark-gap directly in the antenna circuit, and of producing a sharper and better-tuned wave, all of which follow from the use of the Marconi tuning invention.

Q. 5. If you have read and understand the inventions of the Lodge patent No. 609,154, will you please briefly explain the construction and mode of operation of the invention of claims 1, 2 and 5 in issue herein of that patent and point out what, if any, bearing the disclosures of the invention of those claims has on the claims in issue of the Marconi patent in suit. Please give your reasons for any opinion you may express.

A. The Lodge invention of the claims referred to is the variably acting loading or inductance coil in the sending or receiving open antenna circuit for syntonizing or tuning

the transmitting antenna and the receiving antenna one with the other. This invention gave to the art, in addition to a way of increasing the persistence of oscillation of an open antenna system, a species of partial "localization" of messages by providing for the tuning together of the antenna systems at transmitter and receiver. In this connection it should be particularly noted that *only* the antenna circuits, and not any associated or coupled closed resonant or persistently oscillating circuits at either station were tuned together by Lodge. Marconi, in the tuning [fol. 1441] patent 763,772 not merely tuned the open circuits at both stations (as did Lodge), but provided the coupled closed circuits at each station and tuned them to the desired frequency or wave length. Thus Marconi tremendously improved the power radiation, the distance range, and the selectivity of the system. These improvements were so substantial that the Marconi four-tuned arrangement rapidly became almost universal in spark transmitting and receiving systems.

Q. 6. Will you please briefly explain the construction and mode of operation of the apparatus of the alleged "impulsive rush" invention of the Lodge patent, which has sometimes been referred to as "single chunk" excitation in this record, particularly such as is recited in claim 6 thereof, which is not in issue in this suit, and state to what extent such apparatus has been used?

A. The sixth claim of the Lodge patent, to which you refer, appears to be the only one that refers to this so-called "single chunk" method of exciting a transmitting antenna. Both in this claim and in the specification, at page 2, line 61, the apparatus is characterized as operating "by aerial disruption", thus indicating the idea of a sudden breakdown of the path between charging source and radiating antenna and the consequent "impulsive rush" of power to the antenna. Lodge's plan is exemplified by Figs. 3 and 4 of his patent, Fig. 4 being the more complete. Both of these figures illustrate a spark-gap (h^2, h^3), in series with the antenna, as in the Marconi re-issue patent. Lodge did not propose to take the spark-gap out of the transmitting antenna, but apparently thought that by the use of the additional supply-gaps h^6, h^7, he could effectively impress a sudden disturbing impulse upon the antenna, thereafter leaving it free to oscillate and to radiate waves. So far as I have been able to determine, this circuit has never been

adopted in the practical art; and I understand that when it was tested critically, it failed to operate as described by Lodge. The practical method of delivering energy to a radiating antenna system from a spark-gap system, as it has been adopted in the practice of wireless telegraphy, involves the production of a series of oscillations in a closed highly resonant circuit, and their transfer to a tuned antenna which contains no spark-gap, by means of a coupling oscillation transformer, as disclosed by the Marconi tuning patent 763,772.

Q. 7. Referring to Mr. Loftin's answer to Q. 21 relating to coupling (P. R., pp. 1023-1028), will you please state what is the character or degree of coupling between the primary and secondary circuits called for by the Marconi patent No. 763,772 and used in the construction and operation of transmitters and receivers made in accordance with the teaching of that patent?

A. Mr. Loftin says, near the middle of P. R. page 1027, that:

"The Marconi specification does not go directly into the matter of loose coupling between his circuits."

He appears to think, despite this, that, as he says near the bottom of the same page, the specification "implies a proper degree of loose coupling". He says further that Marconi's main object "is the localizing of the energy effort of his system" and that he (Loftin) has "shown that this cannot be accomplished in a coupled system without making the coupling loose". What Mr. Loftin considers would be a "loose coupling" and just how much "looseness" he [fol. 1442] thinks is imperative in his view of the Marconi invention, would appear, from his answer to X Q. 110 (P. R., p. 1166), to be a three per cent. or four per cent. coupling. I cannot agree either with his statement or his conclusion, for I find nothing in the Marconi specification, and I know of nothing in the practice of the Marconi invention in the art, that would tend to limit Marconi to the use of couplings looser than any definite maximum value. Looseness of coupling is a purely relative term. At a receiving station the operator will, following Marconi's construction, use whatever coupling he may find in practice, and under the particular conditions of the moment, will give him adequate selectiveness and responsiveness in his apparatus.

Similarly, at the transmitter, an operator will use whatever coupling gives adequate purity of wave, clarity of spark tone and efficiency. In general, and up to rather large limits, the higher the coupling the better will be the results from the energy viewpoint. On the other hand, too close a coupling will tend to destroy tuning accuracy under certain conditions. The closest couplings used in transmitter practice are about twenty per cent., and even this is relatively "loose" as compared to the theoretical maximum of one hundred per cent. coupling.

I understand that the apparatus specifically described in the Marconi patent, when it was re-constructed and measured by Mr. Waterman and Mr. Weagant, showed a wide range of variation in coupling coefficients. I see no reason why one should be limited in his choice of the best coupling for any particular purpose or condition by anything that is stated or implied in the Marconi tuning patent.

In my personal experience with spark transmitters, which includes the open-gap, rotary-gap and quenched-gap types), I have always found that the choice of coupling in the oscillation transformer was such as to produce the best radiation or largest antenna current, together with a good or clear spark and a satisfactory purity of wave. In any one of these cases resonance between the primary and secondary circuits was of the first importance though, secondarily, the coupling was adjusted or chosen so as to get the best energy and clarity in the radiated signal.

So far as I know, the use of coupled tuned closed and open circuits at radio transmitter and receiver began with Marconi's invention, and what we now know of the effects caused by variation of coupling has been learned from our study and uses of the Marconi tuned system.

Q. 8. Again referring to Mr. Loftin's answer to Q. 21, please state to what extent you may agree or disagree with his explanation of the relation between the primary and secondary circuits of the Marconi transmitter of patent 763,772, particularly as concerns the question of the decrement of the waves in the antenna circuit of such transmitter.

A. Mr. Loftin appears to take this matter up in the portion of his answer at P. R. pages 1042-1044. The first part of this testimony refers to the hypothetical transmitter adjustment represented by Figs. 4, 4*a* and 4*b* of Loftin Sketch II ("Defendant's Exhibit P-2"), opposite P. R. page 1043, and should require no discussion by me. This

is because, as Mr. Loftin says at the bottom of P. R. page 1042, he has "assumed for the purpose of discussion and illustration that the capacity and self-inductance of the antenna have such values as to give them a product differ-[fol. 1443] ent from the product of the inductance and capacity of the lateral circuit". In other words, Mr. Loftin has here assumed that the closed highly resonant circuit is *not* tuned to anything like the same frequency as the coupled open radiating circuit. Such an assumption is, of course, absolutely contrary to anything in the Marconi patent 763,772, which requires that the closed and the open circuits shall be tuned together. It is also contrary to the conditions used in practical operation. Consequently, I cannot see that its development by Mr. Loftin contributes anything to our understanding of the issues here, and it seems to me that a further discussion would be of no value.

The second part of his testimony, at P. R. pages 1044 and 1045, has to do with his Figs. 5, 5*a* and 5*b* in the same "Loftin Sketch H", and is an incomplete description of one transmitter condition or adjustment that *does* involve the Marconi tuning invention. Mr. Loftin shows that by tuning the closed and open circuits to the same frequency, and even under the limiting conditions which he assumes, waves of a single desired frequency are produced. He also points out at P. R. page 1044, by a comparison of Figs. 4*b* and 5*b*, that this tuning, as in the Marconi patent, will result in a greater strength of signal and, consequently, in better communication. Mr. Loftin does not give complete data for the case he illustrates and describes but, so far as the illustration goes, there is little to criticize in it as a showing of one exemplification of the Marconi invention. It must, however, be borne in mind that the conditions which he has assumed can represent only one such set of adjustments. Changes in frequency, in coupling, or in spark-gap characteristics would materially affect the curves drawn by Mr. Loftin. Nevertheless, so long as the two circuits at the transmitter remain effectively tuned to the same frequency, the Marconi invention would be utilized.

Mr. Loftin's testimony seems to imply that the conditions illustrated by his Figs. 5, 5*a* and 5*b* are the only conditions of operation utilizing the Marconi invention. I disagree flatly with any such conclusion, for I know from practical experience that the Marconi tuning invention is valuable, useful and utilized under many other practical conditions

or adjustments. I also disagree with certain of the conclusions that Mr. Loftin has stated at the top of P. R. page 1045, where he seems to insist that in the practical use of the Marconi invention the oscillations existing in the closed or "lateral" circuit must be long-drawn-out and prevented from feeding over into the antenna circuit at the rate that would be most effective.

I note that at P. R. page 1044, and again at P. R. page 1045, Mr. Loftin refers to the Lodge patent as stressing the tuning together of two circuits "for a good resonant transfer of energy" between them. He fails to point out that one of Lodge's circuits was at the transmitter and that the other was at the distant receiving station, so leaving the inference that Lodge had proposed the use of two coupled resonant circuits at either end of the system, similar to Marconi's, which is not the fact.

The Marconi tuning patent 763,772 gives practical directions for constructing both transmitters and receivers which have a wide range of coupling and wave-length adjustments. There is nothing in the patent that would limit the use of [fol. 1444] the tuning invention to any particular range of couplings, of spark-gap conditions or of wave decrements, and I know of nothing in the prior art that would enforce any such limitation. These several factors are interdependent to a considerable extent, one varying as any of the others is changed. The choice of any particular relation between them depends upon the practical conditions of operation. So long as the two characteristic circuits are tuned together, so as to enable effective resonant transfer of energy from the closed circuit to the radiating antenna circuit, the Marconi invention is used. This is the fact quite without regard to what particular values of the various adjustable factors may be selected by the operator to meet the conditions under which he may be working.

Q. 9. Will you please state what is your understanding of the use of the term "persistent oscillator" as used by Mr. Marconi in the specification of his patent No. 763,772, and state to what extent you may agree or disagree with Mr. Loftin's testimony concerning these terms, as given in his answer to Q. 21, P. R. 1033?

A. In the specification of the tuning patent Marconi uses the term "persistent oscillator" to define a type of circuit, viz., the closed highly-resonant primary circuit, in which electrical oscillations once set up will persist for a long

time, unless their energy is abstracted for radiation. The term is in contradistinction to the opposite term "good radiator", which defines the open or antenna circuit from which the energy of electrical oscillations is directly and effectively radiated, so that they cannot persist for a long time. At page 2, lines 15 to 20 of the specification, Marconi himself defines a "persistent oscillator" as

"*an electrical circuit* of such a character that if an electromotive force is suddenly applied to it and the current then cut off electrical oscillations are set up in the circuit which persist or are maintained for a long time."

Mr. Loftin, at the top of P. R. page 1033, in quoting this statement, disregards its punctuation and makes it appear that Marconi says oscillations were to be maintained in the primary circuit of his transmitter "for a long time". What Marconi did say was that best results are obtained when he uses a persistent oscillator in his primary circuit, the "persistent oscillator" type of circuit being defined as above by a parenthetical clause set off by dashes.

Mr. Loftin seems to gather that Marconi intended his primary circuit oscillations to persist for a long time, whereas there is nothing in the patent to suggest any such action. Mr. Loftin says, at P. R. page 1033, "there must not be permitted any too early unnecessary stopping or interference with the oscillations in the primary circuit", and with this I agree, since too early stopping of the oscillations would inhibit the resonant transfer of their energy from the closed circuit to the open circuit for radiation. Mr. Loftin does not point out the equally important opposite limit, however, that the primary oscillations must not be too long-drawn-out. If the energy in the primary circuit is not transferred with reasonable rapidity to the antenna circuit from which it is to be radiated, it will be frittered [fol. 1445] away in lost heat and the transmitter will be inefficient. How rapidly the resonant transfer of energy may best be effected, will vary with different cases and different gap adjustments. In general, the efficiency of the system will be higher as the resonant transfer is made more rapid, for losses in the primary or spark-gap circuit will thus be avoided. Neither the specification nor the claims of the Marconi tuning patent limit the user to any one set of conditions. They leave his choice of adjustment

free, so that, by a selection of the proper values for his own particular condition, he may attain the maximum effectiveness that is characteristic of the Marconi invention.

Summarizing, I disagree with Mr. Loftin's implications or statements that by his use of the term "persistent oscillator" Marconi went beyond the definition of the type of circuit which he preferred to use as his transmitter primary, or that by any statement he limited the use of his invention to systems in which the oscillations of the primary circuit are somehow caused to persist in that circuit for an indefinitely "long time". Any such requirement or limitation would have been an unreasonable thing for Marconi to have made. It would necessarily have worked against the purpose of his transmitter invention, which was to obtain powerful and effective radiation of sharply-tuned radio waves. There is nothing in the Marconi patent, and I know of nothing in the prior art, that would enforce such a limitation upon him.

* * * * * * *

Q. 10. Will you please state what are the actions of spark-gaps or means for producing radio-frequency oscillations that to one skilled in the art of radio telegraphy or telephony would come within the mode of operation and principles of action involved in the use of the transmitting system of Marconi patent No. 763,772?

A. At page 1, lines 43 to 45, Marconi describes, as an element of his transmitting station, "a producer of Hertzian oscillations or electric waves shown in the form of a spark-producer", and at page 1, lines 83 and 84, he refers to the connection in the secondary circuit of his induction coil or power transformer to "the spherical terminals or other contacts of a spark-producer". At page 2, lines 25 to 38, he describes the operation of the transmitting station, from which it is clear that the action of any type of spark-gap in a radio transmitter is fundamentally simple and easily understood. In Fig. 1 of this tuning patent, the closed highly-resonant circuit is shown as comprising the spark-gap *c*, the coil *d* and the condenser *e*. What is desired is to produce in this circuit Hertzian oscillations (or electrical waves or oscillations), by the charge and discharge of condenser *e*.

The coil or power transformer shown at the right of the gap *c* in Fig. 1 provides successive waves or surges of high

[fol. 1446] potential, which may recur at intervals of say 1/1000th second. If the gap *c* is "open" or non-conducting, as it should be at the beginning of such a high potential surge delivered from the transformer, the electrical energy will be applied to the condenser *e* by way of the direct-connection from the top of the power transformer of Fig. 1 and through coil *d* from the bottom thereof. Thus the condenser *e* will be electrically charged to a high voltage, and will, for the time, store or act as a reservoir for, the energy to be converted into electrical oscillations.

If, now, the separation of the electrodes or contacts of the gap *c* has been adjusted, as it should have been, so that a spark-discharge will pass across the gap when the condenser attains a potential slightly below the maximum that can be delivered by the power transformer, it is clear that the gap will "break down" or become conductive just before the peak of the charging surge is reached.

The electrical charge stored in the condenser-reservoir will then spill out across the spark-gap, and, with proper values of resistance in the closed persistently-oscillating circuit *c*, *d*, *e*, will swing back and forth around through the coil and gap. The rate of electrical oscillation in this closed circuit is very high, and is determined principally by the size of the coil *d* and the condenser *e*, that is to say, by the tuning of the circuit. A customary rate of oscillation in such a transmitter for Marine use is 500,000 complete oscillations per second. This corresponds to a radio wave length of 600 meters, and requires that each half oscillation or swing of the electric charge from one side of the condenser to the other be completed in the extraordinarily short time of one one-millionth part of a second. The oscillations of the stored energy would persist for an indefinitely long time, except as their energy was abstracted from the persistently-oscillating circuit for radiation as electromagnetic signal-waves from the antenna or to produce wasteful heat in the circuits or their elements. In a practical transmitter the energy will be withdrawn from the closed oscillating circuit in say from five to twenty complete oscillations, being for the most part delivered to the antenna to produce radiated waves. If, at the common Marine frequency corresponding to the 600-meter wave, the oscillations persist in the closed circuit for ten complete periods, they will last for the full time of twenty one-millionths of one second, which is, of course, a brief interval

as compared to the time between successive charges of the condenser by the power transformer.

When the oscillating energy in the closed circuit reaches a low or zero value (as, for instance, when it has been delivered to the antenna circuit by resonant transfer through the oscillation transformer coupling *d, d'*), the gap should automatically "open" or regain its non-conducting condition. This requires that the spark-gap should be designed to have a sufficient mass of metal or otherwise to be cooled adequately with respect to the amount of power being used, so that the function of "opening" will take place quite rapidly, and well before the generation of the next successive high potential surge from the power transformer. Otherwise, that is, if the gap has remained in conductive condition, the power transformer will be at least partially short-circuited and will be therefore unable to charge the condenser on the next potential-surge,—thus power would [fol. 1447] be wasted. Thus, for efficient operation, any heating of the spark-gap that would tend to maintain its conductivity too long or to cause the formation of an arc discharge, must be avoided.

Assuming that the spark-gap has functioned correctly, *i. e.*, that it is sufficiently well cooled to prevent arcing, the condenser is once more charged up to the break-down potential of the gap and the cycle of oscillation generation is repeated again and again, once for each high-potential surge from the transformer.

Many types of spark-gap, called by many names, have been devised before and since the date of the Marconi tuning invention. When used in a coupled tuned circuit wireless telegraph transmitter, all of them work in the general way I have described, and all are alike in the principle of operation. First, in the art came the "plain" gap between spherical electrodes, then various forms of flat-surfaced electrodes, then gaps operating in series, then gaps equipped with air-blasts for cooling (or operated in compressed air or in gases), then rotary gaps of various design, and, most recently, various forms of the so-called high-quenching or quenched gap. The improvements attained in the evolution of spark-gap design have been toward greater uniformity of operation, quicker recovery of the "open" condition upon completion of the series of oscillations in the closed circuit, and the ability to operate with

greater amounts of power with consequent increase of effective transmission distance.

In the use of any of these types of spark-gap, it is extremely desirable, and indeed, it is essential for high efficiency, to get the stored energy from the condenser into the form of oscillations and across to the antenna circuit to be radiated with as little loss of time and energy as may be possible. The resonant transfer from the closed to the open circuit is of tremendous assistance in obtaining this result, and it is probably the most important single feature of the Marconi invention of patent 763,772. It is utilized in all practical spark radio systems with which I am familiar, including those of the Government described in the testimony here, and it is both necessary and highly useful whether the spark-gap be of the open, rotary or quenched type.

There are no fundamental differences in the action of these gaps, the improved reliability and power-handling capacity of the quenched gap being due to its mechanical design, which involves large flat sparking surfaces, good cooling and the subdivision of the sparking space. In general, greater cooling results in a more definite cutting off of the closed circuit oscillations when the train or group of oscillations in the primary circuit first dies down toward or to zero energy, and thus improves the efficiency of operation.

Q. 11. Referring further to Mr. Loftin's answer to Q. 21, particularly at P. R. page 1034, will you please state to what extent, and for what reasons radio engineers have recognized the desirability of rapidly cooling spark-gaps?

A. I have, in my last answer, generally explained the action of the spark gap in a wireless telegraph transmitter using coupled tuned circuits. I disagree with Mr. Loftin's statements or implications at P. R., page 1034, as to the necessity of producing heat for the satisfactory operation of an open spark-gap, or of any radio spark-gap. Cooling, either by the choice of the size and shape of the electrodes, [fol. 1148] or by supplemental means such as air-blowers, has always been essential to avoid the production of large ionization, such as Mr. Loftin refers to, and thereby to avoid the harmful arcing which I have described. The literature of the art is full of references to this fact. Even when one is using only small power at a transmitting station, the gap must remain reasonably cool, and blowers and fans are often used in small stations when the mass or

shape of the gap electrode is inadequate to conduct the heat away with sufficient rapidity. Many materials have been used for spark-gap electrodes, and, in my experience, the choice between them has been determined by the character and effect of the oxides produced in the continuous use of the gap, rather than for the purpose of increasing metallic ionization, as suggested by Mr. Loftin. Sluggish ionization which would tend to hold over the spark discharge is distinctly harmful.

Q. 12. Please state in what respects you may agree or disagree with Mr. Loftin's statements as to the production of beats being the object of the quenched-gap transmitter employing so-called "tight coupling", as testified by him in his answer to Q. 29, particularly at pages 1103-4 of the Printed Record.

A. The conclusions of Mr. Loftin to which you refer seem to me to be based upon a misconception of the facts underlying transmitter operation, and I do not agree with them. In any Marconi coupled-circuit transmitter, whether it be of the quenched-gap or the open-gap variety, the several oscillations that occur in the primary circuit for each spark discharge represent electrical energy that must be transferred to the antenna circuit in order to be radiated. To effect this transfer of energy efficiently the two circuits must be tuned to substantially the same frequency. The closer the coupling between the circuits, if they are tuned together, the more rapidly and effectively the energy will be transferred to the antenna system. In any properly designed and operated Marconi transmitter the spark-gap will "open" as soon as the energy of the closed circuit has first been delivered to the antenna circuit, and no reaction of the antenna circuit upon the closed circuit can or will occur.

So-called "beats" in a coupled transmitter are produced by continued reaction between the closed and open circuits, and appear only in improperly adjusted apparatus where these reactions are allowed to occur. As to this, the conditions are identical, whether the transmitter uses an open gap or a quenched gap, and "beats" do not occur in either case when the transmitter is correctly used as in practice. "Beats" are an undesirable and undesired incident to the incorrect use of any form of coupled spark transmitter.

It should be noted that these so-called "beats", if they are allowed to appear, are not true beats of the heterodyne

type, and are not described by any proper or strict technical use of the term "beats". They are simply increases and decreases of power in the circuits, as the total energy is allowed to shift back-and-forth between the closed primary circuit and the open aerial or secondary circuit.

The adjustment of the time periods or tuning of the open and closed circuits approximately (or exactly), into accord with each other is for the purpose of providing efficient [fol. 1449] transfer of energy of one to the other, in both the open gap and the quenched gap transmitters. Good practice requires that once the power has been transferred to the aerial circuit it shall remain there to be radiated, and shall not be permitted to react back into the primary circuit with the consequent loss of efficiency and possible production of "beats".

Q. 13. In your last answer you have referred to "beats of the heterodyne type". Will you please explain how such beats are produced, and in what connection they are used; also please give the technical meaning in the radio art of the word "heterodyne".

A. Beats, strictly speaking, that is to say heterodyne beats, are variations in the intensity of a resultant radio frequency current produced by the interaction upon each other of two radio frequency currents derived from different sources and usually having somewhat different frequencies. The addition of the two continuous trains of oscillations, one from each source, results in a single combined train, the amplitude of which varies periodically and continuously from one extreme, measured by the difference of the two components, to the other extreme, measured by their sum. This beating is analogous to that which occurs in music when two organ pipes of slightly different pitch are blown at the same time, the combined tone then having a flutter or beat in its sound.

The heterodyne is a radio-receiving system invented by Fessenden and named by him from the two Greek words meaning "other" and "force". Its connotation is the combination of two separately produced forces, and in the practical use of the heterodyne receiver there are combined radio-frequency oscillations produced from two separate generators. This is evidently quite different from the production of what Mr. Loftin has termed "beats" in a radio spark transmitter, where the waning-and-waxing effect is

simply due to the transfer and re-transfer of energy between the two coupled circuits, and where there is only one source of radio-frequency power.

Q. 14. Do you find anything in the Marconi patent No. 763,772, or in the prior art bearing on that patent, or in the actual practice of the invention of the claims in issue, which requires the coupled tuned circuits of that patent, either at the transmitter or at the receiver, to be mathematically or exactly tuned to secure the beneficial result of the coupled tuned circuits of the patent? Please give your reasons for your answer.

A. I do not. When an engineer uses the term "exactly tuned" or "perfect tuning" he necessarily means that the tuning is to be exact within practical limits. So far as I know, there is nothing made or used that can be adjusted and maintained in adjustment with absolute or precise mathematical exactness. These comments apply not only to the Marconi patent and system, but also to any other part of the electrical or physical arts and sciences.

Marconi does not specify an exact mathematical precision of tuning. Like all other skilled engineers, he would necessarily recognize the impossibility of attaining any such condition. The point is, to my mind, that Marconi's patent discloses, for the first time, coupled closed and open circuits at radio transmitter and radio receiver, these circuits being tuned together with sufficient practical exactness to give the useful result of a good and efficient resonant trans- [fol. 1450] fer of energy from one circuit to the other. Neither now, nor at the time of Marconi's invention, could precise mathematical exactness of tuning be had and maintained. The practical apparatus which Marconi describes in the patent has the necessary variable elements to permit the attainment of good or practically exact resonance and to obtain its benefits over a considerable range of tuning adjustments, frequencies or wave-lengths, and couplings.

Q. 15. Do you find anything in the Marconi patent in suit, or in the prior state of the art relating thereto, that would require each of the coupled circuits at the transmitter or receiver to have variable or adjustable means to bring about the tuned or resonant condition of the circuits?

A. No, I do not. Prior to the application for the Marconi tuning patent, workers in the radio art were, I believe, familiar with both "fixed" and "variable" inductance coils,

and with both fixed and adjustable or variable condensers. However, these elements had never been used for tuning together the open and closed circuits of a radio transmitter or a radio receiver. Having shown to the art the desirability of such tuning, I see no reason why Marconi should have insisted upon the exclusive use of either fixed or variable tuning elements in any or all of his circuits, and I find nothing in the patent to suggest that he intended to do so. Anyone practicing the Marconi invention was left free to use, as he might choose, either a variable or a properly selected non-adjustable condenser or coil in any circuit,—the effective or working value of the particular unit, of course, being chosen so as to give the desired resonance between the circuits.

Q. 16. In your opinion, is there anything in the prior art, or in the Marconi patent No. 763,772, which would require that the invention of the transmitting claims in issue of that patent should be limited to a system in which oscillations of but one and the desired frequency are created in the primary circuit of the transmitter and that such oscillations are transferred at this one and desired frequency into the secondary or radiating circuit, without change of frequency? Give your reasons for any opinion you may express.

A. No, there is no such limitation either in the Marconi tuning patent or required by the prior art. The agreement in tuning between the primary and secondary circuits of the Marconi transmitter is to enable the oscillating current energy to be transferred resonantly, and therefore with the greatest efficiency from the primary to the secondary circuit. This is quite aside from, and has nothing to do with, the development of subordinate or parasitic frequencies of oscillation during the time the energy is being transferred, which may occur in any transmitter using coupled tuned circuits with any type of spark-gap. If very loose coupling is used, so that reaction of the antenna circuit is small or negligible, the radiation will be of practically one frequency. If relatively close coupling is used under good quenching conditions, *i. e.*, with the gap operating properly, the same will be true. If close coupling is used under bad quenching conditions, so that reaction between primary and secondary can occur, there will be at least two, and possibly three, oscillation frequencies developed in substantial or observable amounts. Which of these three conditions may at any time

[fol. 1451] exist in any particular transmitter has nothing whatever to do with the necessity of tuning the closed and open circuits together. This tuning is used to obtain the advantage of rapid resonant transfer of energy as set forth in the Marconi patent, and the Marconi tuning advantages are had in any of the three cases that I have described.

Q. 17. In Mr. Loftin's answer to Q. 27, P. R., page 1085, he states that:

"The idea that the primary circuit at the transmitting station should store a very large amount of energy, so much so that it would be impossible to immediately deliver it to the antenna circuit on account of the structure of that circuit preventing it from having sufficient capacity to take all of the energy it was possible to store in the primary circuit."

is indicated by the statement in the Marconi patent No. 763,-772 (p. 2, lines 6-9), that

"The illustrated arrangement of parts at a transmitting-station enables much more energy to be imparted to the radiator *f*".

State whether or not you agree with Mr. Loftin that such an idea is conveyed by the language of the Marconi specification, giving your reasons for any opinion you may express.

A. I do not agree with Mr. Loftin's conclusion referred to in your question. The statement quoted from the Marconi specification simply indicates one of the main advantages of the Marconi invention, *i. e.*, to enable the antenna circuit to radiate more energy (and, therefore, to send signals to greater distances), than had before been possible with antenna circuits that included a spark-gap.

As a matter of technical fact the sentence quoted from the patent is directly contrary to Mr. Loftin's contention, for it requires "more energy to be *imparted* to the radiator" or antenna. Marconi's emphasis is, as it should be, upon the imparting of energy for radiation. This is entirely inconsistent with any idea that energy is to be *stored* in the primary circuit for a long time.

The fact is, as I have already explained, that Marconi first stores the energy in the primary condenser (not in the highly resonant circuit as a whole), then produces oscillations by the automatic discharge of the spark-gap, and then

transfers these oscillations resonantly, and as rapidly as possible, to the antenna circuit from which their energy is to be radiated in the form of signal-waves. As the patent makes clear, Marconi's invention is for the efficient and powerful radiation of energy, and not for its wasteful retention in a closed non-radiating circuit. I find nothing in the patent, and I know of nothing in the practical art, which would support such a distorted contention as that here made by Mr. Loftin.

Q. 18. Mr. Loftin, in his answer to Q. 23 (P. R., p. 1056) states that he does not consider that the "Waterman Sketch No. 2" entitled "Oscillations of Primary Circuit" and "Oscillations of Secondary Circuit" found opposite page 151 of Mr. Waterman's deposition, properly illustrates the oscillations of current in the two circuits of a transmitter adjusted as described in the specification of Marconi patent No. 763,772. Mr. Loftin also, in his answer to Q. 24 (P. R., [fol. 1452] p. 1058), states that there is no foundation for the sketch either in the Marconi specification or in the way the Marconi apparatus will function under any form of adjustment or association of circuits. Mr. Loftin further states in his answer to Q. 25 (P. R., p. 1060) that Mr. Waterman's sketch shows a quick dumping of energy from the primary to the secondary circuit, without the aid of resonance. Will you please consider Mr. Waterman's and Mr. Loftin's testimony on this subject, and state with which of them you agree, giving your reasons for your agreement.

A. I have read Mr. Loftin's answers to questions 23, 24 and 25, at P. R., pages 1056-1060, and believe that his criticisms of Mr. Waterman's "Sketch No. 2" are not justified; they appear to be based upon Mr. Loftin's own conception that the oscillations in the primary circuit of a Marconi transmitter must last for a long time, that the coupling between the closed and open circuits must be very loose, and that the spark-gap must be used in such condition that little or no cutting-off action could occur. These three assumptions of Mr. Loftin's are, I belie[illegible] contrary to fact.

Marconi describes his primary or closed circuit as a persistently oscillating closed circuit, as I have already described, and nowhere implies or requires that oscillations should remain in it for a long time. It is true that they must persist sufficiently long to make the resonant transfer of energy to the secondary circuit an advantageous action,

but this is true of the four or five oscillations indicated by Mr. Waterman's sketch. This is, of course, a "quick dumping" of energy as compared to a transfer that might last over the time of an entire wave-train, but it is a resonant transfer and extends over a long period of time as contrasted to the single impulse or "impulsive rush" method of excitation attributed to Lodge and stated by Mr. Loftin to be characteristic of the quenched-gap transmitter.

As to coupling, a wide range is shown in the Marconi tuning patent. I understand that the apparatus specifically described by Marconi in the specification, when reconstructed and carefully measured, included coupling coefficients of values as high as about twenty per cent.

As to the spark-gap, it is general recognized and well understood that any adequate type of gap shows some quenching action when in proper adjustment, and that the better the cooling or quenching, in general the better the efficiency of the system in which the gap is used.

In addition to the general criticism based upon the assumptions discussed above, Mr. Loftin falls into a grave technical error when, at the bottom of P. R., p. 1057, he says that "The total area covered by the oscillations illustrated by Mr. Waterman in the secondary circuit represents the total energy in that circuit" and proceeds to criticize the conditions represented by the sketch as one "which could not even be obtained with one hundred per cent coupling between the circuits." The horizontal axis of Mr. Loftin s and Mr. Waterman's sketches represents elapsed time and the vertical axis represents current, as Mr. Waterman clearly points out at P. R., p. 151. The area included by the wavy lines of the sketches therefore represent current multiplied by time, which is a very different thing from the "total energy" upon which Mr. Loftin bases his attack. In [fol. 1453] order to make his statements, Mr. Loftin has apparently assumed that the two axes represent respectively voltage and current, which two components, when multiplied together, give energy per unit of time, or power. The representation used by Mr. Waterman in his "Sketch No. 2" is technically correct and follows the methods of conventional drafting customarily used by authorities in the Radio art.

Summarizing, Mr. Waterman's "Sketch No. 2" indicates graphically the type of circuit action which occurs in a

well-adjusted modern spark-transmitter using the Marconi invention. Mr. Loftin's criticisms seem to me to be based upon limiting assumptions which he himself has made, which do not appear in the Marconi patent and which are not observed in the practice of the Marconi invention.

Q. 19. Mr. Loftin, in his answer to Q. 26 (P. R., pp. 1062, 1063), referring to Mr. Waterman's "Sketch No. 3" inserted opposite page 155 of the Printed Record, states that the condenser j^3 of the Marconi Fig. 2, results in a certain defect in the representation of that figure, and that Mr. Waterman has removeed the condenser from the position occupied in Fig. 2 of the Marconi tuning patent in suit without any justification in said Marconi patent for its removal. What have you to say on this subject?

A. With respect to Mr. Loftin's criticism of Mr. Waterman's "Sketch No. 3", given by Mr. Loftin in answer to Q. 26, I would first say that Mr. Waterman's sketch is exactly what it purports to be and what is labeled, *viz.*, "Marconi's Simplified Receiving Circuits". Mr. Loftin says at P. R., p. 1062, that Mr. Waterman has shown the condenser j^3 "in parallel with the variable condenser h'", but this is not the fact, as may be easily seen by looking at the drawing. He criticises Mr. Waterman for removing the condenser j^3 from the tuned circuit, in spite of the fact that Mr. Waterman accurately states that Marconi's showing of j^3 in the divided winding of coil j^2 "has no essential relation the mode of operation but is merely a refinement". Mr. Loftin should know that j^3 is simply a stopping or blocking condenser of relatively large size and that it has no substantial effect in tuning the closed circuit of the Marconi receiver, whether placed in the center of the divided coil, as in Fig. 2 of the patent, or whether shown as in Mr. Waterman's Simplified "Sketch No. 3". To my mind, Mr. Waterman is amply justified by the Marconi patent in the presentation of his "Sketch No. 3", for the elements there shown perform the useful functions assigned to them by the Marconi tuning patent. The rearrangement of the simplified sketch, which Mr. Waterman carefully explained, simply serves to make these functions all the more clear.

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[fol. 1454] Q. 20. Please consider the Tesla patents No. 645,576, dated March 20, 1900 ("Defendant's Exhibit

X-2"), No. 649,621, dated May 15, 1900 ("Defendant's Exhibit L-6"), the Tesla British Patent No. 20,981, of 1896 (Defendant's Exhibit Y-2") and the Martin-Tesla Book ("Defendant's Exhibit B-4"), in connection with Mr. Loftin's testimony relating thereto and state what bearing such patents and publication have upon the inventions of the Marconi tuning patent No. 763,772, and particularly as to the combination of elements and their mode of operation as nicluded in the claims thereof which are in issue herein. Please give your reasons for your opinions.

A. I have studied the Tesla patents cited, the Martin Book and a number of other publications dealing with Tesla's work. Some of Tesla's publications, his patents 645,576, 649,621 (and a number of others), and the Martin Book, were set up by Defendant and considered as anticipations of Fessenden's continuous-wave radio patent, reissue No. 12,168 in the suit of Kintner *et al. v.* Atlantic Communications Company, before Judge Mayer in the U. S. District Court for the Southern District of New York, which was reported at 249 Fed. 73. In that suit, where I testified on behalf of plaintiff, I made a thorough survey of Tesla's work in high-frequency electricity. Telsa was not working in the radio telegraph art, and his devices have no more than a certain remote resemblance to some elements of radio apparatus.

Mr. Loftin attempts to excuse Tesla's definite statements as to his invention and its uses, by terming them "ambitious", and to avoid the fact that Telsa was working on a plan of high voltage electric *conduction* (not wave radiation), by considering references to that fact as "theory" on Tesla's part. Such excuses cannot avoid Tesla's own statement of his invention, as at page 2, lines 114-121 of his patent No. 645,576, as follows:

"Expressed briefly, my present invention, based upon these discoveries, consists then in producing at one point an electrical pressure of such character and magnitude as to cause thereby a *current* to traverse elevated strata of the air between the point of generation and a distant point at which the energy is to be received and utilized."

Tesla proposed to set up enormous voltages (millions of volts), far in excess of those used in radio signaling, which would *force* electric *current* (not radio waves), through the

rarefied air high above the earth. This current he proposed to use to operate lamps, electric motors, etc. As Mr. Loftin points out, he makes an incidental suggestion of the transmission of intelligible messages (at page 5, line 31), but this is not wireless telegraphy as Marconi knew it and as we have known it since Marconi's first work. That is to say, it is not Hertzian or *radio* telegraphy operating by means of radiated electro-magnetic waves. It is simply a suggestion of a peculiar type of current-conduction telegra-[fol. 1455] phy in which the air's conductivity to high voltage electric discharges was to be utilized just as the wire's conductivity for low voltage electricity is used in ordinary Morse wire telegraphy.

Mr. Loftin also says in effect that the explanation of Tesla's transformer coil design (as given in his patent), shows Tesla to have had the Marconi tuning invention. Such a contention seems to me to do violence to the facts set forth by Tesla, who disregards entirely the destructive effect that his proposed elevated conductors would necessarily have had upon the "synchronizing" of his transformer coils when they were connected thereto. Tesla's explicit directions are to adjust the transformer secondary coil to produce a maximum voltage at its central terminal, and then to apply that exceedingly high voltage to the elevated conductor through the wire B (of his patent 645,576). By following these instructions one would obtain splendid results for the lighting of vacuum lamps, the production of spark displays and the other types of demonstration characteristic of Tesla's work, as described by Martin and others, but the apparatus would be utterly useless for the generation of radio waves from the elevated conductor. To make the Marconi invention Tesla would have had to tune his entire secondary circuit *with the elevated conductor attached* (not merely the secondary coil alone), and this he does not describe and evidently did not do. Moreover, there is no reason why he should have done so, for, if he had, upon removing the elevated conductor he would have found his famous "Tesla coil" an inert thing, incapable of producing the forty-foot sparks and the other striking demonstrations upon which he placed so much emphasis.

The second patent, No. 649,621, is a division of that already considered and adds nothing to its disclosure. However, this second patent makes even more clear the

distinctions between Tesla's projected plan and the Marconi tuning invention, for Tesla says at page 2, lines 73-82, the following:

"It is to be noted that the phenomenon here involved in the transmission of electrical energy is one of true conduction and is not to be confounded with the phenomena of electrical radiation which have heretofore been observed and which from the very nature and mode of propagation would render practically impossible the transmission of any appreciable amount of energy to such distance as are of practical importance."

The British patent No. 20,981 shows a subordinate device and might be used in connection with the high voltage conduction apparatus of the U. S. patents. The Martin Book, to which Mr Loftin refers at P. R., pp. 956-958 and 1155, is on matters entirely apart from radio signaling, is purely speculative as to power transmission and is considerably less definite than even the patents here referred to. None of these patents, nor any publication concerning Tesla's work that I have found (including the Martin Book), shows any realization of radio telegraphy, of the problem in radio that Marconi solved by his tuning invention, nor of Marconi's solution of that problem.

More specifically, none of Tesla's disclosure show or describe Marconi's apparatus or its operation, nor could any- [fol. 1456] thing shown or described in Telsa's disclosures be used to produce Marconi's results, without substantial changes or additions. In fact, the Tesla disclosures do not describe a radio system or any system having an open or radiating circuit coupled to a closed circuit, including a spark-gap or detector, the two circuits being tuned together, or in resonance when in operation. Finally, so far as I am aware, none of these disclosures of Tesla was ever put into practical use.

The "Loftin Sketch A", purporting to represent a Tesla transmitter, which appears opposite page 957 of the Printed Record, shows an organization which cannot be found in any of the Tesla publications. Part of the sketch seems to be based upon part of Fig. 165 of the Martin Book, which illustrates several circuits for converting electric currents. Another part of the drawing is stated by Mr. Loftin to be based upon Fig. 185 of the Martin Book, which figure has nothing whatever to do with radio telegraphic transmission

or reception. The primary and secondary coils P and S_1 of Mr. Loftin's sketch may have been taken by him from one of the Tesla patents, though they are not there shown in association with the other apparatus of "Loftin Sketch A". This sketch is evidently a composite drawing made up of various elements shown by Tesla and Martin at various different parts of their publications and, so far as is shown, never combined by Tesla in the way indicated.

Q. 21. What, if any, bearing has the receiving apparatus described in the Lodge patent in suit No. 609,154, and illustrated in Figs 12 and 13 thereof, upon the inventions of the receiving claims Nos. 2, 13, 14, 16, 17, 18 and 19 of the Marconi tuning patent No. 763,772, and also of the combined transmitter and receiver claims Nos. 10 and 20 of that patent?

A. It seems to me to have no bearing upon the tuning invention.

Mr. Loftin says, at P. R., page 1109, that Lodge shows a secondary circuit that "is impliedly tuned" to resonance with the antenna circuit. He says at P. R. page 1111, that two of Marconi's receiving station claims, Nos. 2 and 13, "do not in any way refer to the matter of resonance between the two circuits" and hence are met by Lodge's Figs. 12 and 13. He also says, at P. R. page 1120, that Fig. 13 of the Lodge patent shows:

"an open absorbing circuit associated with a closed secondary circuit with means provided for adjusting the two circuits to be in tune with the incoming frequency of the radio signals."

I do not understand upon what basis Mr. Loftin thinks that the secondary circuit *e, f, g, u* of Lodge's Fig. 13, or the approximately corresponding circuit of Fig. 12, is "impliedly" tuned, for I find nothing in the Lodge patent to suggest such tuning and the coherer circuits shown are of a nature not susceptible of tuning. Neither figure of the Lodge patent, nor the descriptions thereof, meets Marconi's invention, nor do they meet, even in terms, the Marconi claims that specify the tuning of two circuits at the receiver (*i. e.*, claims 14, 16 and 17), or the tuning of four circuits at transmitter and receiver (*i. e.*, claims 10 and 20). Neither figure nor description meets the spirit of the other claims, as taken in the light of Marconi's specified

tuning relation, nor do they even meet the terms of those [fol. 1457] claims. Marconi's claims 2, 13 and 19 recite a condenser in circuit with the wave-responsive device or detector, and his claims 18 and 19 require a variable inductance coil in the detector circuit. Lodge shows neither of these tuning elements, and the suggestion of a condenser as alternative to the resistor shown at W in Lodge's Fig. 12 has nothing whatever to do with tuning. In the Lodge receiver there is no pair of coupled tuned circuits, as described and claimed by Marconi, nor is there any resonant transfer of energy from one to the other of such circuits.

Q. 22. Please consider the Marconi patents Nos. 627,650, 647,007, 647,008, 647,009 and 668,315 in connection with the testimony of Mr. Loftin bearing thereon, the first of which was referred to by Mr. Loftin at pages 1071, 1160, and 1167 of the Printed Record, and the latter four patents at pages 1021, etc., of the Printed Record and state what, if anything, you find in them bearing upon the claims in issue of the Marconi patent No. 763,772 in suit.

A. I have studied the five patents to which you refer, and find nothing in any of them that bears upon the tuning of two coupled circuits, as disclosed and claimed in the Marconi tuning patent 763,772.

Of the patents which you enumerate, the four latest to issue, show various forms of transformer coils for radio receivers, and teach nothing of tuning the closed circuit of a receiver. The first patent to issue, No. 627,650, shows an inductively-coupled receiver in which the coils are chosen of a fixed value, which has no tuning condenser, and which has no closed tuned circuit. I note that at P. R., page 1072, Mr. Loftin suggests that the stopping condenser k' is to be varied for the tuning of the detector circuit, but to make this statement, he must have overlooked its position in the circuit. The condenser k' is simply a blocking or stopping condenser shown in the first circuit of this patent to prevent the coil j^2 from short-circuiting the battery current around the coherer. Like any stopping condenser, it may be changed in size, to some slight advantage, as the wave-length used is changed; but it is in no sense a tuning element and it is neither suggested nor described as such in the patent. Because of the small capacity and the high resistance of the coherer T, a condenser so connected will not tune the circuit of coil j^2. Consequently this patent,

Mr. Loftin to the contrary notwithstanding, shows nothing of the tuning invention of Marconi's patent 763,772.

Q. 23. Please consider: (1) the apparatus and its mode of operation as described and illustrated in the Stone-Baker letters and the depositions of Joseph B. Baker, John Stone Stone and Walter C. Dean, stipulated in this case, and also Mr. Loftin's testimony relating to such apparatus (P. R., pp. 1076-1082, and 1086); (2) the apparatus and its mode of operation disclosed in the Stone application as originally filed and on which patent No. 714,756, issued on December 2, 1902, and Mr. Loftin's testimony relating to such patent (P. R., pp. 1082-1087); and state whether or not, in your opinion, any of such apparatus and its mode of operation discloses the devices or combination of elements and mode of operation of any of the inventions of the Marconi tuning patent No. 763,772, as recited in the claims thereof which are in issue herein; give the reasons for any opinion you may express.

A. I have carefully read the Stone-Baker letters bearing [fol. 1458] dates of "June 30, 1899" and "July 15, 1899", as well as the depositions referred to in your question. Certain apparatus is illustrated by a wiring diagram on the third page of the earlier Stone-Baker letter, "Defendant's Exhibit F-3". However, no wiring diagram can show whether or not the various circuits illustrated are tuned to the same or to different frequencies; and for information on that important matter we must turn to the next descriptive of the figures.

In the Stone-Baker letters Mr. Stone points out:

(1) That he wishes to produce oscillations "of a frequency corresponding to the fundamental of the *wire*" (June 30, page 2);

(2) That certain of the circuits illustrated are to be tuned "one to another and all to the same frequency" (June 30, page 4);

(3) That simple oscillation of a vertical wire does not produce a pure wave (July 15, page 3);

(4) That in his arrangement "the vibratory current developed in the vertical wire is not due to the oscillatory discharge of the wire" (July 15, page 4);

(5) That the current is a *forced* oscillation depending as to frequency only upon the impressing force and "not

upon the electro-magnetic constants of the circuit in which they are developed" (July 15, page 5);

(6) That the period of the impressed forces may "be the same as that of the fundamental or the *vertical wire*" (July 15, page 5);

(7) That, at the receiver, "the circuit *in which the coherer is located* is practically opaque to" currents produced by off-frequency waves (July 15, page 6); and

(8) That there is no difference in principle between the two species of circuits shown in the June 30th letter (July 15, page 8).

All of these items support the view that Stone did not want and, in fact, would not tolerate an antenna system which was tuned to the frequency of the associated closed circuit, since he conceived that the free oscillations thus produced would not be "simple harmonic" or pure as to frequency. He did propose to improve selectivity by inter-station tuning of closed circuits. He even gave the option of making the closed circuit frequency the same as the fundamental of the aerial *wire* used, but he nowhere suggested that it could be the same as the natural or tuned frequency of the aerial system or circuit, *including both vertical wire and transformer coil.* The suggested "tuning of these circuits one to another" might, in the absence of Stone's more detailed and extensive instructions to the contrary, be thought to indicate a tuning of the antenna system. With his full disclosure before one, however, and considering his repeated injunction to use forced oscillations only, to make the wave-frequency independent of the antenna constants, etc., it is perfectly clear that the only circuits Stone proposed to tune "one to another" were the closed circuits illustrated. His letters to Baker thus lacked any disclosure of the important tuning together of closed and open circuits at transmitter or receiver, or both, which tuning is the essence of the Marconi invention and is stated or implicit in all the Marconi claims.

[fol. 1459] In the above quotations, as elsewhere in my deposition, the italics have been inserted by me.

I have also studied carefully the Stone patent 714,756, to which you refer, and its file history. As originally filed, the specification follows closely along the lines of the Stone-Baker letters, and appears to have been drawn from a similar disclosure. The original application at page 4,

lines 18 to 22, points out that the antenna is to be made a source of waves "of any desired frequency independent of its length and other geometrical constants". At page 10, lines 19 to 26, Stone emphasizes his intention of using *forced* oscillations in the antenna "in lieu of producing natural vibrations". At page 13, he points out the tuning together of the *closed* circuits at transmitter and receiver. At page 15, lines 23 to 25, he states that the forced oscillations in the transmitting antenna are "practically independent, as regards their frequency, of the constants of the second circuit in which they are induced". From the beginning to the end of the original specification there is no suggestion of tuning together closed and open circuits at either transmitter or receiver, and the Stone invention is clearly shown as ignoring any antenna resonance. In some of the transmitters which he illustrates, there are several closed circuits tuned to the same frequency, but no tuned open circuit. Similarly, in some of his receivers, there are several closed circuits which may be tuned together, but no tuned open or absorbing circuit. Stone's theory was evidently that forced oscillations, not utilizing any antenna tuning, were essential for selectivity; and he at his filing date, apparently did not appreciate the tremendous increase in efficiency obtainable by the Marconi invention of using in combination, either at transmitter or receiver, or both, closed and open circuits tuned alike.

However, in the amendment dated April 8, 1902 (Paper No. 7), Stone inserted into the specification a statement that:

"The vertical wire may with advantage be so constructed as to be highly resonant to a particular frequency and the harmonic vibrations impressed thereon may with advantage be of that frequency."

This insertion appears in the issued patent at page 6, lines 62-66, and may be taken as a recognition in 1902 that Marconi's tuning of the aerial circuit was a desirable thing. On page 6 of the long amendment dated June 7, 1902 (Paper No. 12), Stone's attorney refers to "the important cases when the elevated conductor of a wireless telegraph system has a fundamental which is of the same frequency as the forced vibration impressed upon it", when he says, "the natural vibrations excited will not interfere with the wave

form". This is true when "aerial conductor" and "vertical wire" are taken to mean the entire radiating circuit, or antenna plus coils and ground connection, but it is not true when the vertical wire alone is meant. The modification of the specification shows Stone's adoption in 1902 (even though simply as "important" cases), of Marconi's tuning of the antenna circuit to the closed circuit. The application as filed on February 8, 1900, and as prosecuted up to April, 1902, shows no such conception of the Marconi invention.

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[fol. 1460] Q. 24. Please consider the Stone patent No. 638,152, granted November 28, 1899, and Mr. Loftin's testimony relating thereto (P. R., pp. 1065-1067) and state what, if any, bearing that patent has upon any of the inventions disclosed in the Marconi tuning patent No. 763,772, particularly as recited in any of the claims thereof at issue herein, giving your reasons for such opinion as you may express.

A. This Stone patent 638,152 is suggested by Mr. Loftin at P. R., page 1065, as stating the fundamentals for both radio communication and what is now known as "wired radio". The contrary is the fact, for the Stone patent omits the important and essential element, *viz.*, the wave or oscillation detector. This is a patent on a wire telephone system of doubtful operability, and contains no antenna at either transmitter or receiver, no second tuned circuit at either transmitter or receiver, involves no radio or radiated waves, and has no wave-responsive device. It contains no suggestion of the Marconi invention and meets neither the terms nor the spirit of any of the Marconi claims.

Mr. Loftin seems to treat this patent in much greater detail than its disclosures would warrant, and makes a number of interpretations of its wording with which I cannot agree. For instance, with reference to the clause "closed secondary reactions generally should be avoided or minimized", Mr. Loftin finds Stone directing the user to use "loose coupling". This clause has nothing to do with loose coupling or any other working coupling, but is an instruction to avoid what is generally known in the art as the effects of "short-circuited secondaries", *viz.*, a reaction

which tends to increase losses and reduce the effective inductance of any coil in which it is allowed to appear.

Q. 25. Defendant has introduced in evidence (P. R., p. 961), the Stone patents No. 577,214, granted February 16, 1897; No. 714,831, granted December 2, 1902 (Filed January 23, 1901, as a division of patent No. 714,756), and the File Contents thereof; No. 726,368, Filed April 4, 1894, granted April 28, 1903, and the File Contents thereof; No. 726,476, granted April 28, 1903 (Filed March 12, 1903, as a Division of Patent No. 726,368). Will you please briefly consider these patents in connection with the said File Contents thereof, and state what, if any, bearing the disclosures thereof prior to April 26, 1900 (the date of Marconi's inventions of patent No. 763,772), have upon that patent, and particularly the inventions of the claims in issue in patent No. 763,772. Please state your reasons for such opinion as you may express.

A. Of the four patents which you enumerate in your question, only two were filed prior to April 26, 1900. Of these two the first one to issue is No. 577,214, which was filed on September 10, 1896. It is simply for a special type of single resonant circuit in a wire net-work, and has no suggestion of even a wireless use,—much less any disclosure of the Marconi tuning invention. Therefore, I do not see that its disclosures have any bearing upon the Marconi [fol. 1461] patent No. 763,772 or its claims. The other of the two Stone patents is No. 726,368, which was filed on April 4, 1894. This shows a wire telegraph system using low frequencies. The only specific frequencies stated are 250, 500 and 1,000 cycles per second, which are far below the radio frequencies used in the wireless art from the time of Marconi to the present. This patent of Stone neither shows nor suggests antennae, wave detectors, nor plural tuned circuits, and discloses nothing anticipatory of the Marconi invention. This patent did not issue until April 28, 1903, but even in the years after Marconi's invention I find nothing in the File history to suggest that this invention of Stone's had anything in common with Marconi's. For these reasons I see no bearing of either of these Stone patents upon the Marconi invention or the claims of Marconi patent No. 763,772.

Of the other patents cited by you, No. 726,476 was filed on March 12, 1903, as a division of the wire telegraph patent 726,368, just discussed. It issued with the parent case

on April 28, 1903, and its specification is substantially the same, if not identical. The same comments apply to this patent, and I cannot see that it has anything to do with the Marconi tuning invention.

The remaining Stone patent is No. 714,831, which was filed on January 23, 1901 as a division of patent 714,756, which I have already discussed in my answer to Q. 23. The specification as filed in 1901 showed no recognition of tuning the antenna or radiating circuit to the same frequency as the closed circuit, but on April 14, 1902, the case was amended to include this feature,—just as the parent case was amended on April 8, 1902, in the way I have previously explained.

Q. 26. Please consider the Hutin & Leblanc patent No. 838,545 and the Pupin patents Nos. 519,347, granted May 28, 1894, and No. 640,516, granted January 2, 1900, also Mr. Loftin's testimony relating thereto, and state what, if any, bearing they have on the inventions of the Marconi tuning patent No. 763,772 and recited in any of the claims of said patent in issue in this suit, giving your reasons for such opinion as you may express.

A. The Hutin & Leblanc 838,545 is for "multiple telegraphy and telephony" over wire lines. It has no reference to radio or to any other form of wireless signaling. For its wire transmission purposes it proposes the use of a group of circuits associated with the wire line and each tuned to a *different* frequency. The highest current-frequencies suggested are far below those used in radio at Marconi's time, and those proposed for telegraphy are within the audible range never used for radio. The disclosure has no antenna, no spark-gap, no coupled open and closed circuits, no detector,—in fact, it is utterly remote from radio and from the Marconi tuning invention. It represents a pioneer and probably important invention in line wire communication, but evidently taught nothing to the radio art. The principle of single-circuit resonance had already been brought into radio by Lodge, before Marconi's four-circuit tuning invention.

The Pupin patents Nos. 519,347 and 640,516 show respectively a transformer and a wire line signaling system. Such extremely low audible current-frequencies as 150 and 250 cycles per second are suggested in the system patent (as at page 2, lines 70-74), and similar low frequencies are evidently contemplated for use with the transformer, al-

[fol. 1462] though no numerical values of frequency appear to be given in patent 519,347. Like the Hutin & Leblanc invention, these appear to be remote from radio, and include neither antennae, spark-gaps, coupled open and closed circuits, nor detectors. The ideas of resonance set forth by Hutin & Leblanc and Pupin were well known fundamentals of electrical engineering training by the time of the Marconi invention, and had been applied to inter-station tuning by Lodge. Their existence or statement in these patents, or in the text-books, seems to me to take nothing from the beauty, originality, or utility of the Marconi four-circuit tuning invention.

Q. 27. Please consider the Braun British Patents No. 1,862 of 1899 and No. 12,420, of 1899, also Mr. Loftin's testimony relating thereto, and state what bearing, if any, such patents have on any of the inventions of the Marconi tuning patent No. 763,772, as recited in any of the claims of said patent in issue in this suit.

A. I have read the two British patents to Braun, and Mr. Loftin's comment upon them at P. R., p. 1076, and I believe that these patents have no bearing upon the Marconi tuning invention. I note that Mr. Loftin makes no contention that primary and secondary circuits of the transmitter of 1862/1899 and primary and secondary circuits of the receiver of 12,420/1899 are tuned together or in pairs. This is in line with the diclosures of the patents themselves, for they contain no word or hint of tuning together the circuits at either the transmitter or the receiver. Thus that fundamental and extremely important feature of the Marconi tuning invention is missing from the Braun British patents. In addition, the disclosures of both patents are quite vague and indefinite. They give no impression that Braun had, or intended to have, any circuit tuning such as Marconi's.

Q. 28. Please consider "Defendant's Exhibits M-6 and N-6" which are the Braun U. S. Patents Nos. 797,169 and 797,544, applied for April 6, 1899 and May 18, 1903, respectively, both of which were granted October 15, 1905, together with the File Wrappers and Contents thereof, copies of which File Contents are herewith submitted to you, and state what the two applications for the Braun United States patents disclosed prior to the date of the Marconi invention (April 26, 1900), and also what relation such disclosures have to the disclosures of the Braun British pat-

ents you have just discussed in your answer to the previous question, pointing out what significant statements you find in the United States patents which were not included in the British patents, noting the date or dates when such statements were inserted in the applications for the United States patents to Braun.

A. The two U. S. patents to Braun, together show the figures of British 1862/1899. Those of the first U. S. Patent 797,169 correspond to Figs. 1 and 3 of the British patent, in which an "air-wire" or "transmitting wire" is directly connected to a Leyden jar. The drawings of the second or Divisional U. S. patent 797,544, correspond to Figs. 2 and 4 of the British patent, in which an elevated wire is connected indactively, or both inductively and directly, to the spark-gap. The specification of 797,169 is generally similar to that of the British patent, although it is considerably revised and contains a good deal of added material. The specification of U. S. patent 797,544 com- [fol. 1463] prises almost entirely the additional text which is not found in the British patent.

The original U. S. application, as it was filed on February 6, 1899, has its specification practically identical with that of the British patent. It shows no appreciation of the function of the antenna or radiating circuit, it does not show or mention a ground connection, and it has only an incidental showing of a transmitter having two circuits,—even as to this, the specification contains no suggestion of tuning. The entire original specification was cancelled and a new specification substituted by Paper No. 9 on July 22, 1902. Even this new specification contains no disclosure of the Marconi tuning invention. In the argument accompanying the new specification, Braun's attorneys pointed out that the waves from his transmitting wire were "not of its own frequency but of the frequency of the oscillation circuit", and thus different from Lodge. A further substitute specification was filed as Paper No. 11 on June 1, 1903, and here for the first time appears any suggestion that the invention comprised a closed reservoir circuit in combination with an open radiating circuit. I cannot find any statement that these two circuits should be tuned together, either in this final substitute specification or in the patent as issued.

The Divisional U. S. application, which resulted in patent 797,544, was filed on May 19, 1903, just before the third specification was filed in the parent case. It suggests that "by dimensioning properly the Leyden jars, the transformers, the coils, etc., the most favorable results will be obtained", and that the secondary coil "may be earthed", neither of which thoughts appeared in the original specification. On June 3, 1903, a substitute specification was filed in the divisional case, containing the same suggestion. The Examiner required cancellation of the provision that the transmitting wire or secondary might be "earthed", but evidently did not object to the "dimensioning properly" statement. This latter, which made its appearance in 1903, is the only hint of tuning that I have been able to find in either case. There is nothing in the least anticipatory of the Marconi tuning invention.

Q. 29. Are you familiar with the construction and mode of operation of the wireless or radio apparatus, including the quenched gap transmitters, used by the defendant herein, such as the wireless apparatus manufactured by the Navy, Wireless Specialty Apparatus Company, Telefunken Company, National Electric Supply Company, Wireless Improvement Company, Foote-Pierson Company, Simon, Lowenstein and Kilbourne & Clarke Company, as the same are described and illustrated by the proofs herein? If so, please state, as a question of fact, whether in the operation of such quenched gap transmitters there is any action which can properly be termed an "impulsive rush" or "single chunk" excitation which obviates the necessity of tuning the primary and secondary circuits of such quenched gap transmitters to syntony or resonance.

A. I am generally familiar with the apparatus to which you refer and believe that I have used or seen transmitters manufactured by most, if not all, of the organizations listed in your question.

The action of these quenched gap transmitters, not only in my own experience with them, but also as I under- [fol. 1464] stand the work of others, depends upon the resonant transfer of a series of oscillations, in the spark-gap circuit, to a tuned antenna circuit. The gradual transfer of energy in such a system extends over the time of from say five to ten or more complete oscillations, and is in no sense a "single chunk" or "impulsive rush"

transfer. Its character is such that efficient operation can be had only by careful tuning of the closed and open circuits, as set forth in the Marconi tuning patent.

Q. 30. In Mr. Loftin's answer to Q. 29 of his deposition, he discusses the United States patent to Seibt, No. 1,216,615, and on page 1102 of the Printed Record he makes the following statement:

"The foregoing clearly brings out that the quenched gap system is based upon the fundamental principles laid down by Lodge which I have previously quoted, namely, quickly dumping amount of energy into the radiating circuit, and as soon as this accomplished to electrically remove the dumping source from a maintained connection with the radiating circuit in order that the radiating circuit may be free to form this energy into a long drawn out train of very powerful one frequency oscillations."

State whether or not you agree with Mr. Loftin in making the above statement, giving your reasons for your answer.

A. I have read Mr. Loftin's answer to Q. 29, which is summarized in the portion of his testimony quoted by you, and disagree completely with several of his conclusions.

The system of the Seibt patent 1,216,615, which Mr. Loftin says is descriptive of the Wireless Specialty Apparatus type of quenched gap transmitter, depends fundamentally upon resonance between the primary or closed persistently oscillating circuit and the open or radiating circuit, as is characteristic of the Marconi patent 763,772. In Seibt's patent, at page 1, lines 64 to 66, he points out the basic requirement or assumption that the natural periods of these two circuits are to be the same and at page 2, lines 24 to 31, he states that if they are not in resonance with each other, only a part of the energy is transferred. Thus the delivery of power from the closed circuit to the open circuit depends entirely upon the resonance or tuning of Marconi, and takes place over a series of oscillations, as is further shown by Seibt's Figs. 2 and 4, which illustrate five complete oscillations before the so-called "quenching" is to take place. When Mr. Loftin says (at P. R., pp. 1096, 1097) that this system is "fundamentally" "based on the system disclosed by Lodge in his patent 609,154 of 1898, particularly the transmitter illustrated in Figure 4", if

he means to imply the presence of any "impulsive rush" action that might obviate the need of tuning primary and secondary to the same frequency in order to obtain efficient transfer of energy. Mr. Loftin is wrong. The Marconi invention and the disclosure of the tuning patent are not limited to any particular time of transferring energy from the closed circuit to the antenna circuit. Whether the energy is transferred "quickly" or "slowly", if resonance between the circuits is utilized to aid the passage of oscillating energy through the coupling system, then the Marconi invention is used.

Mr. Loftin's testimony seems to be based upon certain assumptions that are contrary to fact. Among these is his [fol. 1465] view that in the Wireless Specialty quenched gap transmitter there is some mysterious time relation between the circuits which, as he says at P. R., p. 1098, "is for a purpose having nothing whatever to do with a resonant transfer of energy". Further, he seems to think that the antenna alone determines the wave period. Mr. Loftin also builds up an elaborate theory of the co-existence in the circuits of a number of currents having multiple frequencies, which is not supported by any facts that have come within my experience or to my knowledge. He has attempted to put the operation of a quenched gap in the Marconi transmitter circuit into a class which he says is different from that of the Marconi patent, and which he contends is exemplified by an impracticable suggestion of Lodge, be a purely artificial differentiation. The facts are that the Lodge Figure 4 transmitter has been found to be of little, if any, value, that the Marconi transmitter is widely recognized to be of the greatest practical importance, that the quenched gap system does not operate by any "impulsive rush" or "sudden dumping" or "single chunk" of energy from closed circuit to antenna, and that the quenched gap system utilizes Marconi's circuit and his resonant transfer of energy from the closed to the open circuit. Consequently, I disagree flatly with Mr. Loftin's conclusion quoted by you.

Mr. Vaill: Counsel for claimant offers in evidence certified copies of the File Wrappers and Contents of the two United States patents to Ferdinand Braun, Nos. 797,169 and 797,544, and requests that the same be marked respectively "Claimant's Exhibit No. 311, File and Contents of Braun U. S. Patent No. 797,169" and "Claimant's

Exhibit No. 312, File and Contents of Braun U. S. Patent No. 797,544".

(The Notary marks said exhibits as requested.)

* * * * * * *

Q. 31. In answer to Q. 12 of his deposition, Major Mauborgne testified (P. R., p. 1220), during his consideration of the quenched spark transmitter, that the last adjustment of the circuit thereof:

"may cause the primary and the secondary to be slightly out of tune with each other, as can be demonstrated by a measurement of the primary if the antenna and ground be disconnected."

Please state in what respects you may agree or disagree with this statement, giving your reasons therefor.

A. Major Mauborgne describes correctly one method of adjusting any spark transmitter for the maximum effectiveness of radiation, but appears to be in error in his final [fol. 1466] statement that the "last adjustment may cause the primary and the secondary to be slightly out of tune with each other." In my experience this adjustment is most likely, if not certain, to bring the two circuits practically exactly in tune with each other so as to give the best resonant transfer of energy from the closed primary circuit to the open antenna circuit.

The demonstration that Major Maubourgne suggests, *viz.*, measurement *after* the antenna and ground have been disconnected, is obviously incorrect, for any such disconnection will necessarily remove from the system the substantial tuning elements contributed by the effective inductance and capacity of the antenna and ground. The mere act of disconnecting the antenna is bound to change the tuning relations in the system. No measurement made after such disconnection could possibly show the relative tuning conditions that had existed in normal operation, when the antenna and ground are always connected.

Q. 32. Please consider and compare the evidence relating to the transmitters used by the defendant herein as represented by the following exhibits:

Telefunken Type, "Plaintiff's Exhibit No. 79" (opposite P. R., p. 180);

National Electric Supply Co., Field Radio Pack Set, "Plaintiff's Exhibit No. 80" (opposite P. R., p. 192);

Wireless Specialty Apparatus Type, "Plaintiff's Exhibit No. 87 (P. R., p. 164);

Wireless Improvement Co. Type, "Plaintiff's Exhibit No. 88" (opposite P. R., p. 188);

Simon Type, "Plaintiff's Exhibit No. 89" (opposite P. R., p. 189);

Type manufactured by Navy, "Plaintiff's Exhibit No. 93" (opposite P. R., p. 187);

Kilbourne & Clark Type, "Plaintiff's Exhibit No. 95" (opposite P. R., p. 183);

which Mr. Weagant testified to be the apparatus installed on the S. S. Achilles;

Lowenstein Transmitter, "Plaintiff's Exhibit No. 101" (opposite P. R., p. 190);

Foote-Pierson Pack Set, "Plaintiff's Exhibit No. 102" (opposite P. R., p. 196);

and state whether, in view of Mr. Loftin's answers to Qs. 29, 30, 31 and 32 (P. R., pp. 1096-1124), of his deposition they are, in your opinion, substantially the same in construction, mode of operation and the results produced as the apparatus involving the inventions of the Marconi tuning patent No. 763,772, particularly as recited in the transmitter claims of said patent Nos. 1, 3, 6, 8, 11 and 12 and the transmitter apparatus included in claims 10 and 20, or any one or more of said claims, giving your reasons for such opinion as you may express.

A. I have considered the evidence and made the comparisons called for in your question. In my opinion, the several types of transmitters that you specify are substantially the same in their combination and arrangement of elements, their mode of operation and the results produced when in use, as the transmitters described in the Marconi [fol. 1467] tuning patent, and as set forth in the claims enumerated in the question, as follows:

The Wireless Specialty Apparatus transmitter shown in "Plaintiff's Exhibit No. 87" (opposite P. R., p. 164) contains the induction coil or power transformer of claims 1, 3, 11, 12 and 20, as shown at *c*. It has the condenser in the secondary circuit of that coil, shown at *e*, as called

for in claims 1, 3, 6, 11 and 12. The spark-gap G is the automatic means for producing oscillations, of claims 1, 3, 6, 8, 11, 12 and 20. The open circuit of claims 1, 3, 6, 8, 10 and 20 is shown as comprising the antenna *f*, A, the variable inductance *g*, the oscillation-transformer secondary coil *d'* and the ground or earth E. This open circuit is electrically connected with the spark-gap G by means of the oscillation-transformer *d*, *d'*, as required by claims 1, 3, 8 and 10. There is also a variable inductance in the open circuit, provided by the coil *g* with its tap 1, and coil *d'* with its tap 2, as called for in claims 1, 3 and 8. The generator in the primary circuit of the induction coil or power transformer, stated in claim 3, is shown at *a*. The "means for varying the primary circuit", also called for by claim 3, is the sending-key *b*, which is specified more closely as a "signaling instrument" in claims 11 and 12. The oscillation-transformer of claims 6, 8, 10, 11, 12 and 20 has already been referred to as comprising coils *d*, *d'*. This has its secondary coil, *d'*, connected in the open or antenna circuit, as in claims 6, 8 and 11. One end of the secondary *d'* is connected (through the coil *g*), to the antenna or radiating conductor A, *f*, as required by claims 6, 8 and 20; and the other end is connected to the capacity of the earth at E, as called for by claims 6, 8, 11, 12 and 20. There are provided, in the variable coils *d*, *d'* and *g*, the requisite means for adjusting the two circuits (closed and open), to accord or resonance, as explicitly stated in claims 6, 10 and 20, and as implied in the other claims by the specification. The primary *d* of the oscillation-transformer and the condenser *c* are in an electric circuit, and the condenser discharges its energy through the coil *d* and the spark-gap G, as in claims 11, 12 and 20. The antenna loading inductance coil *g* is connected to the end of the secondary *d'* opposite to the end that is connected to earth capacity, and the aerial conductor A, *f*, is connected to the inductance coil *g*, as called for in claims 11 and 12. Thus all the elements of claims 1, 3, 6, 8, 11 and 12, and all the transmitter elements of claims 10 and 20, in the specific combinations of each claim and operating as called for by the claims, are found in this Wireless Specialty Apparatus type of transmitter.

The Telefunken type of transmitter is shown in "Plaintiff's Exhibit No. 79" (opposite P. R., p. 180), and is very similar to the Wireless Specialty Apparatus type described

in detail above. The only differences seem to be that the induction coil or transformer C is connected across the terminals of the spark-gap G, instead of across the condenser *c*, that a one-coil or auto oscillation-transformer *d*, *d'*, is used instead of the two-coil variety and that an antenna condenser H, with shunting switch S is provided. None of these variations affect the operation of the apparatus with respect to the claims of the Marconi patent cited, and all the elements of all the claims, in the stated combinations, are present just as in the Wireless Specialty Apparatus type of transmitter.

[fol. 1468] The Kilbourne & Clark type shown by "Plaintiff's Exhibit No. 95" (opposite P. R., p. 183) is identical with the Wireless Specialty Apparatus type, except for the interchanged position of spark-gap G and condenser *c* (as in the Telefunken transmitter), and the addition of another means for adjusting the tuning of the closed circuit, *i. e.*, the variable inductance *d'*. The elements of all the claims appear, in the combinations of the claims, and the apparatus operates in accordance with the specification and claims of the Marconi tuning patent, as I have already explained. It may be noted that this "Kilbourne & Clark type" of quenched gap transmitter is the so-called "Navy Standard" made by Kilbourne & Clark to meet U. S. Navy specifications, and is quite different in its provision of tuning means, omission of the mercury-arc rectifier tube, etc., from the Kilbourne & Clark apparatus referred to by Mr. Loftin in his answer to Q. 37 (P. R., p. 1139).

The transmitter of the type manufactured by the Navy, shown by "Plaintiff's Exhibit No. 93" (opposite P. R., p. 187) is identical with the Wireles Specialty Apparatus Co. transmitter, discussed above, in all respects pertinent to the Marconi tuning patent. It meets the elements and combinations of the claims, and operates as specified in the claims, in the same way.

The same is true of the Wireless Improvement Co. type of transmitter shown by "Plaintiff's Exhibit No. 88" (opposite P. R., p. 188).

The Simon type of "Plaintiff's Exhibit No. 89", also opposite P. R., p. 188, is identical, except for the interchanged position of spark-gap G and condenser *c*, which affects neither the operation of the transmitter nor the appearance in it of the claimed combinations.

The Lowenstein transmitter of "Plaintiff's Exhibit No. 101" (opposite P. R., p. 190) is identical with the Telefunken type discussed above, except for the relative positions of spark-gap and condenser, and the omission of the antenna condenser H. These variations are immaterial with respect to utilization of the Marconi tuning invention, and my previous comments apply to this transmitter also.

The National Electric Supply Company type of field radio pack set, shown in "Plaintiff's Exhibit No. 80" (opposite P. R., p. 192), is identical with the Wireless Specialty transmitter, except that the antenna-circuit inductance coil *g* of claims 11 and 12 is functionally and in substance included in the inductance coil comprising the oscillation-transformer secondary *d'*. As explained in connection with the other transmitters, it thus involves the elements, combinations and operation of the transmitting apparatus of claims 1, 3, 6, 8, 10, 11, 12 and 20.

The Foote-Pierson pack set shown in "Plaintiff's Exhibit No. 102" (opposite P. R., p. 196), is like the National Electric Supply set just described, except for the use of a one-coil oscillation-transformer *d, d'*. The same comments and conclusions apply.

Mr. Loftin, in his discussion of infringement in answer to Q. 29, at P. R., pp. 1110 and 1111, states that if the inventions of the claims in issue are applicable to defendant's types of quenched gap transmitters, then the claims are also applicable to the disclosures of Lodge, Tesla and Stone. In my opinion, for the reasons already stated in my previous answers, the disclosures of Lodge, Tesla and Stone, in construction, arrangement of elements or mode of operation, are not anticipatory of any of the transmitter claims in issue.

[fol. 1469] Q. 33. Referring further to Mr. Loftin's testimony mentioned in the last question, will you please compare the receivers used by the defendant herein and represented by the following exhibits:

Type manufactured by the Navy, "Plaintiff's Exhibit No. 93" (opposite P. R., p. 187);

Wireless Specialty Apparatus Type, "Plaintiff's Exhibit No. 87" (opposite P. R., p. 164);

Telefunken Type, "Plaintiff's Exhibit No. 79" (opposite P. R., p. 180);

Kilbourne & Clark Type, "Plaintiff's Exhibit No. 95" (opposite P. R., p. 183);

Lowenstein Type, "Plaintiff's Exhibit No. 96" (opposite P. R., p. 191);

National Electric Supply Co., "Plaintiff's Exhibit No. 80" (opposite P. R., p. 192);

Foote-Pierson Pack Set, "Plaintiff's Exhibit No. 102" (opposite P. R., p. 196);

Fig. 89 of "Plaintiff's Exhibit No. 78," Federal Co. Receiver (opposite P. R., p. 212);

and state whether or not, in your opinion, the receiving apparatus therein illustrated and described in the testimony are substantially the same in construction, mode of operation and the results produced, as the apparatus involving the inventions of the Marconi tuning patent 763,772, as recited in claims 2, 13, 14, 16, 17, 18 and 19 of that patent, and the receiving apparatus recited in claims 10 and 20, or any one or more of said claims, giving your reasons for such opinion as you may express.

A. I have studied the testimony to which you refer and have made the comparisons requested. In my opinion, the several types of receivers to which the question refers are substantially the same in construction, in operation and in the results obtained when in operation, as the receivers described in the Marconi tuning patent and particularly claimed as the invention of the claims enumerated in the question, as follows:

The Wireless Specialty Apparatus Company type "I P 76" receiver is illustrated in "Plaintiff's Exhibit No. 87" (opposite P. R., p. 164). I have used receivers of this type in practical radio work, and am familiar with their design and operation. The receiver is designed for use with an antenna shown at A, *f*, of "Plaintiff's Exhibit No. 87," and this is the "oscillation receiving conductor" or "aerial conductor" of claims 2, 10, 13, 14, 16, 17, 18, 19 and 20. The variable inductance coil *g* and the variable primary coil *j'*, constitute the variable inductance in series with the antenna, as called for by claims 2, 13, 18 and 19. The detector T in the exhibit is the electrically connected wave-responding device of claims 2, 13, 14, 16, 17, 18, 19 and 20, the electrical connection being by way of the oscillation-transformer *j'*, *j²*. The tuning condenser *h'* connected to, or in circuit with, the wave-responsive device or detector, is the condenser of claims 2, 13 and 19. The pri-

mary and secondary coils j' and j^2 constitute the oscillation-transformer of claims 10, 13, 14, 16, 17, 18, 19 and 20. The aerial conductor A, f, is connected with one coil (j'), of the [fol. 1470] oscillation-transformer, as required by claims 13, 14, 16, 17, 18 and 19, and there is a capacity at the other end of the primary coil j' provided by the earth E, as called for in claims 13, 14, 16, 17, 18, 19 and 20. The variable tuning condenser h', and the variable tuning inductances g, j' and j^2 are the means for adjusting the primary and secondary circuits to resonance, of claims 10, 14, 15, 17 and 20 (specifically), and implicitly required by the other receiver claims in view of the specification. The variable inductance j^2 is in the circuit of the wave-responsive device, as called for by claims 18 and 19; and the battery B and telephone R constitute respectively the battery and "receiving instrument" specified by claims 19 and 20 as in the detector circuit. The only other elements of the claims considered, that bear upon the receiver, are the "closed circuit" connected through the oscillation-transformer secondary j^2 (in claim 10), which is provided by the tuning condenser h', and the shunt condenser connected in the antenna circuit and around the primary coil of the oscillation-transformer, as called for by claim 16. This condenser is added in practice, when the operator desires to receive long waves by use of a relatively small receiving aerial, in order to tune the primary circuit effectively by making up for the insufficient capacity of the small antenna itself. I have thus indicated all the receiving elements of claims 2, 10, 13, 14, 16, 17, 18, 19 and 20, in the combinations of those claims, as they appear in the Wireless Specialty Apparatus Type 1 P 76 receiver. Further, I know, from my use of these receivers, and of others similar to them, that their normal operation and the results obtained, are in accordance with the Marconi tuning patent.

The Telefunken type of receiving apparatus is shown at the right of "Plaintiff's Exhibit No. 79" (opposite P. R., p. 180). I have also used apparatus of this type and am generally familiar with it. It is used with the antenna or elevated conductor f, A, and comprises the variable antenna-circuit inductance j', the electrically-connected wave-responsive device or detector T, the condenser in circuit therewith h', the oscillation-transformer j', j^2 with the aerial connected to one end of one coil (j'), and the capacity or earth E connected to the other end, the means for ad-

justing the two circuits to resonance (comprising variable tuning condensers h and h', and variable coils j' and j^2), the variable inductance in the wave-responsive device circuit j^2, the receiving instrument or telephone R in the detector circuit, and the closed circuit connected to the oscillation-transformer coil j^2. I have already identified all of these elements with the claims in which they appear, in my comparison with the Wireless Specialty Apparatus Company receiver above. In this Telefunken receiver all the elements of claims 2, 10, 13, 14, 16, 17, 18 and 20 are founds in the combinations of those claims, and the operation of the receiver (and its results), are, in my experience, those of the Marconi tuning patent. The Telefunken receiver does not include the battery which is specifically recited as an element of claim 19, although this battery may obviously be added to the circuit in the usual way whenever a detector is used that requires or favors the use of such an additional source of electric potential. The modification of the Telefunken receiver shown on the exhibit chart E[4] does not contain the tuning condenser h' and, therefore, depends upon the variable inductance j^2 (in con- [fol. 1471] junction with the effective capacity of the rest of the closed circuit), for tuning,—otherwise, it is like the E[5] receiver which includes the condenser h'.

The Kilbourne & Clark receiver is shown in "Plaintiff's Exhibit No. 95" (opposite P. R., p. 183). This receiver is quite similar to the Telefunken and Wireless Specialty types just described, and contains all the elements recited as to both those receivers. The battery of claim 19 is shown at B, and the adjustable condenser of claim 16 appears at h. Thus this receiver embodies all the elements of claims 2, 10, 13, 14, 16, 17, 18, 19 and 20, in the combinations of these claims, and its operation and results are those of the Marconi tuning patent.

The Navy receiver of "Plaintiff's Exhibit No. 93" (opposite P. R., p. 187) contains the elements of all the claims, in the combinations of the claims, except the shunting primary condenser of claim 16 and the battery of claim 19, either or both of which may be added in practice, as indicated above.

The same is true of the Lowenstein receiver shown in "Plaintiff's Exhibit No. 96" (opposite P. R., p. 191), and of the National Electric Supply Company, "Field Radio

Pack Set" receiver, shown in "Plaintiff's Exhibit No. 80" (opposite P. R., p. 192).

The Foote-Pierson Pack Set receiver illustrated by "Plaintiff's Exhibit No. 102" (opposite P. R., p. 196) shows the battery of claim 19 and substitutes a one-coil or oscillation auto-transformer for the two-coil oscillation-transformer more commonly used in modern radio apparatus; the general functions of the two types of oscillation-transformer are, however, the same.

Thus, in all of these receivers there are provided the variable or tuning means of the Marconi tuning invention. The normal, intended and best mode of operation of all these receivers is that of the Marconi tuning invention. I have personally used receivers of all these types, either as made by the manufacturers named, or in substantially identical form, and I know that their operation does, in fact, utilize the tuning invention of the Marconi patent.

I find that I have omitted consideration of the Federal Co. receiver shown in "Plaintiff's Exhibit No. 78" (reproduced opposite P. R., p. 213). This receiver embodies all the elements of all the claims, in the combinations of those claims, with the exception of the adjustable condenser in shunt to the primary coil and the battery in the circuit of the wave-responsive device or detector, either or both of which may be added, as I have above described. The diagram does not indicate that the secondary coil of the oscillation-transformer is of variable inductance, but I know from my experience with these receivers that the coil must be made variable, as provided in claims 18 and 19, unless operation over a relatively smaller range of wave lengths is sufficient. In this case the variability of tuning provided by the adjustable condenser in the closed circuit may be adequate.

* * * * * * *

[fol. 1472] Q. 34. Mr. Loftin, in his answer to Q. 32 (P. R., p. 1119), and in his answer to Q. 33 (P. R., p. 1124), states that Mr. Waterman's sketch "Plaintiff's Exhibit No. 102", which is a diagram of the Foote-Pierson pack set, is incorrect in showing two windings in the oscillation-transformer and submits a sketch which, Mr. Loftin states, correctly illustrates the Foote-Pierson pack set, and also the Telefunken and Lowenstein sets. What have you to say on this subject?

A. Mr. Loftin has apparently misinterpreted Mr. Waterman's sketches of the Telefunken, Lowenstein and Foote-Pierson apparatus. He should have noted that the red and black coil indications in each of these circuits are printed one over the other, to indicate conventionally the presence of two circuits or current paths in a single coil or "auto" oscillation-transformer. This is clear from the printed reproductions of the exhibits, but is even more striking in the originals, as, for instance, that of "Plaintiff's Exhibit No. 102", showing the Foote-Pierson pack set, in which the red and black convolutions indicating the two current paths are exactly superimposed. There is no room for confusion on this point, for where the apparatus involved a two-coil oscillation-transformer (as in the Wireless Specialty Apparatus Company's transmitter and receiver of "Plaintiff's Exhibit No. 87", reproduced opposite P. R., p. 164) the two coils have been shown separated and side by side, and not superimposed in the drawings. To my mind, Mr. Loftin's criticism of this point is not warranted. Mr. Waterman has adopted a customary conventional method of representing the two circuits which co-exist in a single coil of a one-coil oscillation-transformer. As is well known, and as I believe Mr. Loftin would agree, the one-coil oscillation-transformer and the two-coil transformer, are alike as to their general functions. "Close coupling" or "loose coupling", as and when desired for the particular conditions of use, can be had with the auto-transformer, just as with the two-coil transformer.

Q. 35. Referring to Mr. Loftin's answers to Qs. 29, 30 and 32 of his deposition, wherein he compares the apparatus of the Marconi tuning patent No. 763,772 with the wireless apparatus of the Wireless Specialty Apparatus type, illustrated in Plaintiff's Exhibit No. 87" and other wireless apparatus used by the defendant, which the claimant considers to be infringements, and state wherein you may agree or disagree with Mr. Loftin's answers, particularly as concerns the conclusions reached as to the operation of said apparatus used by the defendant.

A. Mr. Loftin's long answer to Q. 29 (at P. R., pp. 1096-1109) sets forth a complicated theoretical argument attempting to distinguish between the Marconi patented apparatus and the Wireless Specialty Apparatus Company types of sending and receiving stations. I have also read his an-

swers to Qs. 30 and 32, in which he discusses the relations of the Marconi claims to the Wireless Specialty Company's [fol. 1473] apparatus and to other types used by the defendant. So far as I can determine, he does not deny that both or either transmitter or receiver embody the elements of the Marconi claims in the combinations of those claims. I understand, therefore, that in the application of the technical terms of the claims to the individual parts of the apparatus, as I have stated it in my preceding answers, there is no difference of opinion between us.

However, Mr. Loftin apparently does contend that the elements and combinations of the Marconi tuning claims are found in the prior art and specifically in Tesla, Lodge, Marconi's 1899 patent and Stone. I disagree flatly with this view, and have already set forth, in some detail, my reasons for this disagreement.

Mr. Loftin also appears to urge that (even though I assume that the technical meaning of the terms of the Marconi tuning patent claims is agreed to be descriptive of the Wireless Specialty Apparatus Co. and other instruments), there are certain fundamental differences in the mode of operation of the quenched gap transmitters that take them outside of the spirit of the Marconi tuning invention. I find no such contention as to the receivers I have referred to, and consequently will discuss them no further.

At pages P. R., pp. 1107 and 1108, Mr. Loftin has summarized what he contends to be the differences between the quenched gap transmitters of the Wireless Specialty Apparatus type of "Plaintiff's Exhibit No. 87" and what he calls the "open gap type" of transmitter of Marconi patent 763,772. These points may be briefly stated as follows:

Mr. Loftin seems to think that because the Wireless Specialty Apparatus transmitter (*a*) uses a quenched gap, (*b*) operates with relatively close coupling, and (*c*) follows the Seibt patent 1,216,615 in some respects, he is justified in concluding (1) that it has no persistently oscillating circuit, (2) that there is no resonant transfer of energy from closed to open circuit, (3) that the tuning adjustments required and provided, are not for the purpose of tuning, (4) that the transmitter operates with its two circuits deliberately de-tuned one from the other, (5) that the wave-frequency is "determined" by the antenna circuit and not by the closed circuit, and (6) consequently, that the apparatus

is a "single-circuit transmitter" like that of Lodge's Fig. 4 and not within the Marconi tuning patent.

These conclusions of Mr. Loftin seem to be based upon his statements in various prior answers which relate to his conception of the action of the quenched gap transmitter as compared to that of an open gap transmitter, to some of which I have already referred. His statements are apparently based upon incorrect assumptions of technical fact, which I do not find to be substantiated either by the literature of the art, or by my personal experience in the operation of transmitters using quenched and other types (including the so-called "open") *types* of gap.

Taking up Mr. Loftin's conclusions in order, I have already explained that the persistently oscillating circuit of the Marconi tuning patent is simply a circuit free from radiation loss of energy and, consequently, one that will oscillate for a relatively long time if its energy is not withdrawn for radiation or otherwise. The primary circuit energy in the Wireless Specialty Apparatus Company [fols. 1474] transmitter swings back and forth through from five to ten complete oscillations before being completely transferred to the antenna circuit (just as in the case of the National Electric Signaling Company transmitter which was previously enjoined under this Marconi tuning patent), and would oscillate for a much longer time were it not withdrawn by the antenna circuit to be radiated. The closed circuit comprising the condenser *c*, spark-gap G and primary coil *d* in the Wireless Specialty Apparatus Company transmitter of "Plaintiff's Exhibit No. 87" (reproduced opposite P. R., p. 164), is the same in elements, function and result as the similar closed circuit of the Marconi specification and claims.

Mr. Loftin's first contention is equivalent to saying that the quenched gap transmitter is based upon the principle of quickly dumping the energy of the primary circuit into the radiating circuit, as compared with the open gap system in which the energy is maintained for a long time in the primary circuit.

I do not agree with this first conclusion of Mr. Loftin as given in his summary, because, as a matter of fact, as I have previously pointed out, the action of a quenched gap transmitter is not based upon the principle of quickly dumping the energy of the primary circuit into the radiat-

ing circuit in a "single chunk". Mr. Loftin states that the action of the quenched gap transmitter is like that which he attributes to the transmitter of Fig. 4 of the Lodge patent, but, as I have previously stated, the quenched gap transmitter does not operate in that way. In its usual operation there are six or eight complete oscillations of the primary circuit before the gap is opened by the quenching action, and during this entire period the energy is being transferred from the primary circuit to the antenna or radiating circuit.

Mr. Loftin's second conclusion, that there is no resonant transfer of energy, is similarly not justified by the facts. The Seibt patent itself points out the necessary condition of resonance, or the agreement of circuit time periods, and both theory and practice show that in order to transfer energy quickly and effectively from closed to open circuits (as is done in the defendant's quenched gap transmitters), the resonant adjustment of the two circuits is essential. In my own personal experience with quenched gap transmitters it has always been my custom, with approximately correct coupling, to tune the primary and secondary circuits as nearly to resonance as possible, and then to adjust the coupling of the oscillation-transformer so that the maximum current in the antenna is obtained consistent with a clear note of the transmitter signal. This second conclusion of Mr. Loftin's is really a corollary to the statement given in his first conclusion, for if it were true that there was a quick dumping, impulsive rush, or single chunk transfer of energy from the primary circuit into the radiating circuit, then, of course, resonance between the circuits would be neither necessary nor desirable. But where the transfer is continued through a number of successive oscillations, as it is in the quenched gap transmitter, syntony must be obtained between the two circuits in order that there will be any efficient transfer at all.

Mr. Loftin's third conclusion, that the adjustable coils are provided in order to depart from resonance, rather than to attain it, is hardly to be taken seriously. If absence of resonance were the thing desired, it would be sufficient to leave the primary circuit adjustment fixed at a point out [fol. 1475] of resonance, and utterly unnecessary to readjust the primary coil when the wave-length was changed. But this is not what is done. The adjustments are provided

and used to attain the tuning adjustment that gives rapid and efficient resonant transfer of energy, as set forth by Marconi. Mr. Loftin in effect says that the arrangements for the adjustments of the two circuits of the two systems are for entirely different purposes, in the quenched gap system the adjustment being for the purpose of controlling the point of production of a "beat" of no energy in the primary circuit. To my mind he is entirely wrong in this third conclusion that he gives as a basis for establishing a difference between two types of transmitters. In the quenched gap arrangement the adjustment is not for the purpose of controlling the point of production of a "beat", but it is for the purpose of obtaining syntony or resonance between the two circuits with the proper degree of coupling to give the desired clear note. This signal note or spark tone depends upon the number of spark discharges per second and not upon any "beat" produced in the transmitter. As a matter of fact, the adjustments with the open gap transmitter and the quenched gap transmitter, in the practical use of either apparatus, are made in an identical manner and for the same purpose, *i. e.*, to get the maximum radiation of a substantially pure wave from the antenna circuit, together with a clear spark tone or note having the pitch of the spark-discharge frequency.

Mr. Loftin's fourth conclusion, that the circuits are deliberately operated "out of tune" with each other, is simply contrary to fact, as I understand the testimony in this case shows. My own experience in quenching gap operation agrees with the Seibt patent, and with general theoretical considerations, that operation with the circuits *in tune* is essential to the best results. Mr. Loftin contends that the quenched gap apparatus depends upon a tight coupling that will produce two frequencies sufficiently widely separated to obtain an "early beat", while the open gap apparatus depends upon a coupling relation between the two circuits so loose that the two frequencies will be merged into one. This is an incorrect statement of the fundamental reason for using a somewhat tighter coupling with the quenched gap than with the open gap. The main object of tighter coupling in quenched gap apparatus is to produce a greater transfer of energy from the primary circuit to the open or radiating circuit, and this is made possible by reason of the fact that the cooling action of the quenched gap quenches or causes the spark to "go out" before the energy of the

secondary or radiating circuit can be transferred back to the primary circuit. In other words, the group of oscillations in the secondary circuit is produced by a resonant transfer of energy from the primary circuit to the secondary circuit in a somewhat shorter time than is ordinarily possible with the open gap used at moderate or high powers. It is, therefore, merely a question of degree which relates only to the length of time or number of oscillations which take place while the energy is being transferred from one circuit to the other. This question of time varies and is entirely in accordance with the amount of quenching produced by the various types of gap with the particular amount of power and degree of coupling being used, from the open gap to the quenched gap, including the various forms of rotary gaps and those quenched by the use of blowers, as I have already pointed out.

[fol. 1476] The fifth conclusion, that the antenna circuit "determines" the wave-frequency, is merely a matter of words. If a wave-frequency of 500,000 per second is desired, the antenna will assuredly be tuned to the 500,000-cycle frequency, and so may be said to "determine" the frequency. But in order to obtain adequate resonant transfer of energy, the closed circuit will also be tuned to that frequency of 500,000 per second, and hence it may equally be said to be the determining element. Lodge had no closed circuit so tuned, and therein lies an essential difference between Lodge on the one hand, and Marconi, Seibt and the defendant's transmitters on the other. Thus, Mr. Loftin's fifth conclusion also seems to me to be unsupported by fact.

Mr. Loftin's sixth conclusion or contention, that the quenched gap transmitter is a "single circuit" apparatus and, therefore, results in a three-circuit tuned system when used with a two-circuit receiver, the frequency of the oscillations being determined by the radiating circuit with the primary entirely out of consideration, is a corollary to the second conclusion above mentioned. The second conclusion given by Mr. Loftin (that there is no resonant transfer of energy), is incorrect and it necessarily follows that his sixth contention is also incorrect. There is a resonant transfer of energy between the primary and secondary circuits of the quenched gap transmitter, since these circuits are in syntony with each other. The quenched gap transmitter is thus obviously a two-circuit apparatus, and results

in a four-circuit tuned system when used with a two-circuit receiver.

The six contentions advanced by Mr. Loftin may, as a matter of simple analysis, be reduced to one, *viz.*, that he contends that on account of a closer coupling between the primary and secondary circuits of the quenched gap transmitter a "beat" is produced by de-tuning, thereby preventing a resonant transfer of energy. On the other hand, the actual operation of the quenched gap transmitter, both theoretically, as well as practically considered, involves actual resonance between the primary and secondary circuits. This causes a resonant transfer of energy and permits a quenching of the spark in the primary circuit, so that when the voltage is reduced to a value below that necessary to maintain the spark, the spark "goes out" and the spark-gap "opens". The quenched gap transmitter is an improvement over the original open gap transmitter simply because at higher powers and with higher spark-frequencies, a closer coupling may be employed and, therefore, a greater amount of energy transferred more rapidly and more efficiently without destroying the purity of the signal transmitted. This is merely a matter of degree, and not a difference of substance, between transmitters using the two types of gap.

My theoretical knowledge of, and practical work in the art, and particularly with spark transmitters of all types, convinces me that (Mr. Loftin's statements notwithstanding), the quenched gap transmitter of the Wireless Specialty Apparatus Company, as illustrated in "Plaintiff's Exhibit No. 87", is fully within the spirit and substance of the invention of the Marconi tuning patent and involves the same principle and mode of operation. My detailed discussion of Mr. Loftin's comments upon the Wireless Specialty Apparatus Company transmitter, as given in this answer, is also fully applicable to the other quenched gap transmitters of the defendant.

[fol. 1477] Q. 36. Assuming that in some particular design of quenched gap transmitter, or that in some particular use of a quenched gap transmitter such as those used by the defendant herein, which the claimant considers to infringe the Marconi tuning patent 763,772, it had been found desirable to de-tune the antenna circuit so that its natural frequency differed from that of the closed primary circuit

to the extent of, say, two per cent, and that thereby the quenching action had been improved under the particular conditions then involved; how would the use of such adjustments affect the matter of utilizing the resonant transfer of energy from the primary to the secondary circuit of such transmitter by tuning, as disclosed in the Marconi patent?

A. Assuming the slight de-tuning of the antenna circuit stated by you (although I cannot see how this could improve the operation of any quenched gap transmitter), a transmitting set so adjusted would still utilize, and practically just as effectively as before, the resonant transfer of energy that I have already described. Unless the quenched gap transmitter were completely re-designed and re-organized in a way that (so far as I know), has never been done in the art, so as to cause it to produce a "single chunk", "impulsive rush" or sudden-blow excitation of the antenna, the oscillations in the closed circuit would swing through their normal five or ten complete cycles before transferring the bulk of their energy to the antenna circuit. In order to get this energy through the oscillation-transformer and into the antenna, it is necessary to use a sufficiently close identity or agreement of tuning of the two circuits to give the benefit of resonant transfer. Otherwise the energy will remain too long in the closed oscillation circuit, will not be transferred effectively to the antenna circuit, and will prevent the desired quenching action.

Q. 37. Assuming that, in the use of the transmitter portion of the inventions of the Marconi patent, and using an open gap, a reaction between the primary and secondary circuits was produced, resulting in a so-called "impure wave", but when a quenched gap was used such reaction did not take place and did not produce such a wave; would there be any difference in the mode of operation as to the tuned or resonant transfer of energy from the closed to the open circuit?

A. No, there would be no difference in this respect. The substitution of the quenched gap for the open gap would simply give the advantage of a more rapid cut-off of energy in the primary circuit, so preventing (under the assumptions made in your question), reaction from the antenna circuit upon the closed circuit. This would result in the radiation of a purer wave, probably of greater intensity and with a clear spark tone. There would be an improvement of efficiency in the transmitter, but the open and closed

circuits would still have to be tuned to the same frequency, so as to give the essential rapid resonant transfer of energy to the antenna circuit.

The matter of the purity of wave, to which your question refers, depends largely upon the degree of coupling between the circuits and upon the amount of quenching action developed by the gap, which latter is again dependent upon the amount of power used, as well as upon the tendency of the antenna circuit to react upon the closed circuit. No matter what particular adjustment or value of each of these various factors may be chosen for the conditions of opera- [fol. 1478] tion, it will, nevertheless, be necessary to tune the closed and open circuits to substantially the same frequency, in order to transfer the energy effectively. Moreover, no matter what conditions of adjustment are selected for practical use, there will inevitably be a certain, at least mathematical, and probably practical, impurity of the wave radiated. That is to say, the radiated waves can never be precisely and mathematically of a single frequency for the entire period of their radiation. If poor quenching and moderately close couplings are used, the wave radiated will have the so-called "double hump" or will radiate for part of the time at each of two predominant frequencies. How closely these frequencies of radiation coincide, and the relative effective intensity of the radiation at each frequency, will depend upon the adjustments of the transmitter. If maximum quenching, as attained in the practical quenched gap apparatus, is used, together with the appropriate coupling, the greater part of the radiated energy will be at a single frequency, but there will, nevertheless, be present two parasitic frequencies of somewhat different value for at least a portion of the total time of radiation. With the best quenched gap apparatus available, if the adjustment is such that the primary circuit is not "opened" upon the first resonant transfer of the energy to the antenna circuit (and thus, if power reaction between the circuits is allowed to appear), these extraneous or parasitic frequencies of radiation may become quite prominent and amply sufficient in intensity to produce harmful interference effects. No matter what type of gap is used, the best adjustment requires the maximum suppression of parasitic radiation frequencies and the maximum localization of the transmitted energy in the desired wave. This is true of the quenched gap, and was true of the apparatus used by Marconi at the

time of his invention. It is simply the proper method of using the resonant transfer of the Marconi patent.

Q. 39. Have you any personal knowledge of the construction and operation of the transmitters and receivers of the National Electric Signaling Company, such as is illustrated in "Plaintiff's Exhibit No. 105" (opposite P. R., p. 217), and described in Mr. Waterman's answer to Q. 30 of his deposition, beginning on page 215 of the Printed Record? If so, will you please state how you acquired such knowledge?

Q. Yes, I am familiar with the design, manufacture and use of these transmitters and receivers. I was employed by the National Electric Signaling Company for a number of years, and during that time my duties included the supervision and use of apparatus of this character.

Q. 40. Will you please state how the construction and mode of operation of the apparatus of the National Electric Signaling Company, referred to in the last question, compares with the construction and operation of any of the apparatus used by the defendant herein, and concerning which you have testified.

A. The National Electric Signaling Company transmitter of "Plaintiff's Exhibit No. 105" is substantially identical with the Simon transmitter of "Plaintiff's Exhibit No. 89" and (except for the interchanged position of the spark-gap G and condenser *c*), with the Wireless Specialty Apparatus transmitter of "Plaintiff's Exhibit No. 87". The National Electric Signaling Company transmitters used both [fol. 1479] quenched and rotary spark-gaps and, for emergency, plain spark-gaps, without other substantial change in the apparatus.

In my answer to Q. 32 I have compared the transmitter claims of the Marconi tuning patent with these other transmitters of defendant, and, in view of the substantial identity of the apparatus, the same comments and conclusions apply to the National Electric Signaling Company transmitter.

The National Electric Signaling Company receiver of "Plaintiff's Exhibit No. 105" is almost identical with the Wireless Specialty Apparatus Company receiver of "Plaintiff's Exhibit No. 87", discussed in my answer to Q. 33. In the National Electric receiver there are added the antenna tuning condenser shown immediately over the coil *g'*, its disconnecting (or short-circuiting), switch and the switch for the secondary tuning condenser *h'*. These variations do

not affect the functions or operation of the various corresponding parts, which are like those of the Wireless Specialty receiver. I may note that the National Electric operators frequently connected an antenna shunt tuning condenser across the primary coil j' of the receiver as indicated in the Telefunken and Kilbourne & Clark receivers of "Plaintiff's Exhibits Nos. 79 and 95". The comments and conclusions as to the relation between the receiving claims of the Marconi tuning patent and the construction and operation of these several receivers, as given in my answer to Q. 33, apply also to this National Electric Signaling Company receiver.

Q. 41. In your opinion, what significance is to be attached to the comparison between the action of pendulums and the coupled tuned circuits of the wireless apparatus of the Marconi tuning patent 763,772 as made by Mr. Loftin in his answer to Q. 21 (P. R., pp. 1023-1027)?

A. No significance should be given to the analogies as set forth and explained by Mr. Loftin, nor to the conclusions he has drawn from them, since his analogies are exceedingly imperfect and utterly insufficient to give even approximately a true picture of what occurs in the complicated electrical system of the Marconi tuning patent.

In the first place, mechanical analogies of electrical phenomena are notoriously dangerous, and must be used only with the greatest caution, if one is not to be led astray at the points where the analogue necessarily departs from the true conditions of the actual apparatus being studied. Carefully used, some analogies are helpful in analyzing even fairly complicated electrical effects, but before proceeding with them, one must be sure that his premises are sound.

One of the evident errors in Mr. Loftin's testimony appears at the bottom of P. R., p. 1023, where he says that it is well known that in transferring alternating current energy from one circuit to another through any form of coupling, there results a conversion of the one frequency into alternations into two frequencies. This, which he states as a general proposition, is true only of special cases. It is generally known, for instance, that the frequency of the alternating current of a power system remains unchanged regardless of the number of transformer interlinkages, and this fact is made use of in the operation of electrically-driven clocks which, for the accuracy of their

time-keeping, depend essentially upon a uniformity of frequency. Again, at P. R., p. 1024, Mr. Loftin refers to the effect of throwing a load upon a generator driven by a turbine or engine as analogous to coupled-circuit effects. [fol. 1480] The analogue is extremely far-fetched, for in systems of the type cited there are no resonant elements, and the well-known "hunting" or swinging in speed is caused by simple mechanical or load reactions. Still further, at P. R., p. 1025, Mr. Loftin says that if two pendulums are not of the same size, that is, if one is large and the other small "so that they do not have the same natural frequency of vibration", certain effects upon the rate of transfer of energy will be had. He should know that in a simple pendulum the natural frequency of vibration depends only upon the length of the pendulum and the acceleration of gravity, and that it has nothing whatever to do with the size of the pendulum-bob,—although this latter is, of course, of the greatest importance in considering the way in which energy will be transferred from one pendulum to another. Again, at P. R., p. 1026, his pendulum system utterly fails to reproduce conditions approximating the Marconi circuits, for Mr. Loftin has completely disregarded both the difference in persistence of the closed and open circuits and has failed to introduce any element corresponding in its action to the all-important spark-gap. Of his entire discussion the only statement that I find which I would care to endorse appears at P. R., p. 1027, where he says:

"If the second pendulum is made to have exactly the same natural period as that of the first, then these two energies are brought together to a common purpose, that of creating swings of one frequency, with the result that the extent or amplitude of swing is substantially increased."

This statement, although made in connection with limiting conditions which cannot be accepted as having any bearing on the operation of the Marconi system, is, nevertheless, an indication that Mr. Loftin realizes the fact that when one has an agreement between the natural frequency of the two elements of a coupled system, he will gain the benefits of resonant transfer of energy from one to the other and thereby get improved results.

I have myself used free and coupled pendulums for the study of some of the conditions in oscillatory electric cir-

cuits, but never in the way explained by Mr. Loftin in his answer to Q. 21. With proper precautions, such mechanical experiments show the advantages to be had by tuning the closed and open circuits of spark transmitters of any type, including those using the quenched gap. The particular points thus capable of demonstration are the rapid and efficient transfer of energy made possible by tuning the circuits together, the fact that each circuit can vibrate with only one frequency at a time (and never at two frequencies, as suggested by Mr. Loftin), and that quenching is materially assisted in a system using any type of gap by establishing resonance between the circuits since (as is also pointed out in the Seibt patent), *all* of the energy is thus transferred from one circuit to the other and the gap is permitted to "open" automatically without interference from a residuum of energy in the primary circuit.

There is one additional point, not commented upon by Mr. Loftin, which I might mention in connection with this discussion of pendulum experiments, *viz.*, that consideration of two pendulums hanging side by side and coupled together through an elastic band, or otherwise, can give one at least a partial understanding of why "single chunk" [fol. 1481] or impulsive rush transfer of energy from primary to secondary circuits has never been attained in radio practice. If we set one pendulum into oscillation by drawing it back and releasing it, it can obviously drive the secondary pendulum into full motion only by a gradual transfer of its energy through the elastic coupling link. To get a "single chunk" effect it would be necessary to strike the second pendulum a single violent blow, and it is perfectly clear that nothing of this sort can be accomplished through any elastic coupling that would permit freedom of vibration of the second pendulum. Being forced to transfer the energy over the time of at least five or ten complete swings, radio engineers have, since the time of Marconi, invariably taken advantage of the increase in effectiveness produced by resonant transfer. To approximate the "single chunk" excitation it would be necessary to devise an electrical system that would produce effects somewhat analogous to the striking of a single heavy pendulum with a powerful mallet, and thereafter immediately removing the mallet so as to prevent its interference with the swinging of the pendulum. Such a system would depend upon brute force, rather than sympathetic vibration to

produce its effects, and, so far as I know, has never been successfully approximated in the radio art.

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Cross-examination.

By Mr. Edwards:

XQ. 42. What specific object do you understand to be sought for in the Marconi patent No. 763,772?

A. That which Marconi states at page 1, lines 13 to 19 of the specification, viz.:

"to increase the efficiency of the system and to provide new and simple means whereby oscillations or electric waves from a transmitting-station may be localized when desired at any one selected receiving-station or stations out of a group of several receiving-stations".

XQ. 43. Can you translate this into non-technical language and tell us in a word what that object is?

A. Probably as good a brief statement as I can make is that the object is to improve the distance and effectiveness of radio transmission by providing a system that is more powerful, more reliable and more selective than the radio systems that had preceded it.

XQ. 44. When he speaks of increasing the efficiency of the system, does that indicate that he desires a more powerful system, *i. e.*, which, for a given in-put of energy would transmit to a greater distance?

A. I believe the reference to increased efficiency of the [fol. 1482] system is made broadly, that is, that Marconi there refers to improved performance and, therefore, a better practical efficiency throughout, as well as at various points of the system. One factor that would contribute to greater system-efficiency is the use of a more powerful transmitter and another would be the use of a transmitter having a high power-efficiency.

XQ. 45. But in a word, is not the specific result of increased efficiency the obtainance of greater distance of transmission?

A. That will depend upon what kind of efficiency you mean. If you refer to transmitter power efficiency, an increase may be accompanied by variations in other factors that would decrease effective transmission distance. If

you refer to over-all operating efficiency, your statement is correct. I might add that increased operating efficiency sometimes shows up as permitting greater reliability of transmission of messages, rather than greater distance alone.

X Q. 46. Well, do you agree that the statement of the object of the invention is "to increase the efficiency of the system" suggests that at least one object is to transmit to a greater distance?

A. Yes, the reliability of transmission and other factors being assumed constant.

X Q. 47. When the part of the specification which you have quoted refers to localizing the transmitted waves at one selected receiving-station, does that refer to selectivity?

A. Yes, selectivity of the receiver, as well as definiteness of radiation from the transmitter, is involved.

X Q. 48. That is to say, the means to insure the transmission of one selected frequency from the transmitter and the reception of that selected frequency at one or more selected receiving-stations?

A. Yes, in so far as the practical limitations of the apparatus permit.

X Q. 49. Do you understand that the attainment of selectivity is necessarily accomplished by the attainment of increased efficiency of the system?

A. I believe that an increase in selectivity is an important part of increased system efficiency, though the extent to which it is essential will necessarily depend upon the conditions of interference under which the particular system is operating at the particular time considered.

X Q. 50. Referring to the transmitting apparatus shown in Fig. 1, do you agree that the tighter the coupling between *d and d*¹, other conditions remaining the same, the greater would be the transference of energy from the closed circuit to the antenna?

A. Yes, that is generally true, though it would be more accurate to say that the tighter the coupling, the more rapid would be the transfer of energy. It should be noted that the question is hypothetical, because in practice other conditions in the transmitter will not remain the same when the coupling is changed.

X Q. 51. Do you agree that in this Fig. 1, other conditions remaining the same, that the tighter the coupling

between d and d^1, the more pronounced will be the double-wave transmitted from the antenna?

A. No, I cannot agree with the statement as you make it. The appearance of a double-wave depends upon too many factors other than the coupling. It is a fact that if conditions are such that a double wave can be observed during any part of the transmission time, the two frequencies will, in general, become more widely separated as the coupling between closed and open circuits is increased.

[fol. 1483] X Q. 52. Assuming in Fig. 1 that the LC of the antenna circuit is independently adjusted to be the same as or a multiple of the LC in the closed circuit, do you consider that a resonant transfer of energy will occur, irrespective of the degree of coupling?

A. Such an adjustment, if it takes into account the effective values of inductance and capacity of the frequency used, always gives, in my experience, a sufficiently close agreement of the electrical time-period of the circuits to permit a good resonant transfer of energy from one to the other. In some transmitters the effec ive inductance of either circuit is affected slightly as the coupling is varied, and where this is the case, a re-adjustment may further improve the resonant transfer. Such re-adjustments are, however, always very slight and often entirely unnecessary.

X Q. 53. Is my question X Q. 52 susceptible of a categorical answer Yes, or No?

A. Yes. I think the first part of my answer was, in effect, an affirmative answer, though I thought that the precautions which I understood to be implied by your question should be made clear.

X Q. 54. Do you agree that every circuit that is resonant has an electrical time-period of its own?

A. It has, if you mean that it has a frequency of oscillation for which it exhibits the least impedance. Some circuits containing inductance, capacity and resistance, are called "resonant", even though the resistance is so great that they will not oscillate freely if disturbed electrically and then left alone.

X Q. 55. Then do you agree that this time-period of the circuit is determined by the adjustment of the inductance and capacity of the circuit?

A. Yes, the inductance and capacity are the controlling factors, though in some cases the effective resistance may have a small effect upon the time-period.

X Q. 56. Now, if the time period of the closed circuit is independently adjusted and that of the antenna is independently adjusted, the product of the capacity multiplied by the self-induction of the respective circuits being the same, and the circuits be coupled together, will there be a resonant transfer of energy, no matter how tight the coupling?

A. There will, provided that the act of coupling the circuits together has not, in the particular apparatus under consideration, too greatly modified the effective values of the inductance or resistance in the two circuits.

X Q. 57. Do you place any limitation on the words "effective" and "too greatly" in your last answer?

A. No, I am using the phrase "effective inductance" in its normal engineering sense, which means simply the net amount of inductance that is effective in the circuit under consideration. As to "too greatly", I cannot assign any general numerical limits, but simply wish to make it clear that there may occur mathematically or precisely determinable variations that are of no consequence in practice.

X Q. 58. Do you understand that the Marconi patent teaches that the inductance and capacity of the several circuits are to be adjusted independently, as distinguished from adjusted by first coupling and then tuning experimently?

[fol. 1484] A. No. The instruction that the capacity and self-induction of the circuits are each and all to be so independently adjusted, which appears near the end of page 2 of the specification, refers to the adjustment of each element with respect to the others, and does not make the distinction excluding coupling and experimental tuning, which you state in the last part of your question. At page 3, beginning at line 12, Marconi points out that the adjustment "may be made in any convenient manner and employing various arrangements of apparatus, those shown and described herein being preferred". The apparatus shown in Figs. 1 and 2 is provided with coupled circuits and means for experimental tuning adjustments.

X Q. 59. Do you understand that it is following the instructions of the Marconi patent if the tuning be made without predetermined adjustment of the LC in each of the circuits, but by first coupling them and then adjusting the adjustable tuning elements g at the transmitter and g^1, g^2 at the receiver?

A. Yes, that mode of adjustment would be within those taught by the Marconi patent.

X Q. 60. You do not consider it essential that the two circuits be independently adjusted to a given time-period by calculation?

A. No.

X Q. 61. From your point of view it is quite sufficient to couple the circuits and by varying the adjustable elements, arrive at the tuning by trial?

A. Yes, though, of course, a preliminary calculation or experiment might be helpful.

X Q. 62. When you arrive at the tuning in this manner by trial, how do you know when the two circuits are properly adjusted?

A. From the fact that (considering the transmitter), the greatest antenna current, the purest wave and the best spark tone are had when the circuits are in tune; and (considering the receiver), by the fact that the greatest signal-response is had when the circuits are together tuned to the arriving wave-frequency.

X Q. 63. When you get the greatest response in this manner, how do you know what is the exact value of the LC in either of the two circuits?

A. You do not know it numerically, and you do not care for the numerical values as a practical operating matter. If you wish to determine them for design or other purposes, they may then be measured.

X Q. 64. How do you know, under those circumstances, how the value of the LC in one circuit compares with that of the value of the LC in the other circuit?

A. As a result of observation, a study of the work of others, or by direct measurement, you find that under those circumstances the electrical periods of the two circuits are the same for all practical purposes. This fundamental physical fact is continually used for the measurement of capacity and inductance.

X Q. 65. From your last answer do you mean the same, or do you mean substantially the same?

A. I mean either, that is to say, I cannot exclude either. Generally speaking, the electrical periods will be substantially the same, usually they will be the same within the limits of accuracy of practical measurements and in some

[fol. 1485] cases they will be even mathematically the same, so far as any laboratory method permits us to determine.

X Q. 66. In arriving at the tuning by trial in the manner referred to, you might, or might not, vary the degree of coupling; am I right?

A. It is not necessary to vary the degree of coupling in order to tune the circuits together, but in the practical operation of an apparatus provided with means for coupling variations, one might take advantage of that additional adjustable feature.

X Q. 67. If one did take advantage of that adjusting and varied the degree of coupling, would not that tend to unbalance the periodicity of the respective circuits to some extent?

A. Whether or not it would tend to do so, will depend upon the particular design of the apparatus in use. If the instruments are such that a variation of coupling inherently causes a change in the effective inductance of either or both circuits, as is sometimes the case in practical radio apparatus, there will be such a tendency.

X Q. 68. If an unbalancing does, in fact, result, do you consider that the respective circuits are still resonant with each other?

A. If the result of a change in coupling were to produce a divergence of period so great that the advantage of resonant transfer of energy were no longer had (and this is a limiting case which I have never experienced), it would not be accurate to say that the circuits remain in resonance with each other. On the other hand, if the change produced a variation that was determinable in the mathematical sense, or even measurable, but the circuits were left with such substantial agreement of their natural periods that the condition of resonance aided in the transfer of energy from one to the other, it would be correct to say that they were in resonance to that practical extent.

X Q. 69. In giving your testimony in this case have you had before you any figures as to the actual values of LC in any of the circuits in any of the types of Defendant's apparatus concerning which you have testified?

A. No, I have had no such data here before me. I have used, and measured the values of inductance and capacity in, certain of the apparatus referred to, and I have made many measurements on equivalent circuits corresponding to others of the instruments.

X Q. 70. Which of the circuits of which apparatus of the Defendant have you measured?

A. I have used an measured the elements of the Wireless Specialty Apparatus Company receiver, and, to the best of my recollection, I have had measurement data on the Telefunken receivers, and on the Navy receivers illustrated by "Plaintiff's Exhibit No. 93". I have also used and measured circuits corresponding to those of the pack set receivers. I have used and have had measurement data on the circuit elements of the transmitters of the type manufactured by the Navy ("Plaintiff's Exhibit No. 93") and the Wireless Improvement Company type ("Plaintiff's Exhibit No. 88"). I have also used and measured and worked from measurement data on transmitting circuits corresponding to the detailed variations shown as the Wireless Specialty, Simon and Lowenstein types, though I do not believe I have ever personally experimented with apparatus made by these three manufacturers.

X Q. 71. When did you make the measurements referred to in your last answer?

[fol. 1486] A. In the period within which I was employed by the National Electric Signaling Company and its successor, International Radio Telegraph Company; that is to say from about 1912 to about 1919.

X Q. 72. You did not have any of this data before you at the time of giving your deposition in this case, did you?

A. That is correct, I did not.

X Q. 73. And it is a fact, is it not, in giving your deposition you assumed from the various diagrams that if the circuits were adjusted to give best results, for example, transmission of a single frequency and reception of that frequency at a receiving-station, that then the various antenna and closed circuits would be such that the LC would be approximately the same in each of the circuits, did you not?

A. No, I am afraid that is hardly a fair statement. I have based my testimony only partially upon the diagrams to which you refer, and have relied mainly upon my experience with the apparatus itself.

X Q. 74. But you have assumed throughout, have you not, that the precise value of LC in any of the circuits is unimportant, and that it is sufficient if these values are approximate?

A. No, I do not think I have made any such assumption. The precise values of inductance and capacity in any of the circuits may be extremely important. I have assumed that in using any of the apparatus referred to in the case, the operator would follow the best tuning practice of the art, in order to get the best results of which the apparatus is capable. Such adjustment will, in my experience, invariably result in either a precise or a substantially close agreement of the electrical periods of the coupled circuits.

X Q. 75. If the coupling is made very tight, would the effect be negligible?

A. I am afraid I do not understand your question.

X Q. 76. You may assume the question to refer to the transmitter and that the other adjustments of the circuits remain the same?

A. No, increasing the coupling in a practical transmitter would be likely to increase the rate at which energy was transferred to the antenna circuit, and to increase the tendency of the antenna circuit to re-act upon the closed circuit. These effects would certainly not be negligible from either the theoretical or practical viewpoints. Depending upon the particular transmitter design, and the secondary effects of these two direct results of a change in coupling, there might, or might not, be additional practical variations produced to such an extent as would make them not negligible.

X Q. 77. Assume that the spark-gap of Fig. 1 of the Marconi patent No. 763,772 is a plain gap, then does it not follow that as the tightness of the coupling d, d^1 is increased you would increase the tendency to send out two waves?

A. The tendency to send out two waves, if I understand you correctly, would be increased as the coupling was made closer, without regard to what type of spark-gap was used. This is because in any given apparatus the tendency of the antenna power to react upon the closed circuit would be likely to increase with increase of coupling. Whether or not the tendency would result in the emission of parasitic waves would depend upon the antenna resistance, the gap condition, the spark-frequency and the amount of power used in the transmitter.

[fol. 1487] X Q. 78. The effect of the plain gap used with the tight coupling would be to prolong the effect of the two waves, would it not?

A. That would depend upon the gap condition and the amount of power being used, among other things. For each set of conditions there is usually some maximum coupling that can be used without causing "re-ignition" of the gap and consequent continued radiation of parasitic waves. If the coupling and power-relation are such that re-ignition occurs (which is not a desirable condition in practice), further increase of coupling or further increase of power may cause a greater proportion of the radiated energy to appear in the parasitic waves and this effect is, I presume, that to which you refer in your question.

X Q. 79. Well, do you not agree that, other conditions remaining the same, the effect of the plain gap as distinguished from the quenched gap, is to cause the "re-ignition" to continue for a longer time?

A. I would put it, if you do not mind, that with the quenched gap one can ordinarily use higher power, higher spark-frequencies and closer coupling than with the plain gap, still preventing "re-ignition" and the consequent substantial radiation of parasitic waves.

X Q. 80. Kindly answer my question as it is put.

A. I should have to disagree with it as it is stated, because of the impossibility of holding all conditions the same when one type of gap is substituted for another, and because the effect of "re-ignition" in producing parasitic waves depends more upon the number of times it occurs than upon the absolute time over which it continues.

X Q. 81. Do you not agree that, other conditions remaining the same, the effect of substituting a plain gap for a quenched gap would have the effect of causing the "re-ignition" to occur a greater number of times for each time the circuit is charged sufficiently to break down the spark-gap?

A. No, that is not an invariable rule such as could be accepted as a general statement covering all cases. That result would be had if the coupling and power conditions were such as to cause it, but otherwise not. I do not see how I can improve, as a general statement, my answer to X Q. 79.

X Q. 82. Do you agree that in Fig. 1, assuming the use of a plain gap, that in general the effect of a tight coupling is to send out two waves?

A. No. Whether or not reaction occurs that would produce the so-called "double hump" effect, will depend upon a number of factors you have not specified, including antenna-resistance, spark-gap condition, spark-frequency and power.

X Q. 83. What is the effect of the antenna-resistance?

A. An increase in antenna-resistance will reduce the power available to react upon the closed circuit, if there is a tendency for such reaction.

X Q. 84. Does the presence of resistance in a circuit change the time-period of the circuit?

A. It has a slight effect, as I think I have said, but in most radio apparatus the effect is so small that it is ordinarily neglected.

X Q. 85. The effect of the resistance is, in short, to cause loss of energy, is it not?

A. Resistance of a conductor causes conversion of the electrical energy passing into heat. Whether or not this [fol. 1488] is loss, depends upon the system being considered. In an antenna, for instance, an important component of the total resistance is the so-called "resistance equivalent of radiation" or, for short, "radiation-resistance". An increase of this component represents, in general, a gain in the transmitter efficiency, though it will produce the effect stated in my answer to X Q. 83, just as would any other increase in resistance.

X Q. 86. In answering X Q. 83 did you have in mind the ohmic resistance or the radiation resistance referred to in your last answer?

A. Both; I was referring to the total antenna resistance, which is what I thought was inquired about.

X Q. 87. In order to make your answer to X Q. 83 complete, would it not be proper to add that an increase in antenna-resistance would also reduce the power available to send out waves from the antenna?

A. No, for that would not be the fact. If the increase in total resistance were in the component represented by conductor-resistance, the antenna system as a whole would have a smaller radiation efficiency. On the other hand, if the increase in total antenna-resistance were in the component of radiation-resistance, the antenna system would have a greater radiation efficiency.

X Q. 88. In so far as the resistance comprises what you term "conductor-resistance", its effect is merely to cause

loss of energy both for transmission purposes and for reaction upon the closed circuit and is evidenced only by a flattened or broadened wave-form, is it not?

A. That is correct, unless by "merely" and "only" you mean to exclude other effects and evidences, such as the generation of heat. I assume that by "broadened wave-form" you mean a broadened resonance curve for the antenna system alone.

X Q. 89. What is the spark-gap condition to which you refer in your answer to X Q. 82?

A. That refers to the conditions which are affected by the size and shape of the spark-gap, the condition of the surfaces, its temperature, its ventilation, etc., that is to say, it is a generic term covering the design and operating conditions of the gap itself.

X Q. 90. Will you explain in what manner each of the factors which you enumerate in answer to X Q. 89 affect the matter of whether or not two waves are sent out?

A. Speaking generally, the smaller the electrodes, the smaller the flat areas in opposition, the less the mass of the electrodes, the rougher or dirtier the surfaces, the higher the gap temperature and the poorer its ventilation, the greater will be the tendency for re-ignition under conditions where re-ignition can take place.

X Q. 91. Assuming that you have a given spark-gap and other conditions remaining substantially the same, is it not true that the effect of loosening the coupling is, on the one hand, to tend to cause the double wave to disappear and only one wave to be transmitted and, on the other hand, to reduce the power of the set, *i. e.*, to cause loss of energy in the coupling?

A. The first part of your question is substantially correct, for I understand it to assume the existence of a substantial "double wave" under the initial conditions. With any spark-gap so operated as to show substantial parasitic waves, a loosening of the coupling will cause the frequencies of the two waves to approach each other, up [fol. 1489] to the point where the coupling is not too tight to prevent re-ignition. At that point the coupling-waves, so called, will substantially disappear and further loosening of the coupling will have little, if any, effect upon them. Change of coupling from too close a position, which I understand is the initial adjustment assumed by your question, to a looser value may either increase or decrease the

power radiated by the transmitter,—which way the power changes will depend upon the other factors I have referred to, including the type of gap, its ability to handle the power applied, etc.

X Q. 92. If in Fig. 1 of the Marconi patent the closed circuit is tuned independently to one periodicity and the antenna is independently tuned to the same periodicity and the two circuits are tightly coupled together, will the radiateded wave have the same periodicity?

A. Yes, unless the action of coupling some particular apparatus has the effect of changing the effective time-periods of the circuits. I am, of course, assuming that the power relations and the gap conditions are correct.

X Q. 93. In the case I have stated would not the period of the radiated wave differ from the period of the two circuits by a certain percentage?

A. No, not unless in the particular apparatus considered the circuit periods were changed by the coupling. As I have indicated, if the power and gap conditions were not proper, there might occur re-ignition which would result in at least some radiation of off-frequency waves.

X Q. 94. Have you any reason to suppose that the coupling would change the time-periods, as suggested by you in your answers to the last two questions?

A. Yes, that does occur in some cases. I have seen apparatus in which the placing of a secondary coil into coupling relation with a primary reduced the effective inductance of the primary, and consequently the period of the primary circuit, to a determinable extent.

X Q. 95. In my questions XQs. 92 and 93 I asked you to assume a tight coupling in Fig. 1 of the Marconi patent. With that understanding do you understand that the radiated-wave would have the same periodicity as that of the two circuits, or do you understand that the radiated-wave would differ from that of the two circuits by a certain percentage?

A. The radiated-wave, under the conditions I stated in answer to those two questions, would have the same periodicity as to the circuits. Any wave radiated from any antenna must have the frequency of the currents or oscillations flowing in that antenna.

X Q. 96. Do you consider that with the receiver of the Marconi patent No. 763,772 the tuning would be as good

if the closed circuit was tightly coupled to the antenna as it would be if it were loosely coupled?

A. No, for in general the selectivity of such a coupled receiving system increases as the coupling between the circuits is decreased, within practical limits.

X Q. 97. Referring to your answer to Q. 36, I understand you to say that in that instance de-tuning the antenna to the extent of say two per cent., would still utilize practically and just as effectively as before, the resonant transfer of energy; is this understanding correct?

[fol. 1490] A. Yes. My statement was "practically just as effectively as before" and applies to the hypothetical conditions set up in the question.

X Q. 98. If now, in this case, the closed primary circuit is tuned to a wave-length of 600 meters, would the radiated-wave be exactly the 600-meter wave, the antenna being de-tuned two per cent.?

A. I would expect the first part of each radiated-wave train to contain some energy at the frequency corresponding to 600 meters, some energy at some frequency depending upon the coupling and, perhaps, some energy at a frequency two per cent. different from that corresponding to 600 meters. In the latter part of each wave-train I would expect the radiation to be at a frequency some two per cent. different from that corresponding to 600 meters, for the conditions assumed were that "quenching action had been improved". This is a somewhat complicated case which, so far as I know, has not come within my practical experience, but general considerations of transmitter action point to the conclusion that the radiation would not be entirely of a single frequency. Nevertheless, the tuning of the two transmitter circuits together is sufficiently close, so that resonance would be of substantial assistance in transferring energy from one to the other. As I said in answer in Q. 36, I cannot understand how such de-tuning could improve the operation of the transmitter. My experience on this point agrees with the statement in the Seibt patent, to the effect that resonance between the circuits is essential for a rapid and complete transfer of energy and for good quenching.

X Q. 99. Would your answer to Q. 36 be the same if the antenna circuit were de-tuned say five per cent.?

A. It is impossible to state generally a numerical limit at which the benefit of resonant transfer is no longer had, for circumstances alter cases. I think it likely that at least in some transmitters a substantial advantage of resonant transfer would be had with the circuits de-tuned as much as five per cent., though it is difficult to say, because it is not a practical case and the operation of the transmitter would, in my experience, be tremendously improved, both as to quenching and radiated power, if the circuits were tuned more closely together. I would expect the radiation with a five per cent. de-tuning to be quite complex, if the apparatus were of the customary design.

X Q. 100. Are we to understand, however, that in any apparatus employing designs such as indicated in the various defendant's exhibits that de-tuning at two per cent. does not result in any diminution of the effective resonant transfer of energy?

A. No, I do not think that would be the fact in the practical apparatus. I believe that a two per cent. de-tuning would permit a fair resonant transfer, but that enough energy would be left in the closed circuit when reaction of the antenna commenced, to interfere with the proper quenching and, consequently, that the practical effect would be a spoiled spark-tone and probably a reduction of antenna power. If conditions could be had under which the two per cent. de-tuning resulted in improved quenching action (which is a condition quite outside my experience), I believe the operation would be practically as effective as before and that adequate resonant transfer of energy from primary to secondary would necessarily be had.

X Q. 101. Will you explain how the energy left in the [fol. 1491] closed circuit when reaction of the antenna commenced would, or could, interfere with the proper quenching?

A. One important function of any spark-gap is to "open" quickly, so as to prevent antenna energy from feeding back into the closed circuit. It is difficult, if not impossible, to design a spark-gap which, under service conditions, will retain its ability so to "open" unless the energy passing through it remains at a low or substantially zero value for a longer time than occurs during the powerful oscillatory swings of the charge immediately after the gap breaks down.

As Seibt shows, if the two circuits were tuned together, practically all of the primary circuit energy will be transferred to the antenna, and the gap will thus have adequate opportunity to "quench" or "open". If the circuits are not tuned to substantially the same frequency, the antenna power will start to react upon the closed circuit while the gap is still carrying the last few oscillations resulting from the first charge of the condenser. Thus, the time of zero or substantially zero power will not be sufficient to permit good quenching action.

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X Q. 102. In your answer to Q. 6 you referred to "this so-called 'single chunk' method" in referring to the Lodge patent. I note that you quote the words "single chunk". From what source did you obtain this quotation?

A. From the question, *i. e.*, Q. 6. I don't know where the term originated.

X Q. 103. Do you find any such quotation in the Lodge patent?

A. No, as I recall the patent, that phrase does not appear. The phrases used by Lodge are in his claim 6, and I think in his specification, and are by "aerial disruption" and "impulsive rush". Looking at the Lodge patent I find the portion at page 2, lines 72 to 95, inclusive, bearing on this matter and specifying that the electric charge is to be supplied to the antenna "in as sudden a manner as possible."

X Q. 104. Do you find anything in the Lodge patent which states that after the gap h^{10}, h^{11} breaks down there will not be more than one swing of the current before the gap opens?

A. The specification does not go into detail as to the action of what is called (at page 2, line 48), the "starting-gap" h^{10}, h^{11}, but the tenor of the description beginning at page 2, line 72, is to the effect that the antenna is to be charged "with an impulsive rush" and "in as sudden a manner as possible". The only function stated for the starting-gap h^{10} seems to be that its spark "precipitates a discharge at the supply gaps h^6 h^7 and suddenly supplies the capacity h h' with electric charges, which then surge through the [fol. 1492] connecting coil h^4 (divided into two parts in this figure) and spark into each other at the discharge-gap between the knobs h^2 h^3." This states fairly definitely that the action of the supply-gap, initiated by the starting-gap,

is to place a single impulsive charge upon the antenna, which charge thereafter oscillates in the antenna or capacity area system to radiate waves. Thus the specification does not cover the point as to which you inquire with reference to the spark across the starting-gap h^{10} h^{11}, but is definite as to the single impulsive rush which charges the antenna system through the supply gaps h^6 h^7.

X Q. 105. Do you find anything in the Lodge specification indicating that upon the occurrence of this impulsive charging of the antenna there cannot be a few swings of the current in the charging circuit?

A. The patent does not state what happens in the circuit comprising *k*, *j*, *j*, h^{10} and h^{11}, so that if this is what you call the "charging circuit", my answer would be in the negative. On the other hand, if you mean to include the whole circuit which charges the antenna system through the supply-gaps h^6, h^7, I think it is quite clear that Lodge intended the capacity areas to receive their positive and negative charges in a sudden manner, rather than by metallic conduction from the induction coil secondary, as in the Marconi re-issued patent. This idea excludes oscillations in the supply circuit including the gaps h^6, h^7. It is referred to at page 2, lines 18, *et seq.*, in addition to the portions to which I referred above.

X Q. 106. You say in your answer to Q. 6 that Lodge did not propose to take the spark-gap out of the transmitting antenna. Do you not think that that statement is too broad, in view of the statement on page 2, line 87 that, "it is permissible in the arrangement of Figs. 3 and 4 to close this last gap when desired, etc.", the statement referring to the "knobs h^2, h^3"?

A. Perhaps so, though I have regarded that particular suggestion as of a fugitive type that would certainly be exceedingly difficult of realization. The Lodge transmitters, as the art knows them, invariably have a series antenna spark-gap.

X Q. 107. You refer to the Stone patent 714,831 and refer particularly to Figs. 5 to 7, inclusive. What specific element or elements of the Marconi claims do you find to be absent from these figures?

A. Without attempting to refer in detail to each of the Marconi claims, I can say that the drawings taken by themselves do not establish the vital difference between Stone's arrangement and Marconi's tuning invention. Stone did

not tune his closed and open circuits together to secure resonant transfer of energy from one to the other, nor to utilize the selectivity of the four circuits, two open and two closed. This fundamental distinction does not appear from a simple wiring diagram, unless it be implied by the showing of variable tuning elements (which do not appear in the Stone drawings). I would not consider the showing of adjustable tuning elements to be essential in all cases where tuned circuits are intended, however, and consequently think that the distinction between Stone and Marconi should not necessarily be made on that basis. Your question, I believe, limits me to the Stone drawings, and I have answered accordingly. In my direct examination I have attempted to make clear my reasons for believing that Stone patents, as filed, and up to a time after the date of the [fol. 1493] Marconi tuning invention, did not include Marconi's four-circuit tuning.

X Q. 108. I think you do not question that Stone, in his Figs 5 and 6, discloses that the transmitting and receiving stations, with substantially the same circuits, and assembled in the same way as in the Marconi tuning patent, save with respect to the manner of tuning those circuits. Is that substantially correct?

A. The circuits are not dissimilar in their fundamental components and their mechanical arrangement, but their electrical functions and performance are substantially different, by reason of the tuning feature introduced by Marconi and the detailed changes which that would require.

X Q. 109. Are not Stone's circuits substantially identical with that of Marconi, except in the matter of tuning and such results as may flow therefrom?

A. Yes, if by "substantially", you exclude such differences as the antenna loading coil of Marconi's Figs. 1 and 2, the shunt primary condenser of Fig. 2, etc. I am referring, as before, to Figs. 5 and 6 of Stone patent No. 714,831.

X Q. 110. In short, if Stone had tuned the antenna of his transmitting circuit, with or without an adjustable loading coil, to be resonant with his circuit S, I_1, L C, and had likewise made the antenna at his receiving station resonant with the circuit I_3, C′, C L, his system would then have functioned, according to your understanding of the Marconi system; is that correct?

A. That is correct, as a brief statement, if by "antenna" at transmitter and at receiver, you mean the entire antenna circuit. It would do no good simply to use an antenna whose natural period, taken alone, agreed with the natural period of the associated closed circuit, and then to insert an inductance coil such as L_2 for associating the newly-formed antenna system with the closed circuit.

X Q. 111. But if he had included the coil L_2 in his antenna and then had adjusted the antenna as a whole, so that it would be resonant to the circuit C, S, L_1, L, then the system would have functioned as does the Marconi system?

A. Yes, if the tuned antenna system included the vertical wire V, the coil L_2 and the connection to the earth E, at transmitter and receiver, I believe the Marconi resonant transfer and tuning selectivity would have been had.

X Q. 112. You make no question, do you, but that Stone's circuit C, S, L_1, L, was a tuned circuit?

A. That is correct, that is to say, this was a circuit having a definite natural frequency which would be followed by free oscillations occurring in it.

X Q. 113. What governed the frequency of that circuit, or rather, what governed the selection of the frequency to which the circuit was tuned?

A. The frequency of natural oscillation in the circuit is determined primarily by the amount of inductance and capacity effective under the conditions of use, possibly modified to a small extent by the resistance of the circuit.

X Q. 114. Stone proposed to adjust the inductance L and capacity C of the circuit C, S, L_1, L so that the frequency of [fol. 1494] that circuit could be the frequency at which he desired the waves transmitted from the antenna to oscillate, did he not?

A. Yes, taking the inductance L to be the symbolic representation of the total inductance in the circuit (including the effect contributed by the coil L_1), and not merely the inductance of the coil marked L in Fig. 5 of Stone's patent, No. 714,831.

X Q. 115. There is on question in your mind, is there, but that Stone expected the waves which would be transmitted from the antenna to have the same frequency as the frequency to which the Circuit C, S, L^1, L was tuned?

A. That is correct. He proposed to force oscillations of this frequency upon the antenna system, and thereby to cause radiation of waves of the same frequency, uncompli-

cated by any tuning or natural oscillation of the antenna system.

X Q. 116. It is there, is it not, that you draw the distinction between Stone and Marconi; *i. e.*, that Stone proposed to force the oscillations from the circuit C, S, L_1, L upon the antenna, whereas Marconi would not force the oscillations from his corresponding circuit upon the antenna?

A. That is not quite a correct statement. The point is that the oscillations developed in the antenna by Stone's transmitter were to be forced without the benefit of resonant transfer and without utilizing the natural tendency of a tuned antenna system to oscillate sympathetically at the driving frequency, whereas Marconi's transmitter made practical use of this phenomenon of sympathetic resonance.

X Q. 117. Well do you say that Marconi did not intend to *force* the oscillations from the lateral or charging circuit upon the antenna?

A. That is correct, understanding the customary usage of that phrase as applied to the development of forced oscillations, *i. e.*, to working without the benefit of resonant transfer.

X Q. 118. Is it not fair to say that the substance of your last two answers is, in short, that in Stone the oscillations in the antenna are *forced* to the extent that the antenna is not tuned to the lateral or charging circuit, whereas in Marconi the antenna is tuned to the lateral circuit and to that extent the oscillations in the antenna coincide with the natural period of the antenna, and so are not *forced?*

A. Yes, that is a fair statement within practical limits, unless you are importing some special significance to the word "forced" which I don't understand.

X Q. 119. And if Stone's antenna were, in fact, tuned to the same frequency as his lateral or charging circuit, then the oscillations in his antenna would not be *forced?*

A. That is correct, speaking of the antenna system as a whole, and using "forced" in the customary sense. Of course, you realize that it has been proposed to analyze the oscillations resulting in any tuned circuit from the application of a practically synchronous driving force, into a "forced" component that rapidly dies away and a "free" component that builds-up and represents the final effective oscillation.

X Q. 120. I note that I have been referring to the Figs. 5 to 8 in Stone's patent 714,831. There is no question between us, is there, but that these figures are the same as the corresponding figures in Stone's patent 714,756?

A. I believe the four correspondingly numbered figures [fol. 1495] in the two patents are substantially, if not identically, the same. In the preceding questions, however, we have concentrated rather upon Figs. 5 and 6, and have not discussed Figs. 7 and 8.

X Q. 121. Now, still concentrating on Figs. 5 and 6, and referring more particularly to the receiving station, Fig. 6, may I assume that if Stone had, in fact, tuned his antenna system to the same frequency as that of the lateral circuit L_1, C″, C, L, that then his receiving system would have functioned in the same manner as the receiver set forth in the Marconi patent?

A. That is substantially correct.

X Q. 122. May I assume that you make no question but that the lateral circuit L_1, C″, C, L, was tuned to the same frequency as that of the wave which was to be received?

A. You may.

X Q. 123. And that frequency was the same as the frequency to which circuit C, S, I^1, L at the transmitting station was tuned?

A. That is my understanding.

X Q. 124. Is it your understanding that Stone deliberately detuned the antenna system at the receiving station, so that it would not be in tune with the wave which he expected to receive?

A. Yes, in line with his understanding that the simple or natural oscillation of the antenna system was not pure, and was therefore to be avoided in favor of forced oscillations for a highly selective system.

X Q. 125. To what extent do you understand he intended to detune the antenna at the receiving station?

A. To whatever extent was necessary, in order to avoid the effects he feared, and in general, even to the extent of providing a so-called "aperiodic" or untunable antenna system.

X Q. 126. Do you find in the Stone patent any direct statement to the effect that the antenna system is to be detuned, or the extent to which it shall be de-tuned?

A. Referring to patent 714,831, at page 2, beginning at line 15, Stone says:

"It is further the object of the present invention to enable the vertical or elevated conductor in such a system to be made the source of simple harmonic electromagnetic waves of any desired frequency independent of its length and other geometrical constants."

Noting that the matter which follows was added to the specification in 1902, I would point out that the emphasis upon the independence of the antenna constants is an instruction contrary to tuning of the antenna.

At page 3, beginning at line 6, Stone points out that in general the coupling together of tuned circuits results in a complex oscillation, which, on his premises, must be avoided in a selective system of the sort he proposes.

At page 3, beginning at line 33, he discusses natural vibrations of electrical systems as contrasted with "forced vibrations" which are "independent of the electromagnetic constants of the circuit" (lines 58 and 59), and goes on to show that the natural or tuned vibrations of an antenna system are "necessarily of a complex character" (page 3, [fol. 1496] line 81). He points out at line 91 a similar complexity of frequency in the natural oscillations of a receiving antenna system, which he points out introduce a defect in selectivity. He refers to the use of an aperiodic elevated conductor "adapted to receive or transmit *all* frequencies" in the text at page 3, lines 122 to 130, inclusive, which was inserted by amendment in 1902.

At page 4, beginning at line 42, he points out that for his selective system he causes the electric vibrations to be of a simple harmonic character "by producing what are substantially forced electric vibrations in the vertical conductor in lieu of producing natural vibrations" and thereafter he points out that he attains selectivity by interposing between the receiving antenna and the detector "a resonant circuit or circuits attuned to the particular frequency of the electromagnetic waves which it is desired to operate the translating devices."

Thus the broad instructions specifically direct the user to avoid natural or tuned oscillations in the antenna system, excepting in so far as that thought was introduced as an alternative by the matter inserted in 1902, to which I have already referred.

In his references to the figures at page 4, he points out that two frequencies may be transmitted from the same antenna

by the arrangement of Fig. 15, according to his system, and that waves of two frequencies may be received on a single antenna by the arrangement of Fig. 14. Alternative arrangements for this double use of the antenna system are shown in Figs. 15, 16 and 17, and obviously for proper results at frequencies which can be selected by the associated closed circuits, the antenna system must be detuned from both. At page 5, in describing the operation of the transmitter of Fig. 5, he points out that simple harmonic waves are produced by the forced vibrations in the antenna, and beginning at line 31 of page 5, he emphasizes the tuning effects of the closed circuit for selectivity.

In sum, the whole emphasis is upon the use of either a de-tuned or an aperiodic antenna system to avoid complex effects of multiple frequencies which Stone apparently conceived would interfere with the type of selectivity he sought. As I have pointed out, he later realized that "the vertical wire may with advantage be so constructed as to be highly resonant", and inserted such a statement in 1902, but the original disclosure is perfectly clear as to his intent to avoid any antenna tuning.

X Q. 127. Do you dispute the correctness of the matter stated in lines 6 to 32, inclusive, on page 3 of Stone patent 714,831?

A. This is a very general statement and, generally applied to some associations of simple circuits containing no spark-gap (such as shown in Stone's Fig. 1), the first part of it may be true. The specific numerical examples given refer to two conditions of coupling, by some unknown amount, of two circuits which initially had radically different natural periods. I know of no case in practical radio that corresponds to this complex situation. Whether or not Mr. Stone's figures are correct, would depend upon a number of conditions that he has not defined, though I have little doubt that it would be possible to assume some set of conditions which would lead, at least mathematically, to the figures which he gives. I disagree with his last statement, if it be read to imply the contemporaneous existence [fol. 1497] in any one circuit of more than one resultant current, though, of course, it is recognized that a complex current which is changing as to frequency or otherwise may, over a period of time, be analyzed into several com-

ponents which are not co-existent during briefer intervals, and if this is the interpretation to be given that passage, I can accept it. The general statement "the presence of each simple circuit modifies the natural period of each of the other circuits with which it is associated" is too broad to be true of all cases, for it depends upon many factors, including the way in which the circuits are coupled, whether or not either of them includes a spark-gap or other damping element, their relative resistances, etc. As an illustrative or explanatory comment applying theoretically to the association of simple circuits such as those shown in Fig. 1 of the Stone patent, and without complicating factors, it can be accepted as at least instructive, though necessarily not complete.

X Q. 128. Well, do you disagree with the general proposition in that part of the specification just referred to, that if two simple circuits, each like the circuit shown in Fig. 1, be tuned to have a natural period of its own when isolated, that when these circuits are inductively coupled, as indicated in Fig. 3, there will be an increase or decrease of the natural period of the two circuits?

A. Yes, I cannot accept that as a completely general proposition. For example, you will note that in the instance used by Mr. Stone to explain his theory, circuits with two different natural periods "when isolated" are given, and it is stated, beginning at line 25, that the inductive association of the circuits together has increased the natural period of the high-period circuit and decreased the natural period of the low-period circuit. It should be clear that such a teaching cannot be applied to a practical radio case in which the isolated or initial periods of the two circuits are the same, for there is then no "high-period circuit" to have its period increased, and no "low-period circuit" to have its period decreased. Both start alike, and the opposing tendencies discussed by Mr. Stone should result in no change, unless some other complicating factor were introduced. I have already pointed out, in answer to your earlier question, that if the coupling of two circuits having initially the same period resulted in a change of the effective self-inductance of either circuit, a corresponding change in period might be expected. This is, however, outside the case which Mr. Stone appears to be considering at this portion of his specification.

X Q. 129. In the case you mention, that is, where the initial frequencies of the two circuits are the same, then the coupling would, by reason of the reactive effect of one circuit upon the other, tend to give the wave-form a double hump, would it not?

A. I think you are referring to an effect upon the resonance-curve that may be drawn to illustrate the currents in two simple coupled circuits under some conditions, which will show a "double hump" or double frequency effect. If two simple circuits are coupled as in Fig. 3 of the Stone patent, and if the coupling is sufficiently close, a disturbance of one circuit may result in a natural oscillation of that circuit, part of the energy of which may be transferred resonantly through the inductive coupling to the second similarly tuned circuit. Under some conditions, the current in the driving circuit will be slightly reduced in frequency, while that in the driven circuit will be slightly [fol. 1498] increased, until the energy has substantially passed from circuit 1 to circuit 2. Then, still assuming the simple conditions of Fig. 3, the second circuit becomes the driving circuit and its oscillations may be correspondingly slowed down while it is retransferring the energy through the coupling back to the first circuit. During this interval, the first circuit, being one that is driven, may oscillate slightly faster than its normal frequency. Thus both circuits are the seat of alternately recurring currents whose frequency gradually shifts by a slight amount above and below the natural oscillating frequency, and some of the time they will oscillate at one value and some at the other. There is an intermediary condition in which they will both oscillate at the natural frequency. In such a simple case, uncomplicated by resistance effects, etc., the divergence between the driving and driven frequencies will ordinarily be negligible at moderately loose coupling but will become increasingly great up to the point where the two frequencies may be separately measured, as the coupling is increased.

X Q. 130. Do you agree with the correctness of the statement in the Stone patent 714,831 on page 5, beginning line 90, and ending in line 104?

A. That is an incomplete statement of part of the proposition which I set forth in my last answer, and in so far as it applies to the same conditions and re-states the same

matter in different words, I agree with it. You should note that in this specification, written a good many years ago, Mr. Stone has not drawn the limiting conditions as closely as I did in my preceding answer, nor as I believe he would draw them today.

X Q. 131. Do you agree that the addition of the inductance coils L, L in Figs. 5 and 6, as set forth on page 6, lines 46 to 61, is the same thing as saying that the antenna and lateral circuits at the transmitting and receiving stations are loosely coupled?

A. Yes, generally speaking that is correct. The effect of inserting a loading coil not coupled to the antenna is two-fold, in that in general the time-period of the circuit is increased and the coupling to the associated circuit is decreased. Quantitatively the effects will vary with the constants of the particular case, but as a rule the effect will be to "loosen" the coupling of the circuits, even if not to make the coupling "loose" in any attempted absolute sense of that word.

X Q. 132. I take it that your view is that when Stone on page 6, lines 68 to 72, says that "the vertical wire may with advantage be so constructed as to be highly resonant to a particular frequency, and the harmonic vibrations impressed thereon be of that frequency" that he here means that only the wire and not the coil I_2 is to be of that frequency. Is that your view?

A. I think I pointed out the ambiguity of that particular statement by Mr. Stone near the end of my answer to Q. 23. The statement is a true statement as of 1902, when "vertical wire" is taken to refer to the entire radiating circuit, including the coupling coil and ground connection. It is not a true statement if taken to mean the wire alone. My view is that by 1902, Mr. Stone appreciated these facts and inserted the statement into his specification, to be read as an instruction that the entire antenna system might be tuned with advantage, this then being well known.

X Q. 133. That is, you understand the quotation which [fol. 1499] I have just made from the patent 714,831 to mean that the entire antenna system may with advantage be made resonant with the lateral circuit?

A. You are adding another element not here mentioned by Mr. Stone, namely, the "lateral circuit." My view is that the statement in the patent is ambiguous, but that if

read on the entire antenna circuit, is true, and by 1902 was known to be true; whereas, if read on the vertical wire alone, is not true, and by 1902 was known not to be true.

X Q. 134. And is it your view that prior to 1902, it would not have been known to be untrue unless it referred to the entire antenna system?

A. I am not sure that I understand your question, but my view is that prior to the date of Marconi's invention (some time prior to 1902), it was not recognized that an antenna system might, with advantage, be so constructed as to be highly resonant to a particular frequency and made part of a system in which that particular resonant frequency was impressed upon the antenna system (in a transmitter), or withdrawn from the antenna system (in a receiver) by resonance system. I doubt that anyone, either prior to 1902, or since, would seriously have pressed any advantage supposed to be attained by making a vertical wire, by itself, highly resonant, though, as Stone suggested, he might, in some cases, have wished to radiate waves having a frequency corresponding to the fundamental of the particular Marconi vertical wire antenna which some one of his transmitters was intended to replace.

X Q. 135. Is it your view that one reading the Stone letters of June 30, and July 15, 1899, would understand that his reference to making the fundamental of the wire at the receiving station to be the same as that of the wire of the transmitting station and his reference to the period of the impressed force being the same as that of the fundamental of the vertical wire, are intended to draw a distinction between the fundamental of the wire itself and that of the coil connected in circuit with the wire?

A. It is not a matter of a distinction between the fundamental of the wire itself and that of the coil connected in circuit with the wire, but rather a distinction between the fundamental of the wire itself as it would have been used according to the Marconi re-issue patent and the natural or tuned period of vibration of the entire antenna system, including the vertical wire, the coupling coil and the connection to the ground. My view is that the conclusion of an unprejudiced scientific reader would be that these two statements (referred to by you in the question), are not entirely clear when taken by themselves, but that the balance of the description contained in the two Stone letters

makes them clear and sets up the distinction that I have specified at the beginning of this answer.

XQ. 136. The Stone antenna system was composed of nothing but a wire and an interposed coil, was it not?

A. So far as one can judge from the diagrams in the letter dated June 30, 1899, the antenna system comprised a vertical wire, a serially-connected coil and a ground connection.

XQ. 137. The ground connection was a wire connecting the coils to ground, was it not?

A. That is correct, though, of course, sometimes copper strip or other conductors are used. I mention it simply [fol. 1500] because its constants must be considered in addition to those of the vertical or aerial wire, in tuning the antenna system as a whole.

XQ. 138. And the coil itself was a wire, was it not?

A. Yes, ordinarily, though strip conductors are also used frequently in coil design.

XQ. 139. So that the entire antenna consisted of a wire, and nothing but a wire, did it not?

A. Yes, as a matter of words, though the statement of the question contains an unsound implication.

XQ. 140. And your view that Stone did not intend to tune the antenna system to the lateral circuit is based upon your view that his reference to tuning the wire of the antenna, or making the fundamental of the antenna wire the same as that of the lateral circuit, meant that he only made the fundamental of that part of the wire outside of the coil to be the same as that of the lateral circuit. Is not that substantially correct?

A. No, I have tried to point out that the reference to the fundamental of the wire, taken as a matter of words by itself, is not clear. My conclusion that it refers to the vertical wire, and not to the antenna system as a whole, is based on the additional statements in the Stone letters, which emphasize the tuning of the closed circuit, that the simple oscillation of a vertical wire does not produce a pure wave, that in his arrangement the current developed in the wire is not due to the oscillatory discharge of the wire, and that the current is a forced oscillation, depending as to frequency only upon the impressing force, and not upon the electromagnetic constants of the circuit in which the currents are finally developed.

XQ. 141. I do not think that you quite apprehend my question. I am not asking you as to why you think Stone

did not tune his antenna system as a whole to the lateral circuit, but I am asking you if it is not a fact that your view that Stone did not intend to tune the antenna system to the lateral circuit is based on your understanding that Stone's reference to the fundamental of the antenna wire indicated only that part of the wire outside of the coil?

A. I think that those are both conclusions which are based upon the other statements in the Stone letters, and that neither conclusion is based upon the other.

X Q. 142. Well, I take it you will agree with me that if the reference in Stone's letters meant that he would make the fundamental of the antenna system as a whole the same as that of the lateral circuit, then he described a system such as you understand the Marconi system to be; will you agree with me that far?

A. Yes; based on your assumption, though I consider it inconsistent with the Stone letters.

X Q. 143. Well, will you go further, and agree with me that your view that Stone did not intend to tune the antenna system as a whole to the lateral circuit, is based upon your view that his reference to making the fundamental of the antenna wire the same as that of the frequency impressed upon it, meant only that he would make that part of the wire outside of the coil the same frequency as the frequency impressed upon it?

A. No, I have tried to say that I considered both to be conclusions and both based upon the balance of the Stone [fol. 1501] disclosure. It is true that they are co-ordinate conclusions, so to speak, and that they are consistent conclusions, but I think they support each other only to that extent. Both are based upon the emphasis on freedom from dependence of frequency upon antenna constants, etc., which I have already outlined.

Last Question by the Notary: Do you know of any other matter relative to this claim?

Answer: No, nothing occurs to me that would be pertinent.

Mr. Vaill: Counsel for claimant offers in evidence the following photographs of apparatus used by Mr. Wilson Aull, Jr., during his deposition, as included in his testimony given

in answer to questions 71 to 77, inclusive, and it is requested that said exhibits be marked respectively as follows:

* * * * * * *

"Claimant's Exhibit No. 315 (Ultra Audion Circuit and Two-Electrode Tube of Aull Question 74)":

* * * * * * *

"Claimant's Exhibit No. 317 (Crystal Acting as Oscillator of Aull Question 77)."

(Said exhibits so marked by Notary.)

It is stipulated and agreed by and between counsel for the respective parties that the above-mentioned "Claimant's Exhibits Nos. 313 to 317," inclusive, are photographs of the apparatus used by Mr. Wilson Aull, Jr., in his demonstrations referred to in answer to questions 71 to 77, inclusive, of his deposition, and correctly represent the arrangements of said apparatus; that said photographs were taken under his direction shortly after the making of said tests and demonstrations and that if said Wilson Aull, Jr., were recalled as a witness, he would so testify.

Edward W. Vaill, Of Counsel for Claimant; C. V. Edwards, Special Assistant to the Attorney-General.

[fol. 1502] *Deposition of Frank N. Waterman, recalled as a witness on behalf of the claimant in rebuttal, taken in New York, N. Y., on the 23rd day of December, A. D. 1927.*

First Interrogatory by the Notary: State your name, your occupation, your age and your place of residence; whether you have any, and if any, what interest direct or indirect, in the claim which is the subject of inquiry; and whether, and in what degree, your are related to the claimant.

Answer: My name is Frank N. Waterman; age, 62 years; residence, Summit, New Jersey; occupation, Consulting Engineer; I have no interest, direct or indirect, in the claim which is the subject of inquiry, and am not related in any way to the claimant.

Direct Examination.

By Mr. Vaill:

Q. 1 Are you the same Frank N. Waterman who has heretofore given a deposition on behalf of the claimant during its *prima facie* case?

A. I am.

Q. 2. Do you find that the specification of the Marconi tuning patent No. 763,772 provides or requires that the oscillations in the primary circuit of the Marconi transmitter be long-drawn-out or persistent for a long time, and that the specification includes "loose coupling" instead of "tight coupling"? Also please state whether or not you find that Mr. Marconi considered that his system required "loose coupling" in view of his address before the New York Electrical Society of April 17, 1912, as reported in the Transactions of the Society of November 15, 1912, pages 4 to 29, inclusive ("Defendant's Exhibit J-6"). In connection with your answer, please consider Mr. Loftin's testimony in answer to Q. 21, beginning at page 1017 of the Printed Record, his answer to Q. 22, beginning at page 1052 and his answer to Q. 27, as found on page 1068 of the Printed Record.

A. I do not. The facts are to the contrary.

I have read the testimony referred to and will give my reasons for disagreeing with the opinions expressed.

Mr. Marconi founded a wholly new art. The patent in suit constituted the second great step and so complete was it that the organization disclosed in the patent has been used in substantially all spark transmitters and been the basis of the receiving apparatus employed with such transmitters down to date.

There was no established language or terminology for this art; the patentee had to be his own lexicographer.

To understand the extraordinary novelty of the subject matter dealt with in the patent, it is necessary to bear in mind the fact that Marconi in his patent, which is re-issue 11,913, had disclosed a new phenomenon in the electric arts in general. In the ordinary practice of the electrical art a path for an electric current had to be closed. Current did not flow in an open circuit.

[fol. 1503] Marconi hung a vertical wire in the air, as disclosed in this earlier patent, and caused high-frequency currents to flow between the top of this wire and the earth.

In order to accomplish this, he interposed a spark-gap in the wire and put a charge on the upper insulated part of it, which surging across the spark-gap when the charge became great enough to break down the insulating quality of the gap, caused high-frequency oscillating currents to surge back and forth between the earth and the top of the wire.

This was an amazing phenomenon, and, still more amazing, he found that this wire caused electric waves to be radiated like invisible light from a lighthouse, which followed the surface of the earth.

To designate this circuit by reference to its electrical character, he called it a "radiating circuit" or a good radiator.

This arrangement established the art of wireless telegraphy, or, as we say today, radio. The receiving arrangement consisted of a similar vertical wire in which there was interposed a suitable detector. This wire converted the received waves into high-frequency electric currents. Because of their high frequency, an indicating instrument could not directly respond to them and the detector was employed to bring about a useful response to these received waves in a suitable indicating instrument.

While the invention of this earlier patent was of extreme utility, it had very marked limitations, and efforts were made to overcome or minimize these limitations. It was found that the presence of the spark-gap in the transmitting antenna and of the detector in the receiving antenna, were chief or important factors in causing the limitations. Efforts were accordingly made to get rid of these elements in the transmitting and receiving antennae.

Marconi solved the problem in the patent 763,772, inquired about. In doing so, Marconi recognized that he must avoid at the transmitting station, using two circuits, both of which were capable of radiation and at the receiving station he must avoid two circuits, both of which were capable of directly "picking up" the received waves.

Lodge, in his patent No. 609,154, which otherwise made a great contribution to the art, made the mistake in his Fig. 4, of getting a combination which afforded two radiating circuits. The arrangement was useless, therefore, and has never been employed.

Marconi, in the patent in suit, took the spark-gap out of the antenna and put it into a circuit which could oscil-

late, but could not radiate. He also took the detector out of the receiving antenna and put it into a local circuit which could oscillate, but could not directly "pick up" the received waves effectively.

To distinguish the local circuit into which he put the spark-gap from the radiating circuit which had the power of quickly giving up all its charge in the form of radiated waves, he called it a "persistent oscillator" and described it as being "a good conserver" in distinction from the elevated conductor which he described as being "a good radiator of wave energy."

Mr. Loftin has ignored this manifest meaning of the terms used, and by giving to the expression "persistent oscillator" a meaning out of harmony with all the language and purport of the patent, has built up a deposition which, according to my view, is without meaning or significance, and wholly out of harmony with all of the facts.

[fol. 1504] The distinction between Marconi's "good radiator" and his "persistent oscillator" is simple, intelligible and non-technical. The first gets rid of its energy by radiating it; the energy leaves the system completely and never comes back. The "persistent oscillator" if operating by itself, cannot get rid of its energy by radiating, but the charge imparted to the condenser must oscillate back and forth through the circuit until its energy frittered away in heat, due to the resistance in the circuit, much of which is in the spark-gap. In other words, by itself this circuit is a totally useless thing.

Marconi made the two circuits into a single structure of amazing utility, by "coupling" the two together so that the energy which was first stored in the local "persistent oscillator" circuit could be imparted to the antenna or "good radiator" circuit and he enabled the energy to be transferred from the first to the second circuit quickly and effectively by tuning the two circuits to the same wave-length, by making the product of the self-induction multiplied by the capacity, the same in each of the two circuits. Only by the tuning of the two circuits together in this manner can the energy be transferred quickly and efficiently from one circuit to the other.

The concept, which is fundamental to this invention, is that the local circuit can have a large condenser capacity for a given wave-length, as compared to that of the antenna which is relatively small. The amount of electrical energy,

that is, the size of the charge, that can be put into a given capacity, depends upon the electrical pressure or voltage that can be used to force the charge into it. This electrical pressure, however, has to be appropriate to the spark-gap. Because the capacity of the antenna of Marconi's prior arrangement of re-issue patent 11,913 was necessarily small for a given wave-length, the charge that the suitable spark-gap pressure could impart to it was small. By putting the spark-gap into a local circuit which could have a relatively large capacity included in it, the same spark-gap voltage or pressure was able to impart to the local circuit a correspondingly larger charge.

In other words, the local circuit being a good conserver, having a large capacity, was made by Marconi's arrangement to act as a reservoir for the energy which was to be subsequently radiated, and also it could contain such energy as was necessarily, that is, unavoidably, to be lost in the spark-gap and other unavoidable circuit-resistances.

In the art of transmitting power by wire, when two circuits were linked or coupled together for the purpose of transferring energy from one to the other, it was found necessary to have iron core transformers with the coils as intimately associated as possible, so that the coupling between them was the closest attainable and, in general, higher than ninety-nine per cent. The transformer, for example, whose coupling was as low as eighty-five per cent, would have been quite useless, except for very special purposes.

At the time of the Marconi patent in suit, the technique of this art was not developed to a high state. There were available few methods of measurement. In order to disclose his invention, Marconi was compelled to resort to practical examples of it and through these practical examples, he discloses the fact that he can efficiently transfer energy from one circuit to another by his method, with couplings between the two circuits of only twenty per cent, or less, [fol. 1505] and at the same time get efficient energy transfer, thus enabling the antenna to effectively receive and radiate the energy stored in the local circuit.

When these two circuits—the "persistent oscillator" and the "good radiator" circuits were tuned to the same wave-length, *i. e.*, product of inductance and capacity, and associated as disclosed in the Marconi patent in suit, the "good radiator" preserves its capacity of radiating, but in a

greatly improved form, due to the absence of the spark-gap; while the local "persistent oscillator" becomes endowed with the ability to quickly transfer its energy to the antenna.

Because the antenna no longer contains a spark-gap, it can be charged to a much higher voltage than the spark-gap could tolerate; because the energy is first accumulated in the local circuit, the energy can be handed over to the antenna circuit in a number of parcels, so to speak, by a number of current surges through the local circuit and thus the voltage of pressure in the antenna need not be too great for its insulation to withstand, even though the antenna radiates a total charge which, if imparted directly to it, would have made the voltage impracticably high.

In other words, such an elevated conductor as was disclosed in the early Marconi patent above referred to, had two limitations to the charge which could be imparted to it. One was due to the spark-gap, which could not have a very high voltage; the other was due to the insulation of the antenna and the insulating capacity of the air in which it was suspended and it, therefore, could not have too high a voltage, even if the spark-gap could have tolerated it.

Marconi solved both of these problems. The spark-gap problem he solved by putting the spark-gap in the local circuit, to which he gave a large capacity, so that it could store a large quantity of energy; the problem of getting sufficient energy into the antenna to produce a signal which would carry for a long distance, he solved by associating the local circuit with the antenna in tuned relation, so that the energy could be transferred in more than one swing or oscillation, and by using several oscillations to transfer it, it could pass to the antenna while the latter was radiating and thus the building up of an excessive voltage could be avoided.

As I have stated, the effectiveness of this method was so great that it came into substantially universal use, and has remained in use to this day, for substantially all spark-gap transmitters. I have read the testimony of Mr. Weagant and Mr. Hogan on this subject and agree fully therewith.

Mr. Loftin has based his entire deposition on this patent upon the assumption that the expression "a persistent oscillator" which the patent uses at page 1, line 38 and again at page 2, line 14, is to be understood with the signi-

ficance which he himself imposes, and entirely apart from the definitive significance of the patent itself, where it is obviously merely a term of contrast. What the patent clearly sets forth is that the open or antenna circuit is endowed with the unique property of radiating capability. In contrast to this the local circuit, by itself, is endowed only with the oscillating capacity.

It follows from this, of course, that each circuit necessarily oscillates—again considering each by itself—until its energy has disappeared. How long this will be, depends upon what electrical characteristics each circuit has, that is, upon the relations existing between the capacity of the circuit to receive a charge and its inertia and resistance [fol. 1506] elements, known respectively as the "inductance" and the "resistance." The resistance in the case of the antenna includes what is known as its "radiation resistance," this being a way of numerically expressing its capacity to get rid of energy in the form of electric waves.

How long the closed circuit would keep on oscillating until the charge got too small to continue to pass across the spark-gap, would depend, therefore, not upon any particular emphasis placed on the words of the patent, as Mr. Loftin seems to think, but upon the electrical characteristics of the circuit—still considering it by itself.

Aside from the description of function which the patent contains, and which is very clear, the patentee describes how to build a number of different arrangements to realize the invention. Mr. Marconi is evidently a very thorough and a very practical man, and the instructions which he gives show careful working out of the actual apparatus. From these instructions I was able, many years ago, to reconstruct and test the apparatus described, and did so for a number of the arrangements tabulated on page 4 of the patent, and I found them to work as described. To give an approximate idea of the behavior of the local circuits of these arrangements with respect to the number of oscillations, I have calculated the number of oscillations that might reasonably be expected in these circuits, that is to say, in the primary circuits of tunes 1 to 4 on page 4 of the patent, using the figures given in the table and my own actual measurements of the wave-lengths radiated. To do this it is necessary to know the equivalent resistance of the spark-gap, by which I mean that resistance which

would dissipate the same energy in the same number of oscillations.

Marconi, in the patent in suit, shows spark-electrodes in the form of spheres. Referring to page 185 of the work entitled "Principles of Electric Wave Telegraphy and Telephony," by J. A. Fleming, 1906 Edition, I find curves giving the resistance of spark-gaps between ball electrodes of iron, zinc or brass, and I have taken the data for zinc or brass as giving the lowest resistance and, hence, the largest number of oscillations. Using the length of spark-gap given by Marconi on page 4 of his patent, I find that the number of oscillations which these primary circuits would have, when not connected to the antenna, and assuming that the spark-gap would cut off the oscillations at ten per cent. (an arbitrary assumption commonly made), would be as follows: For tune 1, 12 oscillations; for tune 2, 5 oscillations; for tune 3, 15 oscillations; for tune 4, 5 oscillations. I produce a chart illustrating these oscillations. (Claimants Ex. 319.) These figures, of course, are only illustrative and they will vary with the nature of the spark-gap and the efficiency of the condensers and coils. I have assumed that all of the resistance is in the spark-gap (I may say that the same data regarding spark-gaps, just above referred to, is found on page 185 of the Edition of 1906).

When the two circuits are in operative relation to one another, then the energy from this closed or local circuit is not permitted to exhaust itself in the resistance of the circuit, and the energy is rapidly transferred to the antenna circuit, except in so far as some of it is consumed in the resistance of the primary circuit. It follows that since the spark-gap resistance remains the same the energy will have been imparted to the antenna in fewer oscillations than would occur in the primary circuit oscillating by itself. This is a universally recognized fact and I find that it is [fol. 1507] agreed to by Mr. Loftin in his testimony in this case.

We may find out how many oscillations would be required to transfer the energy of the primary circuit to the secondary, radiating circuit, by a formula which I find given by Prof. Morecroft on page 247 ("Claimant's Exhibit No. 321") of the work entitled "Principles of Radio Communication," published by John Wiley & Sons, Copyright 1921, provided that we know the couplings. While I have

actually measured these couplings, it was many years ago, and I am not now able to find the note-book in which the records were made. I have, however, a definite independent recollection of the first and the third tunes. The first was between ten and eleven per cent. and the third was between nineteen and twenty per cent. Adopting these figures, and using Prof. Morecroft's formula, just referred to, I find that the numbers of oscillations required to transfer all of the energy from the primary to the secondary circuit in tune 1 is five, and for tune 3, two and a half.

It will be seen, therefore, according to the Marconi patent in suit, the number of oscillations is not some indefinitely large number, as seems to be fancied by Mr. Loftin. It is a definite number, determinable from a knowledge of the coupling.

I have a definite recollection that in these tunes the range of coupling was from five to twenty per cent., substantially. It follows that the range of the number of oscillations in the Marconi patent as given by the numerical illustration, is from ten down to two and a half. This is not a matter of opinion, therefore, as to which Mr. Loftin may have one view, and I may have another; it is a matter of fact. So far as the illustrations of the patent go, the persistently-oscillating circuit referred to in the patent is one which, in association with its antenna, has from two and a half to ten oscillations, and in tunes three and one has respectively two and a half and five.

* * * * * * *

In the foregoing I have not referred to transmitter tunes 5 and 6, nor can I, because I have not the data. As to tune 5, it is possible to draw reliable inferences from the data given in the table on page 4 of the patent; noting that tune 5 uses the same transformer as tune 3, it is possible to roughly infer the wave-length from the capacities and to infer the coupling from the transformer. By this process of inference and by extending the curve of spark-gap resistance in the Fleming book above referred to, it appears that the number of oscillations in the closed circuit when not coupled to the antenna is of the order of five. Assuming that the coupling is of the same order as that of tune 3, [fol. 1508] then it appears that the number of oscillations in the primary circuit when coupled to the antenna is of the order of two and a half. With respect to tune 6, I note

that the transformer employed is transformer No. 3, described beginning at line 41, page 3. This transformer has ten turns in each of its two coils and they are wound one over the other. The coupling is therefore close, since there is no added inductance in the antenna, and all the inductance of the primary circuit is coupled to the antenna. I have no recollection of what this coupling was, as a matter of fact, but judging as a matter of experience, it is certainly closer than ten per cent. The number of oscillations in the primary circuit before the energy was all transferred to the antenna would be five or less.

From the foregoing it appears that Mr. Loftin's attempt to make the term "persistent oscillator" mean a circuit which, when coupled to the antenna, has very many oscillations in it, is unwarranted. It is unwarranted in view of the language of the patent. It is unwarranted in view of the descriptive contrast which Marconi was drawing between the two circuits which he proposed to associate, and it is explicitly excluded by the illustrative examples which Marconi gives of embodiments of his system.

Marconi discloses in the patent in suit an arrangement by which he associates a radiating circuit with a non-radiating circuit and he obtained an efficient transfer of energy from the non-radiating circuit to the radiating circuit by tuning the two circuits to resonance. Only by so tuning them could this quick transfer of energy be obtained. The mathematical demonstration by which Prof. Morecroft discusses the operation of the circuit includes the resonance of the two circuits, and by resonance he means the thing defined as such in the Marconi patent in the passage beginning at line 118, page 2, viz., that the product of the self induction by the capacity is the same in each circuit. In stating the formula which I have above referred to, and which determines the number of oscillations required to transfer the energy, he says:

"If the two circuits are properly tuned all of the energy will have been transferred to circuit 2 in $\frac{1}{2}$ *k*. cycles;"

Marconi, of course, was entitled to make the association of the two tuned circuits anything that he pleased. If the two circuits were respectively a closed circuit having little or no radiating capacity, and an open circuit having a radiating capacity, and if the two were tuned together, so that the product of inductance by capacity in one, was the same

as that in the other, and the spark-gap was in the closed circuit, then the arrangement would seem to be that disclosed by the patent, without regard to the coupling. The word "coupling" in this connection was not known at the date when the patent in suit was written. To disclose constructions such as he had in mind at the time he resorted to examples of operative arrangements, which he described in detail.

It is a remarkable fact that the range of coupling covered by these examples is, in fact, the range which continued commercial use of the apparatus since the date of the patent, has proven to be most desirable.

The matter of coupling is wholly a matter of degree; it expresses the extent to which the local reservoir circuit [fol. 1509] is loaded by its association with the antenna. In an absolute sense all coupling used in a radio circuit is loose coupling. It rarely exceeds the twenty per cent. which is the tightest or least loose coupling employed in the illustrations given by Marconi. The expressions "loose coupling" or "tight coupling" merely refer, when applied to radio circuits, to which half of this range given by Marconi, the coupling is in. If it is less than ten, it will often be found to be referred to as "loose"; if it is more than ten, it is sometimes referred to as "tight." But whether it is called "tight" or "loose" as applied to radio circuits, it is always extremely loose by comparison with what is used in circuits for wire-transmission of electricity, such as power and light circuits.

What particular coupling will be used in any given transmitter is a matter of choice with the constructor. If what he wants is efficient radiation of energy he will so choose his apparatus that when the energy has once been transferred to the antenna it will remain there to be radiated, and will not be transferred back to the local circuit to be wasted in the spark-gap and other circuit-resistances. Whether this will happen for given coupling and circuit proportions, depends upon the amount of energy to be radiated and the particular construction of the spark-gap. One cannot successfully send a boy to do a man's work in radio, any more than in any other line of endeavor. That is to say, a spark-gap which might successfully handle a given energy-charge, cannot be expected to handle equally successfully ten or twenty times that charge.

At the date of the Marconi patent in suit, the ordinary source of energy for charging the condenser *c* in the local circuit was a large spark-coil. The energy which it could supply was small. For that purpose the ball type of spark-gap could be used with considerable satisfaction, but in his earlier patent, re-issue 11,913 at page 2, line 7, Marconi pointed out that:

"If a very powerful source of electricity giving a very long spark be employed, it is preferable to divide the spark-gap between the central balls of the smaller oscillator into several smaller gaps in series."

As the art has developed, larger and larger quantities of power have been demanded and constant effort in devising spark-gaps able to handle larger quantities of energy have been exerted. In these constructions cooling is of paramount importance and if a gap is properly cooled, the energy, when once transferred to the antenna, is not returned to the local circuit, and the phenomena that occur are such as are illustrated in my sketch called "Waterman Sketch No. 2" found opposite page 151 of the printed record. No form of spark-gap that I am acquainted with will give this most desirable type of operation if it is over-loaded by using too much power on the circuit or coupling it too tight to the antenna. The ball type shown in the Marconi patent, the series of balls referred to in the re-issue patent, as just above noted, the rotating gap and the parallel plate type of gap in the form of the so-called quenched gap, or otherwise, will all give this type of operation of my "Sketch No. 2", when operated within their proper loads.

Mr. Loftin's testimony differs from mine on this subject, because he insists on imposing on the Marconi patent [fol. 1510] conditions which do not exist, save in his own imagination, and because of a number of misconceptions on his part.

Mr. Loftin imposes upon the Marconi patent a condition which he calls "cumulative resonance"; I refer, for example, to page 1023 of his deposition in the printed record. Because he insists on defining resonance in a way in which it is not defined in the patent, he draws numerous unwarranted conclusions and inferences. He states, for example, that if the two circuits of the patent are so coupled that they re-act on one another, then they do not have this cumulative resonance and, hence, are not within the Marconi

patent, as he interprets it. He forgets that in order to transfer energy from one to the other effectively, as pointed out in the patent, they must re-act on one another. Marconi has definitely arranged them to transfer energy rapidly and efficiently.

Marconi has stated in explicit terms what he means by having the circuits in resonance. He means that the product of inductance by capacity shall be the same for the two circuits (page 2, line 118). There is no warrant in the patent for Mr. Loftin's restrictive limitation to what he calls "cumulative resonance".

Another restriction which he insists on placing upon the patent is that the lateral circuit must produce oscillations which are transferred "without change in characteristics" to the antenna. This is an entirely meaningless expression which is not contained in the Marconi patent. Mr. Loftin's idea seems to be that Marconi made the radiated oscillations more persistent by the use of what Mr. Loftin calls a persistently oscillating circuit. He seems to think that the persistency which he imagines for this circuit is in some way imparted to the antenna. Of course, everyone familiar with the art knows that nothing of the sort happens. Marconi obtained greater persistence of oscillations from the antenna by taking the spark-gap out of the antenna and by the use of the Lodge loading coil. The art has followed in his footsteps and done the same thing.

Mr. Loftin appears to be of the impression that because the circuits are in resonance, therefore the energy cannot be transferred quickly from one circuit to the other. This is exactly the reverse of truth. It is because they are in resonance that the energy can be, and is, transferred extremely quickly, as, for example, in from two and a half to ten oscillations in the illustrative examples given in the patent. In this connection I call attention to the fact that the number of oscillations which might occur in the local circuit by itself has very little to do with the number that will occur before the entire energy has been transferred to the antenna when the two are coupled together. For example, in tune 3, the local or persistent oscillator circuit has fifteen oscillations as shown in my diagram produced in this answer. When coupled to the antenna, however, in the manner described, and as found by test, there are only two and a half oscillations before all the energy has been transferred to the antenna. The coupling was shown by test to

be about twenty per cent., and this means, as I have pointed out, that two and one-half oscillations are all that are required for the transfer of the energy.

The question refers to "Defendant's Exhibit J-6", referred to by Mr. Loftin beginning at P. R., page 1068. Mr. Loftin prefaces his reference to this address by Mr. Marconi with the statement that "there can be no resonance without a coupling condition that will permit of but one [fol. 1511] frequency to which the circuits can be resonated". He then continues with a reference to the Marconi lecture and states that his views are confirmed by a quotation which he makes from that address. The statement made by Mr. Loftin strikes me as nonsense, but whatever he meant by it, it is not warranted by the patent, nor by Mr. Marconi's address, and certainly not by the statement which he quotes from the latter. This statement is as follows:

"These two circuits are tuned so as to have approximately the same natural period of electrical oscillation. They then—like tuning forks—have been adjusted in syntony. It is well known that when using an ordinary spark discharge in the primary circuit, unless weak coupling is employed, the oscillations set up in one circuit create oscillations of two frequencies in both circuits. This has the disadvantage that the radiated energy becomes divided between two waves of different length, and if the receiver is tuned to only one of these wave lengths, it will utilize or absorb only part of the energy reaching the receiver—the energy of the other wave being lost."

This quotation in no way confirms Mr. Loftin's statement above quoted, nor does it in any way affect the meaning of the patent in suit nor limit it to Mr. Loftin's special definition of "cumulative resonance". Mr. Loftin's conclusions, found on P. R., page 1068, following the quotation which he makes, are both erroneous and unwarranted. In the quoted passage Marconi was briefly recounting the characteristics of coupled tuned circuits, electrically speaking, preparatory to discussing improvements in spark-gaps, and in the very next sentence he points out that he had developed a spark-gap which was capable of opening the circuit of the persistent oscillator and preventing any retransfer of energy, even with the enormous powers required for his long distance or trans-oceanic transmitters. He also

refers to the Wien spark-gap or so-called quenched spark gap, but this very gap operates with couplings within the range of the examples given in the patent in suit. Mr. Marconi did not state that the circuits were not in resonance if the spark-gap did not prevent the re-transfer of energy, as Mr. Loftin assumes. On the contrary, he stated that they were in resonance. His statement merely says that if the spark is not extinguished when all the energy has been transferred to the antenna, the most desirable conditions for reception are not attained. On what basis of reasoning Mr. Loftin can argue that this statement constitutes a limitation of the meaning of the patent in suit, I am unable to see. It has nothing whatever to do with the meaning of the patent. Clearly, if the circuits are in resonance, and of the character described in the patent, the arrangement is within the patent, whether the spark gap behaves in the best possible manner or not. There is no spark-gap which will not operate badly if over-loaded, so far as I know, and neither the spark-balls of the patent nor any of the improvements thereon, will operate in the most desirable manner if improperly loaded. It has been found in practice that in general, the better the cooling, the less loose the limiting coupling for effective operation can be, but substantially all of the coupiings used in practice, so far as I know, have been substantially those indicated by the illustrative examples in the Marconi patent in suit. The patent is re- [fol. 1512] markable in that it discloses substantially the arrangement which years of subsequent practice have shown to be best suited to the purpose. The only improvements which have been made in this transmitter, aside from mere details of convenience, have been in spark-gaps capable of opening the local circuit at the end of the energy-transfer period with increasingly greater quantities of energy. In other words, by improvements in spark-gaps, larger and larger transmitters were made possible.

Mr. Vaill: Counsel for claimant offers in evidence the drawing produced by the witness in connection with his last answer, and requests that the same be marked "Claimant's Exhibit No. 319".

(The Notary marks said exhibit as requested.)

Q. 3. Will you please consider your answer to Q. 7 of your *prima facie* deposition, as found on page 147 of the

Printed Record, regarding the number of oscillations represented in the table on page 4 of the Marconi patent No. 763,772, and state in what respect, if any, you may desire to explain your testimony, in view of Mr. Loftin's criticism thereof, as found in his answer to Q. 22 (P. R., pp. 1054-5).

A. The next to the last paragraph on P. R., p. 147, is certainly not correct. It states that "the circuit if oscillating by itself, would have three complete oscillations—if the current continues to oscillate until it fell 1 per cent. of its original value." It would not require three oscillations to cause a fall of one per cent. There is clearly a typographical error in this paragraph and possibly, also an error of calculation.

The words "until it fell 1 per cent of its original value" should have read "until it fell to ten per cent. of its original value". I do not make this statement as a matter of recollection, because I do not remember, after the long interval which has elapsed, just what I did say, but I make it because I do know that my custom at that time and since, in dealing with spark-gap circuits, was, and has been to consider ten per cent. as the limit to which the oscillations might be considered as falling in a spark-gap circuit. Understanding this correction to be made, the statement that three complete oscillations would be required, is an incorrect statement. The word "three" should be changed to "twelve" The clause would then read as follows:

"I find that assuming the resistance of the spark-gap to be 1 ohm, the circuit if oscillating by itself, would have twelve complete oscillations—if the current continued to oscillate until it fell to ten per cent. of its original value."

As I pointed out in my last answer, and as is admitted by Mr. Loftin, this is not the number of oscillations which would occur when the circuit is coupled to the antenna. When so coupled, the number of oscillations will be less, because the circuit is then loaded by its association with the radiating antenna.

As noted in my last answer, it will be five oscillations under the conditions of construction specified for tune 1 in the Marconi patent in suit.

*   *   *   *   *   *   *

[fol. 1513] I note that Mr. Loftin criticizes the value of the spark-gap resistance which I used in this calculation.

I disagree with his criticism and think that a value of one ohm for the resistance of the entire circuit was low, rather than high. I took my value from the curves published by J. A. Fleming, found, for example, on page 185 of the 1906 Edition, using the value for the spark-gap length given in the table on page 4 of the patent in suit.

I note that Mr. Loftin refers to the Zenneck book translated by Selig, entitled "Wireless Telegraphy," and particularly to Figure 21 on page 19 thereof. Mr. Loftin probably overlooked the fact that this set of curves is only given by Zenneck to show the unreliability of the method of measurement used, hence, it can hardly be entitled to great weight.

I note that the formula given by Prof. Morecroft for the number of oscillations required to transfer the entire energy from the persistent oscillator to the good radiator does not depend upon spark-gap resistance. This number, which is five complete oscillations for tune 1 under consideration is, therefore, not subject to any uncertainty, such as Mr. Loftin thinks exists in the choice of spark-gap resistance, when the circuit is not associated with the antenna.

Q. 4. Will you please consider "Loftin Sketch K" (P. R., p. 1057), and his testimony relative thereto in answer to Q. 23 of his deposition (P. R., p. 1056), particularly his alleged correction of your "Sketch No. 2" which is included in Mr. Loftin's "Sketch K"; and also Mr. Loftin's answer to Q. 24 of his deposition (P. R., pp. 1058-60), in which he further criticizes your testimony as relates to your "Sketch No. 2," and particularly as concerns the question of coupling referred to in your *prima facie* deposition (P. R., pp. 152-3), and state whether or not Mr. Loftin's treatment of your sketch and your testimony relative thereto has changed your opinion regarding the same, giving your reasons therefore?

A. It has not. As to most of the statement referred to, the fact is simply that Mr. Loftin is wrong, while as to the second matter referred to in the question, his answer is based on a misquotation.

In considering his "Sketch K," the upper portion of which is my "Sketch No. 2" and the lower one of which is Mr. Loftin's supposed correction thereof, Mr. Loftin makes a number of errors. For example, he seems to assume that such sketches are quantitative and in addition to this error he has failed to note that in my sketch I illus-

trated the charge as determined by the voltage across the condenser, whereas he illustrates the current flow.

Mr. Loftin's first criticism is that my "Sketch No. 2" shows "far too few" oscillations to represent the interpretation which he has chosen to put on the closed or persistent oscillator circuit of the Marconi patent in suit. What Mr. Loftin ignores, and apparently does not know, is the fact that what will happen in the circuits that Mr. [fol. 1514] Marconi put together is determined by the laws of nature, and not by Mr. Loftin's opinion as to the meaning of the language of the patent. The language of the patent, in my opinion, is not open to any such interpretation as Mr. Loftin puts upon it, but is entirely in harmony with the laws of nature which apply.

Having tested the apparatus disclosed in the patent, I know that the laws of nature determine definitely that in those illustrative examples the number of oscillations required to transfer the energy to the antenna, as shown in my "Sketch No. 2," varies from two and a half to ten. My "Sketch No. 2"—which was entirely illustrative, and not intended to represent a particular case, shows four oscillations in the primary circuit, which is certainly well within the range of the illustrations given by Marconi. Mr. Loftin's amendment to the sketch shows about fourteen oscillations in the primary circuit, which is outside the range of the illustrations contained in the patent.

In my opinion, this matter has nothing to do with the presence or absence of the Marconi invention. An arrangement giving either result would be within the patent. An arrangement would also be within the patent which gave fewer primary oscillations, and it would also be within the patent if it gave more oscillations in the primary circuit. The whole matter is immaterial. It is of no consequence one way or the other.

In my opinion, it is also immaterial whether or not the spark-gap cuts off when the energy has been transferred, or whether it fails to cut off and permits some re-transfer. The latter is certainly not ordinarily desirable, although it may be in special cases. I am unable to see that the precise performance of the spark-gap, in other words, has anything whatever to do with the presence or absence of the Marconi invention.

The figures which I have given, based on Prof. Morecroft's equation, represent the maximum number of oscil-

lations required to transfer the energy. If the spark-gap works effectively, the number actually occurring may be less; in this case not all of the energy will be transferred. Mr. Loftin makes the mistake of thinking that resonance between the primary and secondary circuits requires slow transfer. Exactly the reverse is the case. It permits enormously rapid transfer; a rapidity which can be obtained in no other way. Mr. Loftin has apparently confused the action of a resonant receiving system gradually building up energy received from a far-distant transmitter. But resonance has no such limitation; it is the means by which the quickest possible transfer can be obtained.

Mr. Loftin labors under the erroneous impression that in the Marconi arrangement the primary persistent oscillator, or non-radiating circuit, must be the means which determines the duration of oscillation emitted by the antenna. He is entirely mistaken; the primary circuit has nothing whatver to do with the number of oscillations emitted by the antenna; this number is determined by the construction of the antenna and by the magnitude of the coil *a* inserted in it.

Mr. Loftin's next mistake is the assumption that "Sketch No. 2" was intended to be quantitative. He is mistaken. I used the conventional form of illustration which, so far as I know, is never quantitative.

Mr. Loftin says that the oscillations marked (*b*) are much too great in height and that such a result could not be obtained, even with one hundred per cent. coupling. He [fol. 1515] is mistaken. My sketch illustrates oscillating charges as measured by the voltage. If the sketch had intended to be quantitative it should illustrate antenna voltage, and be very much larger than I have shown it and not smaller, as Mr. Loftin assumed. This is easy to see from the following fact: if the same charge were imparted to the antenna by some imaginary extraneous means as is imparted to the condenser of the primary circuit, the voltages produced would be in the ratio of the capacity. The capacity of the antenna might be say .0002; the capacity given for the primary circuit is approximately .007. The voltage on the antenna would then be thirty-five times that on the primary circuit and to illustrate such a condition my curve (*b*) should be thirty-five times the height of my curve (a). This condition, of course, would not exist for two reasons, first, because all the time that the energy is

being transferred from Marconi's closed circuit to his antenna circuit; the antenna is radiating energy and, second, because some of the original charge put into the primary circuit is lost in the spark-gap. To represent the facts quantitatively, however, it is clear that my sketch (*b*), which Mr. Loftin criticizes, should be much higher; that is, should show much higher oscillations than it does and not much less, as Mr. Loftin asserts.

Instead of illustrating a coupling of the order of one hundred per cent., as Mr. Loftin alleges, my sketch illustrates a coupling of the order of eight or ten per cent. To assert the contrary, as Mr. Loftin does, displays either sheer ignorance or a disregard of the facts. The coupling can be directly obtained by taking the reciprocal of twice the number of complete oscillations in which the energy falls to zero in the primary circuit.

Mr. Loftin next accuses me of suppressing the alleged fact that with the very tight coupling which he erroneously imagines will exist, two oscillations would exist. He is wrong again. Mr. Loftin seems to have the erroneous impression that the current in a circuit can be going in two directions at once. Mr. Weagant has clearly pointed out Mr. Loftin's error in this regard, as found in Loftin Sketches G and H, "Defendant's Exhibits O-2 and P-2."

With a coupling of eight to ten per cent. there are not necessarily two frequencies, and my "Sketch No. 2" should not necessarily have shown any waxing or waning of the curves to illustrate that condition. So far from suppressing anything, I fully and clearly pointed out, as, for example, on P. R., p. 152 of my former deposition, that for best results the coupling must not be tighter than will permit the spark-gap to open the primary circuit when the energy has been transferred to the antenna.

From the point of view of the Marconi invention, however, I fail to see that it is of any consequence whether there is, or is not, a coupling somewhat tighter than that required to give best results or, indeed, somewhat looser. The Marconi patent is not a patent for a particular degree of coupling, it is a patent disclosing the association of a non-radiating circuit with a radiating circuit, and the transfer of energy from the first to the second by the principle of resonance. It is capable under the condition illustrated in my "Sketch No. 2" of giving the result there illustrated and I fail to see that it is of the slightest consequence

whether someone uses it with a spark-gap which will give a less efficient result.

[fol. 1516] Mr. Loftin further criticizes the sketch because he says it is typical of that usually found "illustrative of the action of the quenched-gap type of transmitter." I fail to see that this is a basis of criticism. If I understand what Mr. Loftin refers to, it is simply one well-known form of the Marconi invention. Substituting one particular construction of spark-gap for another certainly does not take the construction outside of the description of the Marconi patent, particularly when it does not alter the mode of operation, but simply realizes the result with greater reliability. Indeed, it is going too far to say that it does realize the result with greater reliability than does the rotary gap, for instance. According to my understanding, improving the spark-gap so that it will carry greater energy or attain the desired result with less attention, is only the normal and expected result of extensive use and experience with the invention.

Regarding the second reference in the question, viz., that to Q. 24 of Mr. Loftin's deposition, the fact is simply that the answer misquotes what I said, and it may be that he was misled. He does not put the statement in quotation marks, but represents me as saying "that the coupling cannot be too close if the results indicated in the Sketch No. 2 are to be attained." What I said was "the association, or as it is called at the present time the coupling of the two circuits, must not be too close if the best results and the precise results indicated in Waterman Sketch No. 2 are to be obtained."

Mr. Loftin proceeds to state that I have not shown what he calls "loose coupling" but rather what he calls "tight coupling."

I have already pointed out that these terms have no absolute meaning, since all couplings used in radio transmitters of this type are loose. As a matter of fact, Mr. Loftin himself has defined a loose coupling on P. R., p. 1024, as folllows:

"It is common to say that if the two circuits are so loosely interlinked as to create no apparent two frequencies from a practical point of view, that the circuits are 'loosely coupled'."

My "Sketch No. 2" shows only one frequency, because there are no undulations of strength of the waves and, therefore, it shows loose coupling according to Mr. Loftin's own definition. Furthermore, the coupling which is deducible directly from it seems to be eight to ten per cent., which is "loose" according to any definition that I find in Mr. Loftin's testimony. A quenched spark-gap transmitter, such as he frequently refers to, also gives but one frequency and hence, by his own definition, is also "loosely coupled." Mr. Loftin seems to think that if any transmitter does show undulations in the radiated wave-strength, then it is not within the Marconi invention. Any type of gap will give this result if the circuits are too closely coupled, for the particular construction of the gap, whether the gap be what he calls a quenched-gap, or not. My own opinion would be that such a transmitter, without regard to the gap construction, simply did not illustrate the Marconi invention in the best form; whereas, my "Sketch No. 2" does illustrate it in its best form, without regard to the type of gap. What may, or may not, be within the patent in a legal sense, I, of course, do not undertake to say, as I understand that is a matter for the Court alone.

I note that on page 1059 of the Printed Record, Md. Loftin says:

[fol. 1517] "As a matter of fact, the cessation of oscillations in the primary circuit is not determined by the association of the two circuits, but upon spark-gap conditions. * * *"

This is misleading. The maximum number of oscillations in which the energy will be transferred from the primary to the secondary circuit is determined solely "by the association of the two circuits," if they are in tune with one another, as described by the Marconi patent. If they are not in tune, a greater number will be required. All that the spark-gap can do, so long as there is no re-transfer of energy, is to make the number of oscillations less than that determined by the association of the circuits. For instance, in my "Sketch No. 2," as described in my deposition, the spark-gap is illustrated as requiring a voltage indicated by the dotted line *x* at the time when the oscillations have fallen as indicated in (*a*). The spark-gap, therefore, is shown as having opened the circuit just before all the energy is transferred.

Mr. Loftin is wrong when he states that no open or plain gap will give the result indicated in "Sketch No. 2." It will, as I know from experience. It is also true, as I have stated, that it will require much more care in the construction and operation of the gap to attain that result than would be the case with a quenched gap, so-called.

Further on in his answer to Q. 24, Mr. Loftin states that: "The quick transfer of energy and early cut off illustrated by Mr. Waterman takes away from the primary circuit all ability to create oscillatins of the desired frequency." This is an incorect statement from several points of view. It is based, like many other statements which he makes, upon an assumption on his part, that the words "a means which automatically causes oscillations of the desired frequency," refers to the primary circuit as a whole. I find no warrant in the patent for any such view. On the contrary, the condenser *c* in the primary circuit is said to discharge through this means and this precludes any such meaning. Furthermore, the specification definitely defines what it means at page 1, line 43, where it says "a producer of Hertzian oscillations or electric waves shown in the form of a spark-producer."

The local or primary circuit has nothing whatever to do, except to store energy until the spark-gap goes into operation. The instant this occurs oscillations are produced, and energy is transferred to the antenna circuit. Since the two circuits are in tune of resonance, as described in the patent in suit, the oscillations which the spark-gap sets up will be of the desired frequency, and energy will be rapidly and efficiently transferred to the antenna circuit, at which time the spark-gap will go out of action and the energy accumulated in the antenna will continue to be radiated. An effective gap, that is to say, a spark-gap working in the most desirable fashion, will cut off by or before the time when all the energy has been transferred to the antenna. The antenna begins to radiate as soon as the spark starts and it continues not only till the spark ends, but until long afterwards. Just how long afterwards depends upon the construction of the antenna and the localized inductance which it contains. The spark-gap thus acts to set up or cause oscillations of the desired frequency, and the language of the patent is quite correct. Also, my "Sketch No. 2" is not in conflict with this or any other statement of the patent,

but was intended to be, and is, entirely in harmony therewith.

[fol. 1518] Mr. Vaill: Counsel for claimant offers in evidence a photostat copy of page 185 of Fleming book entitled "The Principles of Electric Wave Telegraphy," 1906 Edition, and requests that the same be marked "Claimant's Exhibit No. 320."

Also a photostat copy of page 247 of a book by J. H. Morecroft, entitled "Principles of Radio Communication," 1921 Edition, and requests that the same be marked "Claimant's Exhibit No. 321."

(The Notary marks said exhibits as requested.)

Q. 5. Please consider the paragraph quoted by Mr. Loftin from printed page 153 of your prior deposition, and found on page 1060 of Mr. Loftin's deposition and state whether or not Mr. Loftin's comments on your testimony has tended to change your views with respect to precise tuning or adjustment of the circuits under the Marconi tuning patent 763,772, giving your reasons for your answer.

A. What Mr. Loftin says in no way alters my opinion, for what he says is substantially wholly erroneous; and, furthermore, it apparently is not consistent with itself. Of course, it would be a mere commonplace to point out that nothing can be precise in a rigid sense. Everyone knows that. Mr. Loftin, in fact, does not seem to insist upon it, yet the reasons which he thinks afford a basis for his differing opinion would require absolute precision.

Mr. Weagant has correctly stated the facts in this regard. They are, briefly, that a spark-gap circuit tunes broadly; that is to say, it is impossible to tell with any approach to absolute precision when it is in mathematically perfect resonance. A considerable variation, as, for example, one or two per cent either way, makes no detectable difference in the result, and ordinarily cannot be detected. Mr. Loftin is laboring under an erroneous impression in his reference to forced and natural oscillations. As Mr. Weagant has pointed out, Mr. Loftin's testimony, and some of his diagrams, as for example, Figs. 2 and 2*a*, of Sketch G, and Fig. 4*a* of Sketch H, are based upon a curious notion wholly erroneous, that current can flow simultaneously in two directions in the same circuit. No such operation is possible, and all of the numerous statements which Mr. Loftin

has based on that concept, are incorrect. As I know, from my own use of the apparatus of the Marconi patent and many tests thereon, it is exactly correct to say as I did, that "No mathematically exact or precise equality between the circuits is realizable in practice" and also it is correct to say as I also did, that "it is important to the efficient transfer of energy that this equality should be approximately or substantially attained." I agree with Mr. Loftin that the apparatus illustrated by Mr. Marconi is "capable of the desired precision of adjustment," but no absolutely precise adjustment is desired. It is neither necessary nor attainable, nor would it add to the practical utility.

Q. 6. Please refer to Mr. Loftin's answer to Q. 26 of his deposition (P. R., p. 1062), wherein he criticizes your "Sketch No. 3," opposite page 155 of the printed record, on which page you refer to a sketch in your prior deposition, and state whether, in your opinion Mr. Loftin's criticisms are justified by the specification or drawings of the Marconi patent in suit, No. 763,772.

A. Mr. Loftin's criticisms are not justified. I was referring to, and in my sketch "Plaintiff's Exhibit 99" (opposite [fol. 1519] P. R., p. 157), I showed the connections described in the Marconi patent by reference to Fig. 2, and the particular embodiments thereof in the table on page 4 of Marconi patent in suit. As I stated, and as is, in fact, stated on the sketch itself, my "Sketch No. 3" is a mere simplification of Fig. 2 of the patent. Functionally the apparatus of the patent is not altered in any way.

All of the testimony which I gave was based upon the drawings of the patent, of which "Sketch No. 3" was a mere simplified illustration. Mr. Loftin states that I changed the position of the condenser j^3, which I also stated. He says there is no justification in the Marconi figure or specification for this change. If he means that there is no instruction in the patent to make the change he is, of course, correct; but if he means that it makes any practical difference, he is wrong. He states that in the position shown the condenser j^3 "has its share in affecting the product of inductance and capacity of the circuit," whereas he says that my "Sketch No. 3" is effectively removed from the circuit. To show how trivial a matter this would be, I refer to tune 2 on page 4 of the patent, where the condenser h' is given as .00004. The value for the condenser j^3 is not

given, because it is quite immaterial. .004 is commonly used and sometimes .002. Taking the latter figure as being most unfavorable to my sketch, the tuning effect of condenser j^3 in tune 2, is to change the effective capacity of h' to .0000392, or about two per cent. Since the condenser h', however, is adjustable, this amounts to a mere trifling change of adjustment.

The operation in any case is precisely the same, and I have no desire to alter the figure of the patent in any respect. I merely introduced my "Sketch No. 3" because it is, in fact, operatively identical, and I thought it might be easier to understand. The other comments which Mr. Loftin makes have no foundation in fact, and merely indicate that he has not used the circuits and is not familiar with their operation.

I have used the arrangement exactly as shown in Fig. 2 of the patent in suit and as shown in my diagram, "Sketch No. 3," and I know that the operation is the same.

Q. 7. Will you please state whether or not you agree with the statement of alleged facts, opinions or views expressed by Mr. Loftin in his answers to questions 29, 30, 31, 32 and 33, which are found beginning at page 1096 of the printed record and continuing through page 1146 thereof, in respect to defendant's quenched-gap transmitters, particularly criticisms made by Mr. Loftin of parts of your previous deposition relating to such transmitters, giving your reasons for your answer?

A. I do not agree with Mr. Loftin's testimony referred to; I am compelled to infer from the testimony which he gave that he has never used the transmitters such as he was discussing, and knows little about them. At any rate his statements are so far from the facts as to leave room for no other conclusion.

These answers deal with what he calls, and what has been called in the trade, the quenched spark-transmitter. Mr. Loftin states that it is "beyond all doubt" that these transmitters are of the type described in United States Patent No. 1,216,615, to Seibt. He proceeds to discuss these transmitters on that basis.

The Seibt patent refered to, is "Defendant's Exhibit C 3." I am familiar with the patent. This patent shows [fol. 1520] a transmitter comprising an open antenna circuit constituting a good radiator and a local circuit constituting a good conserver or pesistent oscillator, which cannot

radiate. These two circuits are tuned, as described in the Marconi patent, to have the same natural period, and they are associated so that energy can be supplied to the persistent oscillator and transferred by it to the antenna circuit, all as disclosed in the Marconi patent.

The Seibt antenna or good radiator circuit comprises an elevated conductor connected through an inductance coil and oscillation-transformer to earth (a measuring instrument is also shown which, of course, does not affect the operation in any way). The local or persistent oscillator circuit comprises a reservoir condenser C, the primary S′ of an oscillation transformer and a spark-gap F.

As Mr. Weagant has discussed this patent and its File Wrapper, and has pointed out the several disclosures of Seibt, to the effect that these two circuits are in resonance with one another, with which testimony I agree, I will not repeat.

It is clear, then, that the Seibt patent, which Mr. Loftin says discloses the quenched spark-gap transmitter employed by the Navy, shows exactly such an arrangement as is disclosed in the Marconi patent in suit. The sole difference is, that whereas the Marconi patent refers to the spark-gap as "spherical terminals or other contacts of a spark-producer or other electric-wave or oscillation producer" (p. 1, line 83), Seibt describes a particular type of spark-gap of the sort set forth in the prior Marconi patent, re-issue 11,913, which is characterized by a succession of gaps in series. The Seibt arrangement is particularly suitable for powerful transmitters, and Marconi says, at page 2, line 7 of his prior patent:

"If a very powerful source of electricity giving a very long spark be employed, it is preferable to divide the spark-gap between the central balls of the oscillator into several smaller gaps in series."

This is what Seibt has done and he shows, in Figs. 10, 11, 13 and 14, arrangements of this character. It is constructions of the type shown in Figs. 13 and 14 that have been commonly employed. In short, the sole difference between what Seibt shows and what is disclosed in the Marconi patent in suit is that Seibt has chosen, and improved upon, the particular type of spark-gap referred to in the above quotation from the early Marconi patent. This,

of course, is in compliance with the instruction of the Marconi patent No. 763,772, that any suitable gap may be employed.

The operation of the two arrangements is the same. Both are subject to the same difficulties and have the same advantages. If in either one a certain coupling is used, a certain number of oscillations will be required to transfer the energy from the closed circuit to the open circuit. Thus, if the coupling is ten per cent., five oscillations will be required in either case; if the coupling is twenty per cent., two and a half oscillations will be required in either case; there is no difference whatever between the structure disclosed by Marconi and that of the Seibt patent, in this regard. If, in either case, the association of the two circuits is too close, in view of the amount of energy to be [fol. 1521] transferred and hence the amount of heat to be dissipated in the gap, reaction will occur between the two circuits and the waves transmitted will pulsate and exhibit two frequencies; sometimes three frequencies. There is no difference between them in this regard. The difference between them is that the Seibt form of Marconi's multiple gap cools admirably and will handle a relatively large amount of energy very reliably and is therefore better than a succession of spheres. It is easier to get a gap of this Seibt type into operation in the most desirable manner, as illustrated in my "Sketch No. 2", than it is in a gap in which the sparking surfaces are open freely to the atmosphere and the gap remains in good operating condition much longer.

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The apparatus constituting the transmitter of the Wireless Specialty Apparatus type which Mr. Loftin was talking about in his answer to Q. 29, and which he says is beyond all doubt that of the Seibt patent, is thus seen to be identically the apparatus of the Marconi patent in suit, it has the same circuits, they are tuned together in the same way, and the spark-gap is such a "spark-producer or other electric-wave or oscillation producer" as is referred to in the Marconi patent.

The two circuits are in resonance in the sense defined in the Marconi patent, viz., their natural frequencies are made alike by making the product of capacity by self induction in the two circuits the same. Mr. Loftin, how

ever, says that they are not in resonance in his use of the term. Mr. Loftin has, throughout his deposition, made use of a special definition of resonance. The fact that the two circuits are "so independently adjusted as to make the product of the self-induction multiplied by the capacity the same in each case", which is all that is required by the Marconi patent (page 2, line 123), is not enough. According to Mr. Loftin, if the circuits are so related that one reacts on the other, then resonance does not exist, even if the circuits have been independently adjusted, as required by the patent in suit. He ignores the fact that if the circuits did not react on one another, they could not act on one another, and the apparatus would be practically inoperative.

Mr. Loftin does not allege that the apparatus of the Marconi patent is inoperative or useless; on the contrary, he seems to recognize its great utility. He merely seems to seek to limit it by imposing definitions of words which the patent does not contain, and by disregarding the explicit instructions which the patent does contain. The Marconi patent specifies that "the electrical time periods" of the non-radiating persistent oscillator circuit and of the radiat- [fol. 1522] ing antenna circuit shall be made the same by adjusting the products of the inductances and capacities of the two circuits to be the same. He does not appear to deny that Seibt has the same adjustment, but he says that "the adjusting of the time period of the two circuits is for a purpose having nothing whatever to do with a resonant transfer of energy between circuits as was necessary to the success of the Marconi system". I have not found that he gave any proof of this statement and it is wholly incorrect. It is only necessary to read the Seibt patent to see that it is incorrect. Figs. 2 to 5, which show different degrees of promptness of response of the spark-gap in opening the primary circuit, which are the basis of the disclosure of the Seibt patent, are all based upon the assumption that the same resonant transfer of energy exists as disclosed in the Marconi patent, and Fig. 6 is said to be "the resonance curve of the coupled system where the spark snaps off rapidly" (Seibt patent, p. 3, line 3). It is exactly the "resonance curve" of the arrangement of the Marconi patent where the spark snaps off as it should. Seibt merely discloses a reliable form of spark-gap for obtaining this desired action with large powers.

Since Mr. Loftin has had so much to say about coupling, that is to say, the closeness or looseness of association of the closed and open circuits of the transmitter for energy-transfer purposes, it is interesting to note that the coupling indicated by the figures of the Seibt patent is about ten per cent., while the range indicated by the illustrative examples of the Marconi patent is from five to twenty per cent. In my experience with quenched spark-gaps I have found that the use of such a gap in the Marconi transmitter as shown by Seibt, enables effective operation as shown in my "Sketch No. 2" to be realized with couplings ranging from ten to twenty per cent.

Another allegation which Mr. Loftin makes is that the Seibt arrangement "is based on the system disclosed by Lodge in his patent 609,154"; it is not. This statement ignores the actual structure of Lodge and is based upon the restrictive definitions which Mr. Loftin has devised, such as the limitation to what he calls "cumulative resonance" and to the entirely impossible restriction which he undertakes to place on the term "persistent oscillator", by which he thinks is meant a circuit which performs in contravention to the laws of nature and keeps on oscillating for a long time, whether it is coupled to the antenna or not. As I have pointed out, the number of oscillations before a non-radiating circuit has transferred all its energy to the antenna is determined by the coupling and is from two and a half to ten in the illustrations given by Marconi.

Lodge made the mistake in his Fig. 4, to which Mr. Loftin refers, of proposing an arrangement having two radiating circuits. Looking at Fig. 4 of the Lodge patent No. 609,154, it will be seen that there is absolutely no more reason for regarding the circuit h, h^1, h^2, h^3, h^4, h', as the radiating circuit, than there is for regarding the circuit h, h^6, h^8, k, h^8, h^7, h' or of the circuit h, h^6, h^8, j, h^{10}, h^{11}, j, h^8, h^7, h'. As a matter of fact, these are radiating circuits.

Lodge made the mistake of associating a charging circuit which was a radiating circuit, with other radiating circuits, and thus produced a useless arrangement which has never been used.

The Seibt transmitter as illustrated either in the patent or in the Wireless Specialty Apparatus Company trans-[fol. 1523] mitter, has the Marconi arrangement, wherein there is one circuit which is a persistent oscillator, and cannot radiate appreciably, associated with a second circuit

which is stretched widely open, with one end in the air, and the other connected to earth and hence, is a radiating circuit.

Quenched spark transmitters of this Seibt type therefore, in no way resemble the Lodge arrangement, but have exactly the Marconi arrangement, viz., a non-radiating circuit containing a spark-gap associated with a radiating circuit, which has no spark-gap, the two having the same "electrical time periods" as required by the Marconi patent. There is no possible basis in fact for Mr. Loftin's contention that the Seibt arrangement is based upon that of Lodge, unless he wants to go so far as to say that the Marconi patent in suit is also based upon Lodge's Fig. 4, which he does not assert. Lodge has a plurality of radiating circuits; the idea which is fundamental to Marconi is that the spark-gap shall be in a non-radiating circuit, which he calls a persistent oscillator, because of its non-radiating capacity. There is no such circuit associated with a radiating circuit in the Lodge patent, nor is there the idea of a resonant transfer of energy from the non-radiating circuit to the radiating circuit, by making the electrical time periods the same.

The sole basis for Mr. Loftin's contention that there is not a resonant transfer of energy in this Sebt arrangement having the so-called quenched-gap, is that the primary or local circuit would, in the best operation, cease to oscillate when all of its energy had been transferred and that this may take place in a number of oscillations, relatively small as compared to the number of oscillations from the antenna circuit. That this idea is without foundation is shown by the fact that the number of oscillations in which the energy is transferred to the antenna, as shown by Seibt, falls definitely in the range definitely disclosed by Marconi, as pointed out in my preceding answers. It is because of the resonant transfer that the energy can be handed over in so few oscillations. What happens is as follows: The spark-gap, discharging, sets up oscillations of the desired frequency in the non-radiating circuit. Associated with this circuit is the open circuit which can, theoretically, be set into oscillation at any frequency, but because its electrical time period has been made the same as that of the spark-gap circuit, it offers the lowest possible resistance to being set into oscillation at this frequency to

which it is tuned. It therefore greedily absorbs energy from the local circuit, which thus acts as a reservoir into which the energy was preliminarily introduced. While it is thus absorbing energy, it also is radiating, and it is thus able to radiate much more energy for a given permissible electrical pressure developed than it could if the energy had to be imparted to it directly.

It is this feature of the rapid resonant transfer of energy from the non-radiating to the radiating circuit which accounts for the enormous utility of the Marconi invention, and it is fundamentally quite immaterial whether the spark-gap employed takes one form or another.

In his answer to Q. 29, referred to in the present interrogatory, Mr. Loftin refers to his sketch G, wherein he says he has shown in red and green the two frequencies which exist when the spark-gap does not function to open the local circuit after its energy has been all transferred. I have pointed out that the Figs. 2 and 2*a* on this sketch (the ones he is here talking about) represent a totally impossible [fol. 1524] state of affairs and show nothing but Mr. Loftin's ignorance of electrical science; but, overlooking this, the use which he makes of his figures on P. R., p. 1098, appears to me to assert that such operation is not possible with the Seibt apparatus. If Mr. Loftin entertains this view, he is mistaken. It is just as easy to cause the Seibt apparatus to operate in the manner which Mr. Loftin apparently intended to illustrate, as it is when some other form of spark-gap is used. All that is necessary in either case is to omit to sufficiently loosen the coupling. Whatever form the spark-gap may take, it will fail to open the circuit at the moment when the energy has been all transferred to the antenna, unless the coupling is made loose enough. It is, of course, true that a gap having ample provision for cooling, does not require the coupling to be loosened as much as one whose cooling is less adequate. This, however, is a mere matter of degree, and has no bearing on the presence or absence of the combination and mode of operation described in the Marconi patent.

Mr. Loftin also seems to suggest that the Seibt patent implies that with other forms of spark-gap the operation will always be that which Mr. Loftin sought to illustrate by his sketch G. I do not so understand the Seibt patent. In fact, I understand it to state quite the opposite.

(A to Q. 7 continued): On page 2 of the Seibt patent, after explaining by reference to Fig. 2, the possible effect of reaction between two circuits, Seibt says at line 32:

"In the foregoing consideration it is assumed that the spark gap acts as an ordinary conductor and retains its conductivity until the energy at the discharge has been entirely dissipated."

"Dissipated" in this quotation, does not mean "transferred"; it means converted into heat, because Seibt is talking about Fig. 1, wherein there is no radiation. The spark-gap, however, does not act as an ordinary conductor, but, as Mr. Weagant has explained, its resistance rapidly increases as the energy in the oscillating charge decreases. Hence, it tends to be extinguished when the energy has all been transferred, and will do so if the spark-gap is properly cooled. In his Fig. 7, Seibt shows a simple way of effecting such cooling by using hollow spark electrodes filled with water. Seibt then goes on to say that the condition of retained conductivity is approximated the more as there is more vaporization of the metal of the electrodes, or as the load on the gap is increased by increase of current; also, this condition, he says tends to exist the more as the heat conductivity of the electrodes becomes smaller.

He then states:

"In practice these characteristics and conditions of the spark gap are never fully attained * * *."

This means that it would take a very bad spark-gap condition, or a very insufficiently loosened coupling, to bring about the results shown in his Fig. 2, which I assume is what Mr. Loftin intends to show in his sketch G.

Seibt then continues at line 51 of page 2:

"Consequently, in practice, after a discharge of the condenser through the spark gap, such a condition of high [fol. 1525] resistance is created at the spark gap as to destroy its conductivity and its capacity to act as a conductor for the current before the discharge energy has been entirely dissipated, thereby, in effect, snapping off the further flow of current across the spark gap when the current intensity falls below a certain value."

He then proceeds to point out in further discussion of what happens in practice, that this condition of high-re-

sistance in the gap may occur at any one of the instants shown in Fig. 2, when the energy has all been transferred to the antenna. I understand that his **Fig. 4** shows what happens when the spark-gap resistance becomes high enough to prevent further oscillation as soon as the energy has all been transferred to the antenna. Fig. 4 (which corresponds to curve (*b*) of my "Sketch No. 2") shows what happens in the antenna. Fig. 5 of the Seibt patent shows what happens in the secondary circuit when the opening of the primary circuit does not occur when the energy has first been transferred to the antenna, but does occur when it has been transferred a second time.

This description of the Seibt patent corresponds to my understanding of the fact. No matter what spark-gap is used, if it is not sufficiently cooled for the energy which it is called upon the handle, or if the association with the antenna has not been made sufficiently loose, the spark-gap resistance may not prevent transfer of energy back to the primary circuit immediately after all the energy has been passed to the antenna. This desired termination of the action of the spark-gap may, therefore, occur the first time that the energy is transferred, or the second time, or the third time. The more re-transfers of energy there are, the less desirable for most purposes, is the operative efficiency. It has, therefore, been the endeavor from the outset, that is, from the first introduction of the apparatus of the Marconi patent, to provide better and better cooling for the spark-gap, and to pay more careful attention to its operating condition, where the highest usefulness of the result has been desired.

I have operated the apparatus of the Marconi patent extensively and have used spark-gaps of a variety of kinds, and I have always found that resonant transfer of energy was essential, and that with proper care all forms of spark-gap give the same result. The operative range of coupling is that covered by the examples given in the Marconi patent. All of them give reaction effects, unless the coupling is loosened down to the neighborhood of twenty per cent. Sometimes it must be loosened still further. This is a matter of degree, depending upon the cleanness of the spark-terminals, their heat dissipation and the load imposed on them. I have adjusted quenched spark transmitters hundreds of times, and have always found that they behaved in the same way as Marconi transmitters having other

forms of spark-gap; a resonant transfer of energy is essential, and resonance has, in a transmitter with a quenched-gap, exactly the same functions and importance as with other forms of gap.

On printed record page 1099 of his deposition, Mr. Loftin quotes from the Seibt patent, page 2, lines 74 to 87, and he seems to intimate that Seibt is there talking about the operation of an apparatus containing the quenched-gap. Examination of the patent will show that he is not; he is talking about the characteristics exhibited by prior circuits, and [fol. 1526] he says that they may act in any of the ways shown in his Figs. 2 to 5, inclusive. The passage which Mr. Loftin quotes, therefore, directly contradicts Mr. Loftin's whole theory of operation of what he calls the "Marconi type apparatus."

It is not until page 3, line 6, that Seibt says:

"From the foregoing considerations it will be seen that in practical operation it is desirable that the spark snaps off, or is quenched, or, in another expression of the idea, that the primary circuit is opened, at the instant when the current amplitude first reaches zero value in the primary circuit, or at the spark gap, and that the duration of the spark be as short as possible, that is, that the coupling between the circuits shall be sufficiently strong or close and adjusted to secure a rapid decrease of the oscillations in the primary circuit with corresponding reduction of the energy in the primary circuit to substantially zero value as indicated by the diagrams, in which case the maximum of efficiency is attained in the transformation of energy from the primary to the secondary circuit."

All of the specification up to this point is devoted to showing the various degrees of operative effectiveness exhibited by prior structures typified by Fig. 1. In the passage just quoted, Seibt points out that it is preferable that the spark should be extinguished when the energy in the primary circuit first falls to zero. The specification then proceeds to explain the construction by which Seibt expected to obtain this result with greater certainty.

Seibt made an improvement if he is, in fact, the original inventor of Figs. 13 and 14 of his patent, in that he accomplished the Marconi result with a degree of reliability which previously had required a rotating gap, and he did it without moving parts. This form of gap came largely into

use, although it never entirely displaced other forms, and my understanding is that the United States Navy, in many of its contracts, required that where the Seibt form of gap was furnished, a rotary form of gap should also be supplied, to be interchangeable with the quenched-gap, so that the rotary gap could be employed when the quenched-gap became dirty, or otherwise out of order, and failed, therefore, to operate as shown in my "Sketch No. 2," and began to operate in the manner which I assume Mr. Loftin intended to illustrate in Figs. 2 and 2ª of his Sketch G. In other words, when the operation changed from that of Fig. 4 of the Seibt patent to that of Fig. 2 of the same patent. Mr. Loftin's quotation from the Seibt patent, therefore, quite completely contradicts his own argument when read in its context.

On page 1103 and following of the Printed Record, Mr. Loftin seems to express the opinion that even though the circuits are adjusted as required by the Marconi patent and have the same electrical time periods, the Marconi invention is not employed if this adjustment is made for some other purpose than what he calls the "resonant transfer of energy" in a manner corresponding to his "cumulative resonance" theory.

I disagree with this opinion. The Marconi patent calls for the association in a transmitter, of a radiating and non-radiating circuit, whose electrical time periods are equal; he does not undertake to give all the reasons why they should be so adjusted, but he clearly and definitely states [fol. 1527] what should be done. These instructions of the Marconi patent in suit are faithfully embodied in the quenched spark-gap transmitters employed by the Navy, and it would seem to be entirely immaterial whether the Marconi patent does, or does not, set forth all of these advantages, or reasons for so doing, which Mr. Loftin finds apply to these transmitters.

With respect to Mr. Loftin's answer to Q. 30, in which he deals with the claims of the Marconi patent, my understanding is that these are proper subjects for interpretation only by the Court. I have explained my understanding of the invention and my reasons for stating that Mr. Loftin interprets the terms of the patent in an erroneous manner, not warranted by the patent, but, in fact, inconsistent therewith. It would seem to serve no useful purpose to

follow him into a detailed discussion of the claims, wherein he applies his erroneous definitions to their terms.

In his answer to Q. 31, Mr. Loftin endeavors to explain away the very definite instructions given by the various Government publications and the like, to the effect that the circuits of the Wireless Specialty Apparatus type of transmitter should be tuned to resonance, or equal time periods. The substance of his contention appears to be that although they are manifestly intended to be accurate, they are merely loose and careless statements, which should not be taken seriously.

Having shown that Mr. Loftin is mistaken in most of his ideas as to what the facts are, it would seem that the statements of the Government publications are entitled to full weight. I will therefore not discuss this matter in detail.

In his answer to Q. 32, Mr. Loftin gives many other forms of apparatus. It does not seem to me to be necessary to follow him through this long list of apparatus. In so far as it refers to quenched-gap transmitter apparatus, what I have already said applies, and I believe Mr. Loftin to be as completely mistaken as to each of these as I have shown him to be with respect to the Wireless Specialty Apparatus Company's transmitter, which he says is beyond a doubt that of the Seibt patent.

In so far as this answer deals with receiving apparatus, it seems to me only necessary to point out that the instructions of the Marconi patent No. 763,772, so far as concerns the adjustment of the electrical time periods, is made to explicitly apply to the receiving circuits.

Marconi's receiving apparatus is, in a sense, an inversion or reversal of his transmitting apparatus. Just as an elevated conductor connected through an oscillation transformer to earth is an efficient radiator of energy passed over to it from a non-radiating local circuit, so a similar elevated conductor at the receiving station is an efficient absorber of the energy of electric waves which it intercepts. Associated with this open antenna circuit Marconi shows and describes a tuned local circuit which has little or no absorbing capacity of itself. By the electrical association of the two circuits, the energy which the elevated conductor absorbs from the ether is handed on to the closed circuit, where it is utilized in any suitable form of detector, as explained at page 2, lines 57 to 77 of this Marconi patent.

The instructions of the patent are that the electrical time periods of these two receiving circuits shall be the same as those of the two transmitting circuits.

[fol. 1528] On page 4 of the specification Marconi illustrates by examples, various forms which the receiving apparatus may take. In Fig. 2 of the drawing, he shows the complete receiver incoporating all of the various devices which he describes. In the examples on page 4, he designates what form each element takes, and he shows that the elements of the complete system of Fig. 2 need not all be used at one time, but that they may be used in various combinations, all embodying the fundamental feature of two circuits whose time periods are equal to one another, and to those of the transmitter.

In "Plaintiff's Exhibit No. 99," found opposite P. R., p. 157, I have illustrated, diagrammatically, the arrangements of these several receivers of the table. I do not find that Mr. Loftin anywhere challenges the accuracy of this showing. He merely seems to assert that receivers used by the Navy, which are of the types corresponding to tunes 1, 3, 5 and 6, are "outside of the showing" of the Marconi patent (P. R., p. 1117). It is a little difficult to know what he means by this, since they are certainly not outside of the showing, but happen to be within it. He calls attention to the fact that the Lodge patent, in Fig. 13 thereof, shows no tuning means in connection with the coil *u*, which is connected to the connector *c* and indicator *g*, and he makes the statement that "Fig. 13 is a tunable circuit." This is decidedly misleading. Lodge gives no instructions that the time period of his circuit is to be made the same as that of the antenna, or as that of the transmitter. Hence, his disclosure is not the same as that of the Marconi patent, and Mr. Loftin has no right to make the quoted statement.

If, on the other hand, Mr. Loftin means to say Lodge's detector circuit is adjustable, then he is also wrong, as it is neither shown nor described as adjustable.

My understanding is that the illustrative examples of the Marconi patent all are intended to have time periods between circuits alike, and equal to those of the transmitter. I constructed these circuits and found this to be the fact. I also understand it to be the fact that circuits used by the Government and referred to in Mr. Loftin's answer to Q. 32, were in fact adjusted to have the time periods of the two circuits alike and equal to those of the transmitters.

from which they were designed to receive signals. I therefore understand that they are such circuits and such receivers as are referred to in the Marconi patent, and I find nothing in Mr. Loftin's testimony to change that view.

In Mr. Loftin's answer to Q. 33, he criticises "Plaintiff's Exhibit No. 102" on the ground that it shows two coil-transformers, instead of what are known as auto-transformers having only a single coil.

In making this criticism, Mr. Loftin evidently looked at the lithographic reproduction, and not at the original exhibit. In the original exhibit the red and black lines are in register, as near as it is possible to get them, while in the reproduction they are very much out of register.

Furthermore, as Mr. Loftin should know, the diagram illustrates current paths, the black showing the path of the antenna current, and the red that of the local circuit current. I find on P. R., pp. 196 and 197, that I explicitly stated that "the Foote-Pierson apparatus of specification No. 410 has both the transmitting and receiving oscillation transformer of the direct coupled or auto-transformer type." I went on to say:

"As I have already explained, this change in the form [fol. 1529] of the oscillation transformer does not in any way alter the operative relation of the two circuits nor change the mode of operation of the apparatus."

I note that Mr. Loftin does not appear to challenge the accuracy of this statement; he merely alleges that the drawing "Plaintiff's Exhibit No. 102," is erroneous. If he had looked at the original exhibit, I think he would not have made even this criticism.

Q. 8. Although you may have, in substance, stated your understanding of the facts, in your previous answers, I will ask you to refer to Mr. Loftin's statement beginning at the bottom of page 1139 of the Printed Record, to the effect that defendant's transmitters of the quenched-gap type "are based on the fundamental principle of the Lodge impulse excitation set forth in Lodge's 1898 patent; that is, the principle of quickly delivering the energy from the primary circuit into the antenna circuit non-resonantly and separating in effect the primary circuit from the secondary circuit once the delivery is complete, leaving the energy in the secondary circuit free to oscillate at the

natural period of the secondary circuit, uninfluenced by any maintained connection to the primary circuit." Please state whether or not the above statement accords with your understanding of the operation of defendant's quenched-gap transmitters.

A. I does not. I have already pointed out that the defendant's quenched-gap transmitters are not based upon, and do not embody, what Mr. Loftin has called the Lodge impulse excitation principle. Lodges does not have in his transmitter a local circuit of the non-radiating type; he does not have such a circuit associated with an open radiating circuit. Furthermore, Lodge does not have two circuits of any kind whose electrical time periods are equal to one another, as required by the Marconi patent.

The defendant's quenched-gap types have all of these things, and they do not have Lodge's multiplicity of spark gaps, nor his multiplicity of radiating circuits. They are, in fact, in every respect like the apparatus of the Marconi patent, and they have the Marconi mode of operation; they are not at all like the apparatus of Fig. 4 of the Lodge patent, and they do not have any such mode of operation as Lodge aimed at, but failed to realize.

* * * * * * *

Q. 9. Please refer specifically to Mr. Loftin's answer to Q. 31 of his deposition (P. R., p. 1113), and state to what extent, if any, Mr. Loftin may be justified in criticising your previous testimony (P. R., p. 171), in which you quote from "Plaintiff's Exhibit No. 78," Navy Manual 1913, and "Plaintiff's Exhibit No. 91," Signal Corps book entitled "Radiotelegraphy," on the subject of resonance or equal [fol. 1530] time periods in quenched-gap transmitters.

A. Mr. Loftin does not meet the issues raised in his question 31, he merely avoids them.

Wherever the question of resonance arises, Mr. Loftin similarly avoids the issue by setting up his own special definition of resonance, which is not an accepted definition, but something for which the has coined the name "cumulative resonance," devised to meet the exigencies of his testimony.

The word "resonance," as applied to the Marconi patent, and as used in the art, means equality of electrical time period (Patent 763, 772, p. 2, 1. 127).

Furthermore, Mr. Loftin, in this answer, does not refer at all to some of the matters raised by my *prima facie* testimony, to which the question referred him.

I will consider the references *seriatim*. The Navy Manual of 1913, "Plaintiff's Exhibit No. 78," page 57, was quoted by me as follows:

"89. In fig. 29a, A B and C D have been given some turns in common, forming an air core-autotransformer, but, whether directly or indirectly connected, these two circuits —the *closed* and *open* circuits—must have equal natural periods in order to produce and radiate electric waves efficiently."

This statement immediately follows the discussion of the quenched-gap transmitter, of which, however, no circuit diagram is given. The reference to "Fig. 29a" is for the purpose of explaining that although there is but a single coil in some transmitters, there are, nevertheless, two circuits "the *closed* and *open* circuits" and the statement is that the two circuits of a transmitter "must have equal natural periods," which means that their "electrical time periods" must be the same.

It is evident from the entire context that this statement is perfectly general and applies to transmitters having closed and open circuits, without regard to the form of the spark gap.

On page 162 of the same publication, Mr. Loftin repeats my former quotation as follows:

"ADJUSTMENTS.

"224. This includes *calibration* and *tuning*. *A station is tuned when both sending and receiving circuits are correctly calibrated, coupled and adjusted to the standard damping and standard wave lengths.*" (Italics in the original.)

Mr. Loftin avoids this by saying that it is a very general statement and might apply to a system having but one circuit at each station. If he had read the rest of the paragraph, which expressly includes the closed circuit, as well as an open circuit at each station, he would have seen that his statement was incorrect. This above-quoted statement is in the chapter entitled "Installation, Adjustments and Measurements," and immediately follows the specific de-

scription of transmitting and receiving apparatus, the immediately preceding page to this chapter being a diagram of a quenched-gap transmitter.

Continuing under the same heading on the same page and referring broadly to any "spark sending set," I find the following also quoted by Mr. Loftin:

"The closed sending circuit should be in resonance with [fol. 1531] the open circuit and coupling and decrement of the open circuit such as to afford the necessary selectivity to the receiving circuits with the best efficiency of radiation."

In order to avoid the effect of this quotation, Mr. Loftin actually misinterprets it and attempts to make it a mere nonsensical statement without meaning. There is no excuse for such a distortion; the meaning is perfectly clear; it says, first, that the sending circuit should be in resonance with the open circuit and, second, that the coupling and decrement of the open circuit should be such as to afford the necessary selectivity in reception, together with best efficiency of radiation.

The meaning is conclusive that the writer definitely intended to say the circuits of the transmitter should be in resonance, without regard to the kind of spark-gap used. It is interesting to note that the sense in which this writer uses the term "resonance" is identically that of the Marconi patent, as will be seen by reference to page 43, where I find the following:

"63. Two circuits having the same electrical length are said to oscillate in *resonance;* their periods are equal, though the inductance and capacity may not be the same in each."

This means that the circuits have the same time period, electrically speaking, and the products of their inductance and capacity are alike.

On page 163, Mr. Loftin quotes the next sentence following the last quotation on page 162, as follows:

"Receiving circuits to receive from such a sender should be in resonance with each other and with the sending circuits and should have the same coupling as the sending circuits. The telephone diaphragm should be in resonance with

the wave train (alternator) frequency and with the operator's ear."

Mr. Loftin says that this quotation is important, because it shows that the previous quotation as to the sending circuit refers to the open-gap type, rather than the quenched-gap type. It does not. Mr. Loftin is mistaken, because he bases this statement on his own peculiar and limited definition of "resonance," which is not the definition of the Marconi patent, nor is it a definition having any application in this case, or any warrant that I know of, outside of Mr. Loftin's own mind.

The important thing is this statement, together with the preceding quotation from page 162, used terms as defined by the writer of the book (Navy Manual, "Plaintiff's Exhibit No. 28"), and it will be noted that these two statements toge' r exactly correspond to the instructions of the Marconi patent in suit, page 2 lines 118 to 128, where Marconi also says that all four circuits of the sending and receiving systems should be in tune.

Mr. Loftin also alleges that this quotation "is also a very loose technical statement," and says in effect that there is no sense in having the circuits of the receiver in tune with the circuits of the transmitter. This, however, is merely another attempt to enforce his peculiar definition of "resonance" and has no foundation in fact.

The two quotations are accurate technical statements and describe the necessary conditions for the operation of the [fol. 1532] apparatus of the Marconi patent, whether the transmitter employed one form of spark-gap—as, for instance, the so-called quenched spark gap—or some other, such as the ball-gap shown in the Marconi patent.

In my former answer, as found on P. R., p. 171, I quoted portions of a resume statement from page 163 of "Plaintiff's Exhibit No. 78," which manifestly and necessarily includes the quenched-gap form of spark-gap, as well as the open-gap. Mr. Loftin has neglected to refer to this at all. The portion which I quoted in my previous deposition reads as follows:

"Resonance is thus seen to be a vital quality in wireless telegraph circuits. * * * (3) Resonance of closed oscillating circuit with open radiating circuit. (4) Resonance

of coupled receiving circuits with each other and with coupled sending circuits."

Since this a resume and clearly includes all of the apparatus previously described by the author and uses the term "resonance" with the author's meaning, and *not* with Mr. Loftin's meaning, it is important. It seems strange that Mr. Loftin should have omitted to refer to it.

It is a succinct statement that the condition laid down by the Marconi patent 763,772 is recognized by the author as the proper condition and "a vital quality" without regard to the type of spark-gap employed.

Mr. Loftin next quotes from the Signal Corps book, "Radiotelegraphy," "Plaintiff's Exhibit No. 91," page 67 (the reference "page 57n" P. R., p. 1115, is a typographical error; the correct reference is given P. R., p. 171). The quotation reads as follows:

"When the adjustments of a quenched spark transmitter have been correctly made—that is, the circuits are in resonance, the coupling is right, etc.—a simple experiment will show that the primary current is a *minimum;* that is, the spark has been quenched and the primary current has been stopped quickly, as at the point Q. of figure 53, and that at the same time the secondary current is a *maximum;* that is, it persists for a long time, as shown in figure 56." (Italics in the original.)

Mr. Loftin admits that this quotation definitely refers to the quenched-gap type. He says, however, that the statement is incorrect; that the use of the word "resonance" is incorrect. This, as I have repeatedly pointed out, means merely that it does not agree with Mr. Loftin's use of the term.

The author of this Signal Corps book, "Plaintiff's Exhibit No. 91," defines the sense in which he uses the term as meaning that the two associated circuits have "the *same frequency*" and he continues: "that is, the two circuits are *syntonized*, or *tuned*, or are *in resonance*." (Italics by the author.)

Two circuits which have the same frequency, have the same electrical time period. The usage by this author of the Signal Corps book is, therefore, in accord with that of the Marconi patent, in the passage above noted.

The statement in this book is entirely correct, as I know by the very large number of tests which I have personally made. It is only Mr. Loftin's definition of "resonance," for which he is the sole authority, that is wrong.

It is interesting to note that Figure 56, to which this quo- [fol. 1533] tation refers, shows three oscillations in the primary circuit, and that applying Prof. Morecroft's rule, to which I have already referred, this means a coupling of about sixteen per cent., which is within the range of the illustrative examples given by Marconi. It is interesting to note, also that the same statement is true of the illustration on page 56 of "Plaintiff's Exhibit No. 78," Navy Manual. These authorities, therefore, are in all respects in accord, and in total disagreement with Mr. Loftin. They are also in accord with my testimony in my former deposition and in this deposition.

In my former deposition (P. R., p. 171), I quoted from this same book (Signal Corps Manual, "Plaintiff's Exhibit No. 91"), the following statement from page 107, as follows:

"The tuning of the closed and open circuits to resonance, and the determination of the correct coupling between them are the two most important adjustments in a quenched spark transmitter."

Mr. Loftin neglects to refer to this quotation, although it is a flat contradiction of his entire testimony on this subject and, as I have just noted, the coupling referred to as illustrated by the author on page 67, is within the range of coupling of the apparatus by which Marconi illustrates his invention. Remembering that the author's use of the word "resonance" is exactly in accord with that of the Marconi patent, and that it is used specifically with reference to a "quenched spark transmitter," it will be seen that the denial of the correctness of Mr. Loftin's position could not be more complete. It is strange that Mr. Loftin should have overlooked this quotation.

Q. 10. In answer to Q. 2 of your present deposition you referred to the limitations of the apparatus involved in the invention of Marconi's plain antenna patent (re-issue No. 11,913), and the problems Marconi had to solve, and how he solved them by the invention comprised in his tuning patent No. 763,772, thereby producing the result of selectivity and long-distance signaling. Were these results due wholly to

the improved mechanism of the invention of the tuning patent, and not to a mere increase in energy or power applied to such mechanism? If so, what is the technical explanation of Mr. Marconi's comparison of these two systems, particularly referred to on page 24 of "Defendant's Exhibit K-6," Marconi's lecture of 1909 before the Stockholm Royal Academy of Science. Please give your reasons for any opinion you may express in your answer.

A. Considering the two structures on a comparable basis, the improved results of Marconi patent No. 763,772 are wholly the result of the improved mechanism of this tuning patent. The subject is an involved one, and it is doubtful, perhaps, whether Mr. Marconi himself would today make the same statement, because we now know so much more about the vagaries of transmitting conditions, that we are well aware that the comparison of efficiencies on a basis of distance attained at different times, is wholly without meaning. It is well known that for little-understood reasons a given transmitter will often make a record for distance that could not normally and regularly be made, except by a transmitter of many times the power.

The only basis upon which such a comparison could be made would be to have two transmitters having the same size of antenna, emitting the same wave-length, and applying the same power, and operating successively one immediately after the other, for repeated trials, the signals being received [fol. 1534] in both cases, with the same receiver, in the same state of adjustment. Of course, this would mean that the transmitter of the tuning patent would be operating way below its normal power-input, or else that the plain antenna transmitter of patent re-issue 11,913 would be operating with an excessive voltage and, hence, in excess of the high-resistance spark-gap. If any such test has ever been made, I am not acquainted with it.

The practical test would be to arrange a given antenna and ground connection, so that they could be alternately switched from a transmitter of the type of patent 11,913 to a transmitter of the tuning patent, each being adjusted for its best performance with the same charging voltage. This test I have made. Naturally, I could not be at both the transmitting and receiving ends, but judging from the readings of the antenna ammeter, the comparison was extremely unfavorable to the plain antenna transmitter.

As Mr. Marconi himself points out in the exhibit referred to, either an enormous antenna has to be used to radiate a comparable amount of energy from the plain antenna, or else brush-discharge losses and spark-gap losses become so large that there is no gain in trying to increase the energy.

The comparison is easily seen by taking the illustration used in one of my former answers, where I pointed out that assuming the antenna of tune 1, page 4 of patent 763,772, to have a capacity of about 200 micro-microfarads and taking the capacity of the primary circuit as given for that tune to be approximately 7,000 micro-microfarads, the ratio is one to thirty-five. Assuming the same spark-gap and spark-gap potentials and the same antenna, it is thus seen that the energy delivered to the two transmitters (of patent 11,913 and patent 763,772 respectively), would be in this ratio of one to thirty-five. If the signals were received by the same receiver and comparisons made of the signal strengths, it would be found that the effectiveness of the transmitter was of this order, if not considerably greater. The reason it might be expected to be greater, is that the transmitter of patent 11,913 would not give as well-sustained a train-of-wave, because of the resistance of the spark-gap in the antenna.

This last condition means that the signal from the plain antenna could not be as selectively received. It would interfere with other receivers tuned to a widely different frequency. Comparable results in this regard could not be attained with the plain antenna, using the same antenna in both cases, which is the only basis on which the comparison would be instructive.

These greatly-improved results of the arrangement of the tuning patent are due wholly to its construction; that is, to the mechanical differences between it and the transmitter of the plain antenna patent. I, of course, use the word "mechanical" broadly to refer to the different elements, and association of elements which go to make up the two transmitters and not as distinguishing between what might be called mechanical and electrical elements.

This answer also holds good, whether the spark-gap employed has one form or another, always assuming that the structure of the tuning patent is arranged in conformity with the patent, and that the spark-gap is properly proportioned to its load, and in good operating condition.

Even if the arrangement of the tuning patent made no [fol. 1535] gain in power and distance, as determined by the strength of field created at the receiving point (which is the true measure of effectiveness, because it measures the energy at the receiver) the apparatus of the tuning patent would, nevertheless, be used to the exclusion of that of the open antenna patent, because of the greatly reduced interference which it would cause and the better effective reception. In other words, two transmitters, one of each type, which created the same field strength at the receiving point, would not be equally effective. The one might be intolerable, because of the interference which it created, or give an impracticably weak signal, because incapable of sharp tuning, while the other would be effective and would not cause a comparable amount of interference. Thus the effective range of the transmitter of the tuning patent would be greater, and its interference less, and these qualities are wholly the result of an enormous improvement in the combination constituting the transmitter invention of the tuning patent.

Q. 11. Please refer to Mr. Loftin's answer to Q. 37 (P. R., p. 1139), in which he refers to "Plaintiff's Exhibit No. 105," produced during your former deposition as illustrating the apparatus of the National Electric Signaling Company, including plain, rotary and quenched-gap transmitters, and which was before the Court in the suit against that company, for the infringement of the Marconi tuning patent, and particularly where Mr. Loftin statse that he failed to find

"any evidence in the opinion of Judge Veeder having considered anything but the plain or open-gap type, this being at page 849 of the report, where he states:

" 'While in the defendant's apparatus the loose coupling emits only one wave and admits of electric resonance.'

"This conclusively shows that defendant's apparatus in that case which was considered in Judge Veeder's opinion was of the Stone or Marconi plain-gap type, depending upon loose coupling."

Does this quotation from Judge Veeder's opinion show that he considered in that opinion only the plain-gap type of transmitter used by the National Electric Signaling Company, and from your knowledge of that case did the defendant claim that its quenched-gap apparatus did not infringe the tuning patent?

A. It does not. To understand the opinion one should understand that the defendant in that case did not contend, as does Mr. Loftin, that the Marconi patent was limited to some excessively loose coupling. On the contrary, it contended that it was limited to what Mr. Loftin calls a "tight coupling." It contended that the Marconi patent was limited to an operation in which the four circuits all stayed in operation all the time, and that the defendant's transmitter, when used with that particular gap, called the quenched spark-gap, was so loosely coupled with respect to the capacities of the gap that the gap ceased to operate after the energy had been transferred to the antenna.

I was a witness in that case and testified to the same effect as I have here testified, viz., that the transmitter of the Marconi patent was not altered by changing the type of gap, that all three forms of gap illustrated in "Plaintiff's Exhibit No. 105," were equally within the disclosure of that patent, and that except as to questions of degree, they operated in the same way.

[fol. 1536] The defendant, through its expert witness, Dr. Kennelly, contended specifically that the transmitter with the quenched spark-gap was not within the disclosure of the patent, and that the patent did not disclose loosely coupled circuits.

Judge Veeder, therefore, not only had before him all three forms of spark-gap, viz., the ball-gap, the quenched-gap and the rotary-gap (Plaintiff's Exhibit No. 105" in this case), but he had testimony at length regarding the transmitter with the quenched-gap specifically.

There is, therefore, not the slightest reason for any supposition that Judge Veeder was not referring collectively to all of the forms of the National Electric Signaling Company apparatus. I have, of course, no way of knowing what was in Judge Veeder's mind, but I was present throughout the trial and know that he listened to the testimony attentively, and that he appeared to understand it. I would therefore assume that had he intended to base his decision on part of the apparatus only, and exclude the rest, the fact would have been made clear in his opinion.

The quotation from Mr. Loftin's testimony contained in the question, indicates to me that Mr. Loftin was again merely hiding behind his own unwarranted restriction on the meaning of the word "resonance." Certainly there is no other possible basis for his deduction from Judge

Veeder's opinion, that I can think of. It seems clear to me that Judge Veeder accepted the views of the Marconi invention, and its application to the defendant's apparatus, presented by the plaintiff in that case, and rejected the views presented by the defendant.

* * * * * * *

[fol. 1537] Q. 12. Please refer to the synchronous rotary gap transmitter used by the Navy and purchased from the NESCO around the year 1910, according to the testimony of Mr. George Clark (P. R., p. 231); also the non-synchronous rotary gaps purchased and used by the Navy in 1911 [fol. 1538] and 1912, and continuing up until 1919, according to the testimony of Mr. Clark, and as illustrated in Mr. Clark's drawing "Plaintiff's Exhibit No. 112" (opposite P. R., p. 241), and compare the same with the construction and mode of operation of the rotary gaps involved in the suit of Marconi *vs.* National Electric Signaling Company, and with the inventions of the transmitter claims in issue of Marconi tuning patent No. 763,772, pointing out any similarities and differences which, in your opinion, exist between the rotary gaps referred to by Mr. Clark and that involved in the suit against the National Electric Signaling Company and the inventions of the said claims of the Marconi tuning patent.

A. Mr. Weagant, in his answer to Q. 197, beginning at P. R., p. 1608, has clearly and briefly outlined the development of spark-gap construction, and I am in entire agreement with his testimony.

As I have pointed out, the apparatus constituting the transmitter of the Marconi patent 763,772 in suit at once established its enormous superiority over everything that had gone before, and the progress of the art from that time on the transmitters consisted largely in the production of improved details, of which the spark-gap was one. The effort was to produce spark-gaps which, by reason of better cooling required less care to obtain perfect results and would permit larger powers to be employed. One form that this development took was the production of so-called "rotary" gaps. As Mr. Clark's diagram indicates, these commonly consisted of two fixed electrodes, between which rotated a wheel having numerous electrodes so mounted as to come into line with the fixed electrode. Sometimes this arrangement was reversed, and there were a large

number of fixed electrodes circularly arranged, while a pair of electrodes were mounted to rotate with relation to them. The principle is the same. By this means excellent cooling was obtained and, so far as I know, the largest amounts of power that were ever transmitted with damped oscillations have been transmitted by the arrangement of this Marconi patent in suit, employing rotary-gap transmitters.

The synchronous gap of the National Electric Signaling Company, referred to in the question, and the nonsynchronous gaps referred to in the question, are illustrations of this rotary gap construction. The designation "synchronous gap" merely means that the rotary element is driven by the generator which furnishes the energy to the transmitter, while the designation "non-synchronous" means that the rotary element is not driven by the generator and, consequently, does not have a speed of rotation definitely related to that of the generator.

As is the case with the multiple gap transmitter of the particular construction which has been called the "quenched-gap," these rotary gaps merely replace the ball electrodes shown in the Marconi patent and the system functions in the same way in all cases, when properly adjusted. These gaps, in other words, constitute the "spark producer or other electric wave or oscillation producer" referred to by Marconi at page 1, lines 83 to 85, of patent 763,772.

As with the quenched-gap, the ball-gap, and all other forms of gaps, the effectiveness of the rotary gap in permitting the rapid transfer of energy to the antenna made possible by the resonant adjustment of the circuits depends upon the condition and construction of the particular gap in use. For a number of years I operated a transmitter provided with three forms of spark-gap, either of which could be used at will. One was an open-gap, having [fol. 1539] cylindrical electrodes, with hemispherical terminals, one was a non-synchronous rotary gap, and the third was a quenched-gap. I found that the resonant adjustment of the circuit was always the same, no matter what wavelength was used within the range of the transmitter for best results, but that the coupling necessary for proper quenching to give the operation illustrated in my "Sketch No. 2" (opposite P. R., p. 151), differed, being very loose in all cases, as compared to the possible maximum of 100 per

cent., being slightly looser for the rotating gap than for the "quenched" gap, and still looser for the plain open gap. In the testing of many other transmitters I found it to be generally the case that the operation is the same, without regard to the character of the gap, the sole difference being a matter of degree in the association or coupling of the circuits.

The rotary gaps referred to by Mr. Clark correspond in all respects to the rotary gaps involved in the suit of Marconi *vs.* The National Electric Signaling Company, in which I was a witness, and the transmitter claims in issue apply to them in the same way as to transmitter having the open gap or the quenched gap, in my opinion. As I understand these transmitter claims, which are 1, 3, 6, 8, 11 and 12, they are in no way restricted by their description to any particular sort of spark-gap construction. Claims 1, 3, 6 and 8 refer to the spark-gap as "a means which automatically causes oscillations of the desired frequency." Claims 11 and 12 refer to it as "a producer of electric waves of high frequency." These expressions correspond to the description of the specification, which refers to what is now called the spark-gap as "a spark-producer or other electric wave or oscillation producer." It is, therefore, immaterial what the precise construction of the spark-gap is, in my opinion, and the rotary gaps referred to in the question are such as I understand are referred to in these claims, just as are the so-called quenched-gap and the so-called plain gap. The transmitters being otherwise in accordance with the description of the claims, it is immaterial, so far as the correspondence of the transmitter to the description of the claims is concerned, whether one of these constructions of spark-gap is used, or another.

Q. 13. Will you please consider the paragraphs quoted by Mr. Loftin in connection with the construction and mode of operation of the apparatus described in the Stone-Baker letters purporting to be dated June 30, July 18 and July 22, 1899, and referred to in the depositions of Messrs. Stone, Baker and Dean herein (P. R., pp. 863-952), and referred to by Mr. Loftin in his testimony relating to such apparatus (P. R., pp. 1076-1082, 1086, 1091), and state what, if any, bearing said letters have on the apparatus and the mode of operation of the Marconi tuning patent in suit, and particularly the apparatus recited in the claims thereof, in

issue herein, giving your reasons for any opinion you may express.

A. I have read the testimony referred to, and I have also read Mr. Weagant's testimony beginning at P. R., p. 1650. I disagree entirely with Mr. Loftin's opinions as expressed in his testimony, and I agree with the statements and opinions of Mr. Weagant.

Mr. Loftin, in my opinion, entirely overlooks or ignores the point of view of the art and the meaning of terms. What the art then knew as a wireless telegraph system comprised an elevated wire hung in the air, and one terminal connected to earth; there was one such wire at the [fol. 1540] transmitter and the other at the receiver. This was the original Marconi arrangement disclosed, for example, in his re-issue patent No. 11,913.

As disclosed in that patent, these two wires,—one constituting the transmitter, and the other the receiver,—had the same fundamental wave-length, which meant that the transmitting wire sent a signal whose fundamental wave-length was determined by the wire, and because the receiving wire had the same wave length, it readily picked up the signals so sent; that is, it readily responded to that wave-length.

By means of a spark-gap in the transmitting wire, this wire was charged, as, for example, by an induction coil, and its discharge gave rise to the waves which, however, due to the rapid radiation, the high resistance of the spark-gap and the small initial charge, rapidly died out and became insensible.

As Stone points out, the wave-trains which were emitted were of an extremely complicated nature, expressable mathematically by a long mathematical expression, which is worth quoting because these Stone-Baker letters are mathematical in character and show that what Stone was engaged in was a mere excursion into mathematical realms. The expression is:

$$\text{“}A_1 \sin pt + A_2 \sin 2\,pt + A_3 \sin 3\,pt + \&c$$
$$B_0 + B_1 \cos 2\,pt + B_3 \cos 3\,pt + \&c\text{”}$$

Stone undertook to provide a means of simplifying the radiation, which he thought he could do by avoiding the shock of discharge and producing what he calls "forced

current vibrations in the wire" and he reasoned that he would accomplish his object because, as he says:

"forced vibrations as is well known depend for their period and form only upon the period and form of the impressed forces and not upon the electro-magnetic constants of the circuit in which they are developed as is the case with *free* or *natural* vibrations of a system."

Stone proposed to retain the transmitting wire, with its fundamental frequency of oscillation or wave-length, and the receiving wire of the same fundamental wave-length, and he proposed to force thereon "oscillations from an oscillator, which oscillations shall be of a frequency corresponding to the fundamental of the wire." To accomplish this result Stone associated his oscillator with the antenna or vertical wire by means of a transformer, one coil of which was connected between the wire and earth. In this manner he forced upon the wire, according to his first letter, oscillations of the same frequency as the fundamental of the wire, and he expected thereby to realize simple oscillations which he thought he had mathematically proven to be represented by the simpler mathematical expression

"A_n sin pt."

Stone's idea was a very natural one. Forced oscillations were well known and, according to Stone's mathematics, expressed in the second Baker letter, he should be able to force oscillations on the wire of his mathematically simple form, without regard to the fundamental frequency or wave-length of the wire.

Furthermore, Stone's idea was perfectly sound: he could force oscillations on the wire, but the trouble was, and is, that they are of no use, because of their excessive feebleness. Stone's mathematics was defective because of its [fol. 1541] incompleteness. Nature does not work that way, and the scheme proposed by Stone is utterly worthless because of its extreme ineffectiveness and the feeble character of the oscillations produced.

Stone followed the thought of the art. Marconi, in the patent 763,772 in suit, entirely disregarded it; that is, he entirely disregarded the teachings of his own former work and obtained a true grasp on the processes of nature. He entirely ignored the fundamental frequency of the wire

as it had existed, and directs that a radiating *circuit* should be constructed, having such a wire that when combined with the coil of his oscillation transformer and, if desired, also with an additional coil *g* (Fig. 1 of patent 763,772), the whole combination circuit should have the same fundamental frequency as that of the non-radiating circuit in which the oscillations were generated.

This is a wholly different concept from that of Stone: Stone's instructions were to erect a wire having a given fundamental wave-length, to construct an oscillator that would generate oscillations of that same wave-length and to force these oscillations upon the wire. He thus entirely missed Marconi's concept and failed to produce anything useful. Of course, he also did not attain the simple oscillations that he expected. Indeed, such oscillations as he could have produced would have been of still more complex form, which could still have been represented by the very complex formula quoted above by adding more terms, which are indicated in the formula given by Stone by the character "&c."

It does not seem to me to be profitable to consider in detail Mr. Loftin's long dissertation on these letters; he reaches his conclusion by disregarding what Stone says and credits him with saying something which he does not say. Stone throughout deals solely with the existing concepts of the art and undertakes to produce forced oscillations of the frequency of the wire itself. The idea of forced oscillations was a familiar one to the art, and has been well explained by Mr. Weagant.

Mr. Loftin credits Stone with the disclosure of a circuit comprising the wire and the coil, which are together so proportioned as to have the fundamental frequency of the oscillator. No such idea is stated or suggested by Stone, either in words, or in mathematical symbols, nor is it in any way implied by his diagram. I regard it as a wholly gratuitous assumption on Mr. Loftin's part, not having any foundation in the Stone-Baker letters.

I call attention to the fact that the second of his letters makes it clear, both mathematically and in terms, that Stone regarded it quite immaterial, so far as attaining his result of simple oscillations is concerned, or apparently any other result, whether the oscillator was made to have the frequency of the wire or not.

In my opinion, the Stone-Baker letters do not disclose structures such as set forth in the Marconi patent or referred to by the claims thereof in issue.

Q. 14. Will you please state the fundamentals of construction, function and mode of operation of the two-electrode vacuum tube, and also of the three-electrode vacuum tube, as a result of your personal knowledge and use of such tubes, and state in what respects they are similar or dissimilar. In this connection I will ask you to please consider the testimony of defendant's witnesses as comprised in the answers of Dr. John H. Miller to Q. 6, beginning on page 764 of the Printed Record, to Q. 17 (P. R., p. 786) and Q. 21, (P. R., p. 805), where he states what [fol. 1542] he considers to be essential differences, and the answer of Dr. Robert A. Millikan to Q. 12, beginning on page 845 of the Printed Record, pointing out in what respects, if any, you may agree or disagree with the statements made by these witnesses and the conclusions expressed by them, as bearing upon the technical facts, inquired about in this question?

A. In answering the question and in considering the testimony referred to therein, it is important to bear in mind that Fleming's patent was applied for in 1905, De Forest's patent was applied for in 1907 (I refer to the so-called grid patent, "Defendant's Exhibit K." No. 879,532), while Dr. Miller and Dr. Millikan testified in 1923, when their minds were so filled by the enormous mass of information acquired subsequent to 1907, that they were entirely unable to get back to the fundamental basis on which these structures rest.

Only the most trifling part of the knowledge with which these gentlemen testified could have been known to Fleming and to DeForest, when they were working their two and three-electrode vacuum tubes. I shall endeavor to get back to the fundamentals involved which constituted the great disclosure of Fleming, on which the whole radio art of vacuum tube devices rests.

These two and three-element vacuum tubes are, in substance, small incandescent lamps, within the bulbs of which there is, in addition to the filament, a cold plate with a terminal wire sealed through the glass in the same manner that the filament wires are sealed, so that electrical connections can be made with them without disturbing the vacuum existing in the bulb. In the three-element bulb,

there is an additional cold element, which most commonly takes the form of a sort of wire screen, usually interposed between the filament and the first-mentioned cold plate; this grid-like element also has a lead wire sealed into the glass for purposes of external connection.

When the filament is not lighted, the space between the elements inside the bulb is non-conducting for ordinary voltages or electrical pressures such as are used with these bulbs in practice. When, however, the filament is heated, the vacuous space becomes conducting and this fact had been known and intensively studied for very many years prior to Fleming's application for his patent 803,684, in suit. It was a very extraordinary phenomenon which ultimately came to be explained as the result of the emission by the filament, due to its high temperature, of minute charges of negative electricity derived from the atoms of the filament material.

After studying this strange phenomenon for many years and publishing results of many experiments on it in various technical publications, Fleming at length discovered that these minute charges of electricity given off by the hot filament could be utilized to give highly useful results in wireless or, as we now call it, radio telegraphy, and he disclosed in his patent, and in publications of about the same date, how to so use such a device, and some of the fundamental ways in which it acted.

The most fundamental fact involved in such vacuum tubes is that the electrons given off by the heated filament can be controlled by electrical voltages connected exteriorly between the cold plate element and the filament, even when these voltages oscillate at the enormous frequencies employed in radio work. When there is no voltage applied between the cold element and the filament, the electrons [fol. 1543] which are emitted have, so to speak, nowhere to go, and in general return to the filament. If, however, the cold element is made positive with respect to the filament, then some of the emitted electrons follow the electrostatic field so created and pass from the filament to the plate, so that a negative current flows from the filament to the plate as long as the latter is maintained positive and the filament is kept lighted.

Fleming discovered that if such a tube were so associated with a receiving antenna, or other source of wireless or high-frequency currents, so that the plate element was al-

ternately made positive and negative with respect to the filament, electricity flowed between the plate and the filament when the plate was positive, but did not flow when the plate was negative. This, at any rate, was his theory and it is the theory still entertained today by those skilled in the art.

By putting a receiving instrument either in the same or in another circuit between the plate and the filament, an indication was obtained showing or detecting what was happening in the bulb and, therefore, in the antenna connected to it; if waves were received indicating a dot or a dash, the indicating instrument responded with a visible or audible signal corresponding to a dot or a dash. In the same way, if a current carrying the voice or music, is received, a telephone connected with the bulb responds by reproducing the voice or music. This result is due to a variation of potential between a cold element in the bulb and the lighted filament therein.

If a battery or other constant source of voltage is applied, so as to make the cold element positive with respect to the filament, a steady current flows between the plate and filament (negative in the direction filament-to-plate). if, while this battery is still applied, radio-frequency signals are also impressed between the plate and the filament, this steady current is correspondingly varied up-and-down and, by properly adjusting the battery voltage, very loud signals may be obtained.

DeForest improved the Fleming two-element vacuum tube by putting into it the third or grid element. That it was an improvement, no one would deny, but he did not thereby take away from the device any of its former properties. If the filament is not lighted, the space is non-conductive. If the filament is lighted, but no voltage is impressed between either of the cold elements and the filament, nothing happens. If a voltage is impressed between either of the cold elements and the filament, or both connected together, and the filament, the same results happen as in the case of Fleming's two-element bulb. If the voltage so impressed is a radio-frequency voltage, the same effects happen as already described. If a battery or a battery and a radio-frequency signal are impressed, the results are as in the simple two-element structure.

As in the case of the two-element bulb, the battery and the radio signal do not have to be applied by the same ex

ternal connections, the signal may be applied by one external connection or circuit, and the battery by another, and the external indicating device may be in the circuit of the battery.

A well constructed two-element bulb used as a detector has a sensitiveness when a tungsten filament is employed, about equal to, or slightly superior to, that of the best crystal detector (Dr. Miller says it is only one-fourth or one-fifth as sensitive, but this only shows that he never used, or never properly used, a good two-element tube). When there are three elements in the tube, as shown in the DeForest patent 879,532, it is possible to connect the cir- [fol. 1544] cuit containing the radio signal between the filament and one of the cold elements, while the battery and indicating device are connected between the filament and the other one of the cold elements. In other words, the two functions, viz., that of receiving signals and of carrying the steady current and indicating the response, are divided between the two cold elements, and when this is done, an improvement in sensitiveness is attained, which Dr. Miller rates at about fifty per cent., as compared to the crystal.

What it is that happens under these circumstances is fundamentally the same as before. If a signal is applied to either of the cold elements, the potential of that element with respect to the filament changes; when it is made positive, electrons pass from the filament to the element; when it is negative, they do not, unless a battery is also connected from the positive terminal to the element, in which case the number of electrons passing changes, that is, the current waxes and wanes. If the signal is applied, say, to the grid element, which is not otherwise polarized, while the plate element is polarized by a battery, then when the grid element is made positive by a signal, electrons flow to the grid, and when it is negative, electrons do not flow to the grid. When electrons flow to the grid as the result of a positive potential, if a measuring instrument were inserted and a positive potential maintained, a certain current would be found to flow between the grid and the filament. Assume that the same voltage were now transferred to the plate element (assuming no polarizing battery present), a current would similarly flow between the filament and the plate. In other words, the two elements considered

separately, act in the same way. When a positive voltage is applied to the grid while there is also a battery connected to the plate, not only does a current flow between the grid and the filament, but an increase of current occurs between the plate and the filament. The same result would occur if the signal were also applied to the plate. The three-element bulb, however, has the advantage that when the signal is applied to the grid or a positive voltage is so applied, the increase of current between the plate and the filament will be greater than it would have been if the same positive voltage had been applied directly to the plate. In other words, while the effects are the same in kind, the three-element construction is an improvement. It enables a more effective control of the electrons emitted by the filament to be had; and, further, because there are three elements to be utilized, instead of only two, the device is obviously susceptible of more different modes of connection. For example, one of the cold elements may have one polarizing battery connected to it, and the other may have another and different battery connected to it, and these may be even opposite in polarity. In this way, a great widening in the possible range of control of the electrons emitted by the filament may be had. In all cases, however, the operation is the same, and consists in the control of the movement of the electrons which are emitted by the heated filament.

Referring to the answer of Dr. Miller, mentioned in the question, Dr. Miller admits that "the operation of both the Edison two-element valve, which is Fig. 1 of my drawing A, and the three-element DeForest device, which is Fig. 2 of that drawing, depend upon the peculiar property of a heated filament of emitting what are called electrons or thermions."

Although I do not find that Dr. Millikan makes any simi- [fol. 1545] lar explicit statement, he does not deny the facts stated by Dr. Miller. I assume, therefore, that it may be taken as admitted that these two devices, the two-element and the three-element vacuum tubes operate, in view of this fundamental fact.

Dr. Miller then proceeds (P. R., p. 765) to state briefly with respect to the two-element bulb, some of the controlling effects which exist when the cold element is polarized with respect to the filament and subjected to oscillating currents. When, however, he passes to the consideration of the three-

electrode vacuum tube, he fails to point out that exactly the same sort of control occurs. He passes at once to the statement that "the three-electrode device of DeForest, which is Fig. 2 of drawing A, is an amplifying relay or amplifier."

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Dr. Miller thus entirely omits any fundamental consideration of the three-element tube and passes at once to the uses of it, for which he has laid no foundation by a description of its fundamental properties, other than the admission which I have quoted, that it depends "upon the peculiar property of the heated filament of emitting what are called electrons or thermions." He does not state that like the two-element valve, it depends upon the fundamental fact disclosed by Fleming, that when these electrons are subjected to electrostatic field effects produced by high-frequency radio currents, they are able to respond thereto and that the utility of the device depends upon this ability. If he had pointed out this fact, the fundamental identity of the devices would have been at once apparent.

He did not point out that the mere emission of the electrons by the filament is of no more consequence in the three-element device, than it is in the two-element. He did not explain that the operation of the device depended upon the same fact as the two-element device, viz., that by an additional electrode and an external circuit connected thereto, and to the filament, electrostatic field effects are produced in the same manner as in the two-element valve, which control the response of the emitted electrons and their flow in the external circuit. Elsewhere, of course, he has at various times substantially admitted this, but not in a context which brings out the fundamental identity.

In the diagram which he produces in answer to Q. 6, and which is marked "Miller Drawing B" and is found opposite P. R., p. 765, he does not illustrate the two devices under at all similar conditions, and, therefore, his diagram is highly misleading.

I produce a diagram, "Waterman Drawing No. 5" (Claimant's Exhibit No. 322), in which I have illustrated the two devices under the conditions of Dr. Miller's Fig. 1 of "Miller Drawing B". Fig. 1 of this drawing is the same as Fig. 1 [fol. 1546] of "Miller Drawing B"; Fig. 2 is the same as Fig. 1, except that the bulb shown is a three-element bulb, with the grid element connected to the filament through an

indicating instrument I and the source of oscillations; Fig. 3 is the same except that the plate element is connected to the filament through the same indicator and oscillator elements. The results are indicated by the diagrams below the individual figures. They are identical.

I produce a second diagram, "Waterman Drawing No. 6" (Claimant's Exhibit No. 323), in which I show the three-element and the two-element tubes employed with a battery, after the manner of Fig. 2 of "Miller Drawing B". Fig. 1 of this drawing is like Fig. 1 of "Miller Drawing B", except that it has the battery marked B, in Dr. Miller's Fig. 2. Fig. 2 of this sketch is the same as Fig. 2 of "Miller Drawing B". Fig. 3 of this drawing shows a two-element valve, arranged with two external circuits, after the manner of "Miller Drawing B", Fig. 2, the condenser C acting as a break between the two circuits, fulfilling in this regard the function of the space in the tube between the plate and grid elements. The diagrams below each of the figures show the effect produced, which is the same effect as shown by Dr. Miller's Fig. 2.

Thus it appears, and is the fact, that in their fundamental mode of operation these devices are the same. Both the two-electrode tube and the three-electrode tube take advantage of, and utilize, by causing the received signal to produce a varying electrostatic field, the extraordinary sensitiveness of the emitted electrons from the filament to the effects of such a field. No matter how they are utilized in radio work, including all of the uses which Mr. Miller mentions, it is this responsiveness of the emitted electrons to electrostatic control, even at the highest frequencies employed in radio, and which Fleming discovered and disclosed, that is the cause and means of the operation. Whether the three-element device is acting as what Dr. Miller calls a rectifier, or an amplifier, or an oscillator, it is always responding to and exemplifying that control of the electron stream which is produced by introducing an auxiliary electrode into the tube in proximity to the filament, and introducing by an external circuit, fluctuating voltages between these elements, as Fleming taught.

What DeForest did when he introduced the grid element was to extend this control. The idea of controlling an electrostatic field by the screening action of a grid, was not a new phenomenon in the art. Dr. Miller admits (P. R., p. 767) that it had been recognized and mathematically investigated by Maxwell many years prior. That the introduction

of this screen or grid into the tube greatly increased its utility nobody, I assume, would deny. Certainly, I have never denied it. But in adding this device, DeForest did not abandon anything which Fleming had, nor anything which affected the fundamental mode of operation, by electrostatically controlling the emitted electrons. On the contrary, what he did was to perfect and extend this control.

Fleming, in his Royal Society paper read February 9th, 1905, showed several forms in which his device could be constructed, and gave extensive experimental data illustrated by curves. This showed how the device responded with varying potential differences applied between the cold element and the filament, and how the current, that is, the electron flow between the filament and cold element varied, when this potential difference varied. In this case a battery was employed in the external circuit. Where DeForest's [fol. 1547] grid element was employed, it was found that the same effects occurred, but that the electron flow, that is, the passage of the electrons from the heated element to the plate element when the battery is present, may be altered either by applying oscillations to the plate, as in Figs. 1 and 3 of "Waterman Drawing No. 6", or it may be effected by applying the oscillations to the grid element, as in Fig. 2 of my "Drawing No. 6."

It is found, however, and this is the particular advantage of the arrangement, that a given oscillating electromotive force has a greater effect when applied to the grid, than it does when applied to the plate. For example, in the early DeForest tube which I used, a voltage of one volt applied to the grid was from one and a half to two and half times as effective as the same voltage applied to the plate, other things being the same. At the present time, greater ratios are commonly employed, which have become possible as knowledge of such devices has increased. This ratio is sometimes spoken of as the "amplification ratio" or "constant" of the tube.

Obviously, this is an advantage, as it gives a device of definite amplifying capabilities, but it does it by virtue of the same principle and mode of operation as in the case of the Fleming device, and the device is the same, with simply the addition of the screening element.

It must not be imagined that because the three-element tube shows this ratio of effectiveness of one to one and a half, or one to two and a half, or more, that, therefore, it

always amplifies in that ratio; it does not, and it was many years before much of anything was understood about it or, perhaps, even its existence was known. In the early years after the production of the three-element tube this property existed rather as a dormant capability than as anything to which real effect was given.

The sole basis of Dr. Miller's differentiation and comparisons, to a considerable extent also, Dr. Millikan's, is that when they talk about the three-element tube, they have in mind, as indicated by their statements, the present-day highly-developed bulb, with filaments which represent years of investigation and structural arrangement representing still more years, and endowed with all of the capacities which have resulted from the labors of many workers in the development of circuits while, when they refer to the Fleming device, they refer to the necessarily crude first attempts and insist that it must be considered only in the precise circuit arrangement that Fleming shows. It is like comparing the highly-developed locomotives, stationary engines or turbines of today, with the first crude steam engine of Watt, wherein the piston moved in one direction by taking steam through its full stroke, and in the other by squirting cold water into the cylinder, and allowing the steam to condense. Nobody doubts, however, that the present-day highly developed steam engines are simply improvements upon Watt's device. There is not, of course, any such difference between a two-element device constructed with modern knowledge and a three-element device constructed with the same knowledge.

Dr. Miller, for example, has stated that the three-element device as a detector or frequency converter, gives results about fifty per cent. better than a good crystal, and about five times as good as a two-element valve. On a proper basis of comparison, that statement is not true. I have made an immense number of such comparisons, and a good Fleming valve is equal to, or a little superior to, the best; that is, the most sensitive crystal, when used in place of a crystal in a [fol. 1548] radio circuit and when constructed with a modern high vacuum. It is, of course, true that some of the Fleming valves that were produced in the early days were less sensitive than the crystal, and it is also true that some of the early DeForest three-element valves or audions, were less sensitive than the crystal, when used to replace it in a radio circuit.

In point of fact, uniformity or the possibility of uniformity in either two or three-element tubes is strictly a modern achievement, resulting from a vast amount of investigation and labor, in the standardization of materials, the standardization of dimensions and great exactness of construction to the dimensions, together with cleanliness in construction and perfection and standardization in evacuation processes. The early Fleming two-element tubes and the early DeForest three-element tubes did not have the benefit of any of these improvements, with the result that individual tubes, whether two-element or three-element, differed greatly among themselves.

It is manifestly misleading, and only obscures the true facts, to compare the highly-developed device of one form with the undeveloped device of the other form. It tends to still greater confusion, when the properties which grow out of the development of modern circuits are ascribed to the device itself.

In his answer to Q. 17, beginning P. R., p. 786, Dr. Miller discusses the Fleming two-element device in the arrangement of Fig. 1 of the Fleming patent and the three-element device in the arrangement known as the PX circuit. These two circuits were not new to the art, they are the two circuits of Marconi's prior patent 627,650. Fig. 1 of the Fleming patent corresponds to Fig. 2 of the Marconi patent 627,650 ("Defendant's Exhibit B-6"), while the PX circuit corresponds to Fig. 1 of the same patent. The two circuits differ in that in Fig. 2 of "Defendant's Exhibit B-6" the signal is put into the connected device and the result thereof indicated in a single circuit; whereas, in Fig. 1, to which the PX circuit corresponds, the signal is put into the connected device by one circuit, and the result thereof is indicated in a second circuit. DeForest, in his so-called PX circuit, connects one of these circuits, viz., that which impresses the signal on the tube, between the grid and the filament, while the other of the two circuits, viz., the indicating circuit, he connects between the plate and the filament.

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It will be seen that in Fig. 1 of this patent 627,650 ("Defendant's Exhibit P-5") the functions of receiving the signal and impressing it on the device T are assigned to one of the two circuits, namely, that containing the receiving coil j^2, [fol. 1549] while the function of indicating what happens

is assigned to the second circuit containing the indicator R.

The condenser k' in the input circuit is primarily for the purpose of confining the current from the battery B to its own circuit including the device T. This device T is indicated as a glass tube, containing terminals, known as a coherer, which was a form of detector used at that time, and which required the battery B to be applied to it.

DeForest applied the condenser which is shown at C of Fig. 1 of his three-electrode patent 879,532 ("Defendant's Exhibit K"), for the same purpose, as stated at page 1, line 95, where he says:

"In order to close each of said circuits to the passage of direct current from the aforesaid battery [B] there-through or to prevent the development of a difference of potential between the members *a* and *b*, or between *a'* and *b*, or to prevent the members *a* or *a'* from receiving an electric charge from said battery, I insert the condenser C″ in said otherwise mechanically closed circuit and find that the presence of said condenser produces a great increase in the sensitiveness of the oscillation detector" * * *

DeForest connected his input circuit between the grid and the filament, to impress the signal on the device, and he connected the output circuit between the plate-element and the filament to indicate what happened as a result of impressing the signal. He thus chose the one of the two old Marconi circuits which was appropriate to his three-electrode tube. Fleming having a two-electrode tube, chose Fig. 2 of this Marconi patent 627,650.

Of course, we now know that DeForest did not need the condenser k' (C″ in the DeForest patent) for the purpose which he specified, but DeForest did not know it at the date of his patent. Dr. DeForest did not have the large amount of subsequently-acquired knowledge with which Dr. Miller has testified.

To show the similarity of the fundamental action of the Fleming two-electrode and the DeForest three-electrode devices, I produce a drawing, which I have marked "Waterman Drawing No. 7" (Claimant's Exhibit No. 32-). Fig. 1 shows the two-electrode tube in the circuit of the Fleming patent, and Fig. 2 the three-electrode in the circuit of the DeForest patent, except in each case I have omitted the antenna. Assuming continuous oscillations are being received, the electron-control by the electrostatic fields pro-

duced within the tube of Fig. 1, would produce an electron flow from the filament to the cold electrode during each portion of each oscillation, when the cold electrode was positive, and the flow would be suppressed during every interval when the cold element was negative. This I have indicated by the semi-sinusoid marked "Electron Flow".

In Fig. 2 the same condition exists in the grid circuit. Since there is a condenser in this circuit, this action could not continue indefinitely in the way that I have indicated it, unless there were some leakage path by which the charge in the condenser would be kept from increasing to too great an extent.

In the tubes as DeForest built them, the tube itself was leaky enough to accomplish this result. With the improved tube of today, it is customary to provide a definite leakage for the condenser by putting a very high resistance around it.

[fol. 1550] It will be seen that the control of the electron flow by the electrostatic field produced by the signals is the same. In the lower lines of Figs. 1 and 2 I have shown the current in the telephones indicated by I in the drawing. This would be a steady current, which would be indicated in a galvanometer by a steady deflection in Fig. 1, while in Fig. 2 it would be a similar steady current, which would be evidenced in the galvanometer by an alteration in the deflection which would be produced by the battery B, in the absence of a signal. Of course, in neither case would a sound be produced in the telephone, other than a "click" when the signal began, and in order that a signal might be produced in the telephones, the continuous oscillations supposed to be received would have to be "modulated",—either by suitably breaking them up in sending dots and dashes,—in which case telegraph signals would be received, or varying them to correspond to the voice or music, in which case the voice or the music transmitted would be audible in the telephones.

The purpose of the diagram is to show that in each case, that is, with the two-electrode and the three-electrode tubes, we are dealing with the same fundamental electrostatic control produced by the radio-frequency oscillations, in accordance with Fleming's discovery. Dr. Miller has pointed out the fact that the mass of the electrons is only one two-thousandth part of the mass of the atom of hydrogen for the speeds at which the electron moves in the tube, and that

this is the reason why it is supposed that the electrons can respond at radio frequencies. But the fact that it could so respond was discovered by Fleming and disclosed by him, in his patent in suit, and it is this fact that is fundamental to the operation of the tube, whether it has two electrodes or three.

The two and three-element tubes may be used to detect oscillations by responding to their electrostatic control in another slightly different arrangement, wherein a polarizing or biasing battery is employed. In "Waterman Drawing No. 8" (Claimant's Exhibit No. 325), I have indicated this battery by C, and it will be noticed that they are oppositely poled, because in the two-element tube the single plate element has to perform both of the functions of receiving and indicating; whereas, in the three-electrode tube they are separated. In this instance, therefore, it is the electron flow to the plate-element which is to be compared, since the grid-element is so biased as to leave it to perform solely the function of electrostatic control, while the plate circuit does the indicating. In the two-element tube in this single circuit, the single circuit has to perform both functions.

It will be seen that in each case in the absence of a signal, a certain current would flow, which is indicated by the dotted line in the middle diagram of each figure. The polarization in each case is assumed to be such as will result in an unsymmetrical effect, which Dr. Miller has called a "distortion", to occur when oscillations are received. The result is that the normally flowing direct current is varied more in one direction than the other, with the result that the current in the indicator I is altered, as in the lower sketch of each figure.

Here, again, we have illustrated the same fundamental electrostatic control exerted by the impressed oscillations.

Of course, these diagrams are not intended to be quantitative. As I have already said, the control which is effected when the grid-element is present is greatly improved. The grid-element is enabled to control the current of a battery [fol. 1551] in the output circuit, viz., the battery B. As a result of this improved control, the three-element tube has improved capabilities, but these improved results should not be allowed to obscure the fundamental fact that both act in response to the same primary causes, viz., the emission of electrons in a heated filament in a vacuum and their con-

trol by electrostatic fields produced by cold elements introduced for the purpose, when signals are introduced between the cold element and the filament in each case.

Mr. Vaill: Counsel for claimant offers in evidence the four drawings referred to by Mr. Waterman in his last answer, as Waterman Drawings Nos. 5, 6, 7 and 8, and requests that the same be marked respectively "Claimant's Exhibits Nos. 322, 323, 324 and 325".

(Said exhibits marked as requested.)

Q. 15. Dr. Milier, in his answer to Q. 17 (P. R., p. 786), compares the operation of the Fleming valve in the circuit of the patent in suit with that of the DeForest PX audion used by the Navy as illustrated in "Plaintiff's Exhibit No. 83". Will you consider this answer of Dr. Miller regarding the PX circuit, and state whether or not you agree with his conclusions, or whether he has advanced any reasons which affect your opinion on this subject, as stated in your previous deposition? (P. R., 222-3.)

A. I have considered the testimony of Dr. Miller referred to, and it does not alter, but rather confirms the opinions which I expressed in my former deposition. With Dr. Miller's technical explanation of the operation of the three-element bulb in the PX circuit, I have no quarrel; it is, as I understand it, the usual present-day method of explanation which assumes the fundamental facts of electrostatic control. I differ radically, however, as to the statements of differences and implied differences between the two-electrode and the three-electrode tubes. I also differ with his statement that his explanation "stresses the most essential features of the action of the tube in this circuit". It does not; on the contrary, he omits to point out that the fundamental action is the control of the emitted electrons by the electrostatic fields produced, which control is the same as that in the two-electrode tube of Fleming.

The PX circuit is the circuit of Fig. 1 of the Marconi patent 627,650, reproduced in my preceding answer and in Fig. 2 of my drawing 7 ("Claimant's Exhibit No. 324"). Fig. 2 of my drawing 8 ("Claimant's Exhibit No. 325") differs in that the biasing battery is substituted for the condenser in the input circuit. I will here confine my answer to the form of circuit shown in Fig. 2 of drawing 7.

As pointed out in my last answer, the operation of this arrangement is fundamentally the same as that of the two-electrode tube. It is commonly referred to at the present time as the "grid-rectification" circuit. Dr. Miller, on P. R., p. 788, admits that the action which I have indicated occurs in this PN circuit:

"Received oscillations, modulated at audio-frequency, produce rectified current pulses in the grid circuit of the tube when the amplitude of the oscillations is increasing, and thus, by reason of the grid condenser, cause the average grid potential to become more negative."

[fol. 1552] Again on page 787, he says:

"When the first of these alternations are applied to the tube they produce rectified current pulses between the grid and filament of the tube, the action being similar in this respect to that of the two-electrode rectifier".

He then explains that, owing to the presence of the condenser, in order that this action may continue, the condenser must be able to discharge through some leakage path, and that current will continue to flow in rectified pulses to compensate for this leakage.

Dr. Miller, in other words, admits that the controlling action in the three-electrode tube in this PN circuit, is exactly the electrostatic control of the Fleming two-element bulb, and he points out that the effect of this rectification is to cause the current in the plate-circuit to change, and thereby indicate in the output circuit the result of the receipt of the oscillations exactly as indicated in my drawing No. 7.

In view of this admission, the only differentiation between the two and the three-electrode tubes is that in the specific circuit shown in the Fleming patent the impressing of the oscillations and the indication of what happens, is done in one circuit. He says (P. R., p. 790):

"The two-electrode device in the Fleming circuit constitutes essentially a rectifying device in a single circuit in which the received oscillations are rectified and themselves flow through and are indicated by the indicator. On the other hand, the three-electrode device in the PN circuit as well as in the regenerative circuit constitutes essentially a different type of vacuum tube connected to two separate

circuits neither of which performs the functions of the Fleming circuit."

Unless this is a mere quibble, it is wholly untrue. I pointed out in my former deposition that in the circuit as shown in the Fleming patent, two functions are performed, one being to impress the signal on the tube, and the other being the indication of the current or current-change which results, and I pointed out that in the PN circuit there are two parts, viz., that which is commonly called the input circuit, which performs the function of impressing a signal on the tube, thereby setting up the electrostatic field, and the other, viz., the output circuit, which performs the function of indicating the result. The only basis for Dr. Miller's statement that I can conceive of is that he means that neither of the two parts into which the PN circuit is divided (as taught by Marconi), as compared to the single circuit shown in the Fleming patent, performs *both* of these functions. The input circuit, of course, does not indicate because there is no indicator in it, but if Dr. Miller means to suggest that it would not indicate if an indicator were put in it, he is wholly mistaken, and his statement is entirely incorrect. If, for example, a telephone is put into the input circuit of the PN circuit, as for example, adjacent the filament, in the same relative position as the indicator I of Fig. 1 of my drawing No. 7, the signal will be heard. Exactly the same thing is true of the modified arrangement of my drawing No. 8. From every point of view that I am able to think of, therefore, Dr. Miller's statement that neither of the two parts of the PN circuit "performs the functions of the Fleming circuit" is wholly incorrect and misleading. The fact is that because the input path of [fol. 1553] the PN circuit performs the functions of the Fleming circuit, the three-electrode tube is able, through the more effective use of the electrostatic control, to perform the additional function of influencing the flow of the battery B in the PN circuit, as though a stronger signal had been applied to the plate of that circuit.

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On P. R., p. 790, Dr. Miller says, in continuation of the last quotation:

"In the PN and regenerative circuits, while the received oscillations are increasing in amplitude, they produce

rectified current pulses which charge the condenser, which charges leak away when the received oscillations are decreasing in amplitude or are not being received."

This is a correct statement, and is an admission of the same electrostatic control action upon the electrons, as found in the two-electrode tube. His immediately following statements, however, are wholly incorrect. He says:

"Such rectification as occurs is not indicated in either circuit. Even if it were possible to observe this rectification it would not reproduce the audio-frequencies which the detector is intended to indicate."

These statements are incorrect. Dr. Miller himself, in fact, goes on to say that it is this rectification which is the cause of the variation of voltage between the grid and filament which, in turn, by the same sort of electrostatic control, causes the indicator and the plate circuit to show what has happened by responding to the signal. His statement that "even if it were possible to observe this rectification it would not reproduce the audio frequencies" is absurdly incorrect, as anyone can prove for himself who has a radio set, the wiring of which is accessible enough to permit the introduction of a telephone in the position corresponding to that of the indicator I, Fig. 1 of my "Drawing No. 7" ("Claimant's Exhibit No. 324"): that is, into the input circuit of the PN or regenerative detector. Whatever the detector is responding to, whether it be telegraph signal, speech or music, will be heard just as faithfully reproduced as though the telephone were in the output circuit. Of course, it will not be quite as loud because, as I have stated, the three-element tube has the advantage that a given signal applied to the grid is equivalent to a somewhat greater signal applied to the plate, this being one of the improvements that DeForest made when he inserted the grid. But to say that the audio frequencies which the detector is intended to indicate, cannot [fol. 1554] be observed in this circuit, is highly absurd and untrue, and merely shows that Dr. Miller never tried it, and has an incorrect understanding of the operation of the device.

None of the statements which I have made in this answer, or in my preceding answer, are in any way dependent upon the degree of exhaustion or vacuum in the tube. I have

tried the experiment with both so-called "hard" tubes and with so-called "soft" tubes, and the result is the same.

This statement of Dr. Miller's is so astonishingly in conflict with the facts as ordinarily understood, that to make perfectly sure of the matter I repeated the test within the present week, although I have done it many times before. I found the facts to be exactly as I have stated them, and entirely in conflict with Dr. Miller's statement. It is clear that Dr. Miller was simply not informed as to the facts.

The action of the three-electrode tube in the PX circuit is exactly the same sort of action as occurs in the two-electrode tube.

Q. 16. In Dr. Miller's answer to Q. 18 (P. R., p. 791), he compares the operation of the Fleming valve with the three-electrode audion when a local battery is employed. In his answer to Q. 21 (P. R., pp. 808-10), and in his answer to Q. 23 (P. R., p. 814), he comments on, and criticises your previous testimony relating to the use of a local battery with the Fleming valve and audion, and says that your testimony conveys a wrong impression in respect to such use, and that you are incorrect in stating that the Fleming valve may be used to amplify when a local battery is employed. Dr. Millikan also states that a hard Fleming valve with or without a battery is incapable of amplification (P. R., pp. 847-8). Will you please make such comment upon defendant's testimony above referred to on these subjects, as may seem necessary, and also briefly point out why users of the Fleming valve are entitled to employ a local or B battery therewith, giving your reasons for any opinion you may express?

A. Referring first to the latter part of the present question, I may say that the use of a local or B battery with detectors of many sorts, was exceedingly well known, both as a matter of disclosure in patents and publications and as a matter of actual use in connection with detectors of many different forms, long prior to either the Fleming or the DeForest patents for the two-electrode and three-electrode tubes respectively. Since Mr. Weagant, in his answer to Q. 260 (P. R., p. 1719), and to Q. 304 (P. R., p. 1766), has given references to a number of instances of this prior knowledge, I will not repeat. I have read his testimony and find that it correctly states the facts as known to me.

Dr. Miller's statement of the physical basis of his comparison is found in his answer to Q. 21 (P. R., p. 805), as follows:

"The Fleming valve as shown in the Fleming patent is identical in structure with the device shown in the Edison patent No. 307,031 of October 21, 1884, and illustrated in my Figure 1 of drawing A. Both structures consist of an incandescent filament and cold plate insulated from each other within an evacuated inclosure and both devices utilize the emission of electrons from such an incandescent filament and the flow of these electrons through the space between the filament and the plate. The shape and location of the cold plate is somewhat different in the two cases. Edison shows a plane plate located between the two legs of the filament while Fleming shows a cylindrical plate [fol. 1555] surrounding the filament. This, however, is an immaterial difference which does not change the principle of operation.

"The three-electrode vacuum tube has a third electrode called the grid which is utilized as a sensitive control member over the electron flow between the hot filament and the cold plate."

I have no quarrel with this statement, so far as it goes. It simply does not go far enough. After the Edison tube had been known for very many years, and had received a large amount of study by eminent scientists, Fleming discovered that the electrostatic forces that could be produced by high-frequency radio signals could control the electron emission from the heated filament for the purposes of radio work. This was an epoch-making discovery in the history of radio, and I agree with Dr. Miller also, that the three-electrode tube, by virtue of its grid electrode, exercises this function of electrostatic control over the electron stream in a very sensitive way.

The statements of Dr. Miller confirm me in the view which I previously expressed, that what DeForest did when he put in this third electrode, was to divide the cold element into two, so that the two functions performed in the two-element bulb can be divided, the function of impressing the signal being assigned to one cold element, while the function of indicating the result is assigned to the other. I disagree entirely with Dr. Miller's view that such a statement is misleading. I think, on the contrary, that it is Dr. Miller's

statements that are misleading. I believe that stating the matter in this way states the exact fact in readily comprehendable terms.

Dr. Miller is apparently loath to admit that the two-electrode tube of Fleming can be used in the PN circuit, viz., in the circuit of Fig. 1 of the Marconi patent 627,650, reproduced in my second preceding answer. A glance at the figure shows that it is only necessary to substitute the two-electrode tube for the device T, in order to connect it in that circuit, and it is only necessary to try it to find that it works. Mr. Weagant testifies to having so used it, and I can confirm that testimony, because, on a considerable number of occasions I have seen him use it, and have participated in the observations. I have also so used it myself hundreds of times. On many of these occasions I simply inserted this two-element tube into the socket of a DeForest PN detector box connecting both circuits to the plate element, as in the Marconi patent, and used it without other change, save, of course, of a proper adjustment of the battery. It will be seen that if the electrode of the device T in Fig. 1 of Marconi patent No. 627,650 is regarded as split into two parts, each of which has a lead wire brought out, no alternation is effected except that one of these parts receives the signal from the branch i^2, k', while the other branch containing the battery B and the indicator R connected to the other portion, indicates what happens. What is more, the device T of the Marconi patent could have its left-hand electrode split longitudinally and connected in the manner just described, and it would continue to function just as before. Furthermore, this device T of the Marconi figure being, as I understand it, a coherer, is, as Mr. Weagant has pointed out, also an amplifier. Since, however, it has no such refined and graduated electron control as that discovered by Fleming, it is not capable of corresponding usefulness.

One reason why Dr. Miller says that he does not regard it as proper to consider that the plate element of Fleming [fol. 1556] is split into two parts, is because the ordinary three-electrode tube has its additional cold element in the form of a grid. Dr. Miller should know, however, that this is not an essential. It is not the thing which DeForest did in his original three-element amplifier patent. In that patent (841,387) ("Claimant's Exhibit No. 270"), De-

Forest literally divided the cold element into two parts, to one of which he assigned the function of impressing the signal to control the electron flow, and to the other of which he assigned the function of indicating what happened and so giving the amplified signal, using a battery, of course, in this circuit, because, as Dr. Miller says, such a battery is required by the fundamental law of the conservation of energy.

Looking at Fig. 4 (part of Claimant's Ex. 270) of this patent, for example, it will be seen that the cold element is divided into two plates, D, D′, to one of which, D′, is assigned the function of impressing the signal, while to the other, D, is assigned the function of indicating what happens. It will be noted that this is the PX circuit, and that it is identical with the circuit of Fig. 1 of the Marconi patent 627,650, with one electrode of the device T split into two parts. It is, of course, immaterial which is split in the case of the device T, but it is the cold element that must be split in the case of the vacuum tube, because it is the cold element which exercises the electrostatic control of the electrons emitted by the filament. DeForest, in turn, got this split element from Fleming, as will be seen by comparing the DeForest patent 841,387, "Claimant's Exhibit No. 270", with the Fleming paper, "Claimant's Exhibit No. 269". This has been referred to and explained by Mr. Weagant in his answer to Q. 263 (P. R., p. 1723). I may say that I have a number of tubes in which the two cold elements are both plates, although not in the precise form shown in the DeForest patent, and they all perform all of the functions of the three-electrode tube in which the third electrode has the form of a grid.

The construction of the third element in the form of a grid was an improvement patented by DeForest in his patent 879,532 ("Defendant's Exhibit K").

In view of the foregoing facts, I am unable to see any possible basis for Dr. Miller's criticism. It is his statements, and not mine, that are misleading.

In connection with his answer to Q. 21, Dr. Miller, in effect, admits the substantial identity, but undertakes to distinguish by saying that the functions are different. Here, again, however, he omits to refer to the fundamental similarity of operation, and to the obvious fact that the two functions are in both cases divided so that the input func-

tion is served by one branch, and the output by the other branch, of the PX circuit.

In "Miller Drawing L" he has produced as Figs. 3 and 4, illustrations of the two-electrode and three-electrode tube in this form of circuit. He attempts to disguise the similarity by putting in the tuning condenser in Fig. 4, and leaving it out of Fig. 3, and putting in the coil L in Fig. 3 and leaving it out of Fig. 4. Of course, neither is essential in either figure. Certainly, if the tuning condenser was used in one case, it would be in the other.

Dr. Miller then proceeds to illustrate in Fig. 3*a* a different arrangement of the elements, but, for some unexplained [fol. 1557] reason, fails to point out that an identical re-arrangement can be made for Fig. 4 of his drawing L.

I produce a drawing, marked "Waterman Drawing No. 9" (Claimant's Exhibit No. 326), in which I have made this re-arrangement.

Under the heading "2-Electrode Tube" I show the parallel arrangement of Dr. Miller's Fig. 3*a* and underneath it the ordinary diagram of the PX circuit, in which I have included a tuning condenser indicated by the arrow. Under the heading "3-Electrode Tube" I show this parallel arrangement of the circuits after the manner of Dr. Miller's Fig. 3*a*, and underneath it I show the same PX circuit in the ordinary method of illustration. It will be seen that whether illustrated in the parallel arrangement of drawing of Dr. Miller's Fig. 3*a* or in the usual form of Fig. 1 of the Marconi patent 627,650, they are identical, save that the three-electrode tube has two cold elements, while the two-electrode tube has but one.

With respect to the use of a local battery, I have no essential disagreement with Dr. Miller as to the use of a biasing battery in connection with crystal rectifiers, as found in his answer to Q. 18, beginning (P. R., p. 791), nor have I any disagreement with his statement that "the use of a battery with an amplifier is indispensible by reason of the law of conservation of energy." What I object to is again what he omits to say. Dr. Miller fails to point out that an exactly similar use of a biasing battery is made with the three-electrode tube, as I have shown in Fig. 2 of my drawing No. 8, in my second preceding answer.

Dr. Miller also omits to point out that just as the biasing battery when used with the crystal and with the Fleming

two-element bulb when employed as a simple detector, is adjusted to take advantage of a bend in the characteristic curve, so also in the three-element tube in a precisely similar manner, the biasing battery, shown at C, Fig. 2 of my "Drawing No. 8", is adjusted to take advantage of the bend in the characteristic curve of the three-element tube. Had Dr. Miller pointed out these facts, the precise parallelism between the two-electrode and the three-electrode devices in this regard also, would have been apparent.

As a matter of fact, in the practical use of the three-electrode tube, as in that of the two-electrode tube, sometimes the tube is used in the PX circuit with the condenser, and sometimes it is used with the biasing battery. The first shown in my "Drawing No. 7", Fig. 2, is commonly known in the art as grid rectification; whereas, that with the biasing battery shown in Fig. 2 of my "Drawing No. 8", is commonly known as "plate rectification." Both forms of detection are common, and both are rectification, if that term has any appropriateness in either case. Of course, when either the two-element or three-element bulb is used as an amplifier, or to generate oscillations, then, of course, energy must be supplied by a local source, and this is drawn from a local battery, whether the tube has two electrodes or three. Of course, the two-electrode tube having only two electrodes, naturally has only one battery, while the three-electrode tube, having two cold electrodes, may employ two batteries, and one of these may be utilized to take advantage of the shape of the characteristic curve only, while the other is used to furnish the energy for amplification and maintain an electron flow to its cold element. When the two-electrode tube is used to amplify or generate oscillations, one battery serves both functions.

I note that Fleming, in his Royal Society Paper of 1905, which is "Claimant's Exhibit No. 269", shows fully the [fol. 1558] effect of a battery with different filament temperatures and different values of the battery in the two-electrode tube.

Mr. Vaill: Counsel for claimant offers in evidence the drawing referred to by Mr. Waterman in his last answer as "Drawing No. 9", and requests that the same be marked "Claimant's Exhibit No. 326".

(Said exhibit marked as requested).

Q. 17. Will you briefly state what is meant when it is said that a two or three-electrode tube will "generate oscillations", and also explain the relation of the so-called negative resistance and falling or rising characteristic curves, as used in connection with the subject of such oscillations, and also please state the relation of these functions to the amplifying action of vacuum tubes, particularly regarding the regenerative circuits of "Plaintiff's Exhibit No. 114, Vacuum Tube Receiver Arranged for both Oscillations and Regeneration", considered by you in your previous deposition, and referred to by Dr. Miller in his answer to Q. 17 (P. R., p. 786).

A As Mr. Weagant has pointed out, it is a misnomer to say that a tube "oscillates", regardless of the number of electrodes. What happens is, that the whole circuit combination, whatever the circuit arrangement may be, together with the tube, generates oscillations by an amplification process. A tube alone can neither amplify, nor produce oscillations; but in connection with appropriate circuits, either the three-element tube or the two-element tube, whether the vacuum be high or low, can amplify, and can produce oscillations, in various specific ways. Both the two-electrode and the three-electrode tubes, when so amplifying or producing oscillations by means of appropriate circuits, do so in virtue of electrostatic control of the electrons, emitted by the filament, of the same sort as disclosed by Fleming, and this is true for both forms, whether the vacuum is of the so-called "soft" order, so that, according to present-day theories, ionized air or gas particles take part in the action, or whether the vacuum is high, or of the so-called "hard" order, so that all of the electron control is produced directly by circuit elements.

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Of course, the oscillating circuit, in any case, must be provided with a battery or other source of electrical energy, and what happens is that the combination of circuit and tube is able to draw energy from the battery in pulsating fashion, the energy being represented by an electric current which alternates in direction, so far as its alternating-current component is concerned. Of course, as Dr. Miller has himself several times stated, the law of conservation of [fol. 1559] energy makes a battery or other source of energy indispensable in an oscillatory arrangement, whether the

oscillator be a crystal, an arc or a two or three-electrode tube. See, for example, Dr. Miller's testimony at the top of page 768 of the Printed Record, and again at the top of page 793. In contending that the two-element bulb must not be used with a battery, Dr. Miller's position amounts merely to the contention that the two-element bulb must violate the law of the conservation of energy.

Fundamentally, all so-called oscillators are merely devices which, when associated with the appropriate circuit, enable energy to be drawn in pulses from a local source of current. The device in question may be a crystal, or an arc, or any one of a considerable variety of tubes in which the electrons emitted by a heated filament, are controlled. In general, devices which are able to produce a negative resistance effect in a circuit connected to a local source of current, can be made to "generate oscillations" in this sense which I have just defined. When such a condition exists, the phenomena that occur are, of course, exceedingly complex, but in general it may be said of all of them, that what happens is that increase of current gives rise to a decrease of the effective resistance offered to its flow. As Dr. Miller has himself said, this would naturally give rise to a further increase of current, which would go on forever, if not stopped. It is usually stopped by an increase of the losses, and as soon as the current begins to decrease, that decrease is accompanied by increase of effective resistance, and a continued decrease follows until the losses are so far reduced that the reverse effect again occurs. Thus the device draws pulses of current from the battery, the precise nature of which is determined by the circuit as a whole.

In the vacuum tube, whether of two or of three electrodes, the whole operation depends upon the control of the electron emission, through the instrumentality of one or more cold electrodes by means of suitable associated circuits, and depends upon the ability of these electrons to respond to such control at radio-frequency, as first discovered and applied by Fleming.

It is clear that in this process of generating oscillations, the tube or other device must enable the circuit to draw momentarily from the battery more energy than it is consuming. This, of course, is simply another way of saying that the circuit combination is amplifying and, in general, any device which is capable of so amplifying at radio-frequencies, is capable of oscillating. Conversely, if it is

capable of oscillating, it thereby demonstrates its ability to amplify.

In order that either tube may amplify, it is necessary to so control the electron flow that the internal resistance of the tube or, perhaps, what might better be called the effective internal resistance, varies by an amount sufficient to produce a greater current change than would result from applying the input voltage alone, directly between the plate and filament element.

In either the two-electrode or the three-electrode tube, this effect may be brought about in various ways. If, for example, a sufficiently high battery voltage be applied between the plate and the filament—whether there is, or is not, a grid element present—the remnant gas which is in the tube, seems to play a part in the operation and may act to give the tube what is called in the present question "a [fol. 1560] falling characteristic", in virtue of which the circuit behaves as though a negative resistance were introduced into it.

When the electrons are emitted by the heated filament they create around it what is sometimes referred to as a "space charge". The electrons, in other words, carry negative charges and are said to negatively charge the space they occupy. Fleming discovered—although he did not phrase it that way—that by impressing radio-frequency voltages on the space between the cold and hot elements, the "space charge" was controlled and varied to produce the effects illustrated in my several drawings 5, 6, 7 and 8. When the remnant gas which remains after evacuation is sufficiently affected by the electron flow, the impression of radio-frequency signals on the space, causes this gas to aid in the variation of the "space charge" and very great amplification may be attained thereby by either a two or a three-element tube. I have many times obtained signals in this manner with a two-element tube that were enormously louder than could be obtained with a crystal, or with a two-element or three-element tube operating with an adjustment that did not bring this element into play.

Either a two-electrode or a three-electrode tube when so amplifying, may, by suitable adjustment, be made to "generate oscillations". This means, as I have above pointed out, not that the tube generates oscillations, but that the combination of the tube and circuit is able to draw pulses of current from the local battery.

If either the two or the three-electrode tube is so circumstanced with reference to the connected circuit that the electron stream is influenced in some other way, so that sufficiently great changes in its apparent internal resistance are produced by the input circuit, then, also, we have a combination which will amplify and may produce oscillations.

Mr. Weagant has shown, and I have myself many times made the same use of the two-element bulb, that amplification and oscillation may be produced if the input circuit acts electrostatically on the space surrounding the filament, without connection to the plate. When the circuit in which the received oscillations exist is connected to the plate of the two-electrode tube, it acts electrostatically, as I have explained, to control the electron emission. This, as I have just stated, may be expressed as a control of the "space charge". If the input coil of the condenser is so placed that it acts directly on the space surrounding the filament, then the electron flow is controlled so that the battery connected to the plate furnishes current by way of the electron flow, which varies more greatly than would have been the case had the input circuit been directly connected to the plate. In other words, it behaves as though a larger signal had been applied to the plate. In other words, the tube amplifies. By impressing this amplified output upon the input in any suitable manner, the arrangement generates high-frequency currents. I have done this both with tubes having an ordinary incandescent lamp vacuum exhausted exactly in accordance with the knowledge of the art at the date of the Fleming patent, including all of the precautions mentioned by Fleming, and also with tubes exhausted with more modern means, so that an even higher vacuum was obtained, and I have done it many hundreds of times.

What DeForest did when he put in the third electrode, was to give a convenient way of applying this electrostatic [fol. 1561] control of the electron flow, and the grid structure of DeForest's second patent 879,532 is a particularly useful way of accomplishing the result.

In either the two or the three electrode device, producing oscillations in its associated circuits by the control of the "space charge" around the filament, the tube behaves as though its resistance was changed by a greater amount than would be due to the application of the input oscillations directly to the plate-element. In other words, the circuit

behaves as though a negative resistance had been inserted. Fundamentally, therefore, the operation of either of the two or three-electrode tube in all methods of functioning depends upon that control of the electron emission or, if it is preferred, variation of "space charge", which Fleming discovered and taught in his patent in suit, and while various specific theories may be applied, depending upon the circuit used, they are all fundamentally based on the same principles, viz., this electrostatic control of the electron stream.

If one studies the mathematical theories that have been evolved to express the conditions of oscillation of the three-electrode tube, for example, he will find that there is a different specific theory for each specific circuit. The same, of course, is true for the two-electrode tube; in each case the results will be expressed in terms of the electrical characteristics of the circuit and of the tube, but, so far as the tube is concerned, they all rest upon the emission of electrons and the electrostatic control of that emission. To insist on specific theories, and overlook the fundamental substratum of fact, as Dr. Miller has done, merely obscures the issues, and does not elucidate them. It is certain that neither Fleming, when he applied for his patent in suit 803,684, on April 19, 1905, nor DeForest, when he applied for his patent 879,532, on January 29, 1907, knew anything about the ability of the devices which they disclosed to produce oscillations. This was only realized subsequently.

It was shown in the case of *Marconi vs. DeForest*, before Judge Mayer, in which case I was a witness, that DeForest worked directly on the disclosures of Fleming, and he improved Fleming's device without, however, altering its fundamental mode of operation. Subsequent use of these devices has developed the fact that the same fundamental control has the added capacity of generating oscillations.

The regenerative circuit of "Plaintiff's Exhibit No. 114", referred to in the question, is a means whereby the oscillations produced in the output circuit may be impressed upon the input circuit, so that they again affect the electron stream or, as I have expressed it, are again detected, whereby further amplification is secured. The same circuit arrangement is applicable to the two-electrode tube, as shown, for example, in "Weagant Sketch No. 7", found opposite P. R., p. 1770 ("Claimant's Exhibit No. 301"). I have used this arrangement very many times, both with

two-electrode and three-electrode tubes. In the three-electrode tube the effect which the high potential end of the oscillatory circuit of "Weagant Sketch No. 7" has upon the space charge around the filament, is similarly exerted through the instrumentality of the grid-electrode.

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[fol. 1562] This regenerative circuit is simply one of a variety of circuit arrangements in which the capability of these tubes of producing the so-called negative resistance effects may be utilized to produce amplification and oscillation. Another circuit, which is referred to in the record as the ultra-audion circuit, as, for example, "Plaintiff's Exhibit No. 109, Waterman Diagram Defendant's Ultra-Audion", although specifically different in appearance, is equally a regenerative circuit. The regenerative circuit is a convenient one to employ when the tube is called upon to detect and amplify or oscillate at the same time.

Mr. Weagant has dealt at length with the matter of the falling characteristic in high vacuum two and three-electrode tubes as, for example, in his answer to Q. 50, beginning P. R., p. 1706, and as I fully agree with the statements that he has made and the authorities which he has produced and referred to, I will not repeat. The whole matter of the so-called falling characteristic or negative resistance is somewhat esoteric in nature, but it refers to the fact that under some conditions various devices, including the two and the three-electrode tubes, may so operate that when the current increases, the apparent driving voltage decreases, and *vice versa*. If the circuits are properly disposed and adjusted, either the two-electrode or the three-electrode tube will exhibit this property—in co-operation, of course, with its circuits—whether the vacuum be of the so-called "hard" or the so-called "soft" order. Both may, when exhibiting this property, show effects indicating that the residual gas is playing some part in the operation if the tubes are "soft", and both may exhibit it without any trace of residual gas action through direct electrostatic control of the electron flow by the electromotive forces developed in the circuits.

In all the various uses the tube, whether of two electrodes or three, is operating in virtue of that unilateral conductivity described by Fleming, which results from the control of the electrons, emitted by the hot filament, by electrical

potentials applied from without. Through the instrumentality of external circuits connected to one or more internal cold elements in various forms of circuit arrangements, all of the various effects which have been discussed in this record are produced.

Q. 18. Defendant's Expert, Dr. Miller, in his answer to questions 4, 9, 11, 12 and 13 (P. R., pp. 758-784), refers to the demonstration before Judge Mayer during the supplemental trial (see "Claimant's Exhibit No. 171," P. R., pp. 1543, et seq.), in the case of the present claimant against the DeForest Radio Telephone and Telegraph Company, which demonstration related to the capability of the tube of the Fleming patent in suit to oscillate, and he comments on, and criticises, some of your evidence in that case in respect to such demonstration, stating that the demonstration was a "trick", that essential facts were withheld, and that evidence was given which was not in accordance with the facts. Will you please consider the above-mentioned testimony of Dr. Miller, stating what demonstration was [fol. 1563] required by Judge Mayer, the facts of what occurred during such demonstration and whether or not the demonstration was a "trick", or whether any essential facts were withheld or any evidence given that was not in accordance with the facts. If necessary, you may refer to "Claimant's Exhibit No. 171", which contains the record of evidence before Judge Mayer during the supplemental trial in the DeForest case.

A. Dr. Miller was, of course, entirely unwarranted by the facts in using any such language as that referred to in the question. Dr. Miller was not present at the tests before Judge Mayer, and had no knowledge of the facts. Dr. Miller has, throughout his deposition, used strong adjectives and characterizations. It has seemed to me, however, that it is unnecessary to pay attention to them.

It is, of course, not true that the demonstration before Judge Mayer was a trick, or that any essential facts were withheld, or that evidence was given which was not in accordance with the facts. It is true, on the contrary, that no part of the apparatus or its connections, was concealed from view, that everything that was done was done openly in the presence of both sides, and that there were men present quite as competent to detect a trick or deception as Dr. Miller, and that they examined the apparatus with the greatest particularity and that, in fact, some of the modes

of procedure were mutually agreed upon by plaintiff's and defendant's representatives respectively. The testimony was, to the best of my knowledge and belief, in accordance with the facts as understood by the witnesses; certainly that was the fact so far as I am personally concerned.

By reference to "Claimant's Exhibit No. 171" I note that I explained to the Court that the wiring was clearly traceable upon the board containing the apparatus and that I also produced a diagram of it marked in that case as "Plaintiff's Exhibit No. 2". It is my recollection that this diagram was examined and checked by the representatives of the defendant. I note that it appears, also, that the defendant requested, and was given, permission to operate the apparatus. My recollection is that Dr. Stone, one of the representatives of the defendant, did operate it.

The circuit that was thus used was the PN circuit and the oscillating two-electrode tube was used as a transmitter to furnish a signal with an ordinary operator's telegraph key. This was received by heterodyne reception with a three-element tube.

Later, according to my recollection, other tubes were used, because it was pointed out that the first one employed had shown some evidence of ionization in the form of a "blue glow" around the edges of the plate element. In subsequent tests, a bulb or bulbs which showed no trace of "blue glow" under the most critical examination, was employed. The circuit was also changed to the form of circuit shown in the Fleming patent, first with a shunted condenser, as shown in the sketch on page 89 of "Claimant's Exhibit No. 171", and later with this condenser removed. Also, the view was expressed by someone representing defendant that the oscillations of the two-electrode tube might be due to a reflex effect of the oscillating three-electrode tube used as a receiver. It was therefore agreed between Dr. Stone and myself that a crude form of "ticker", which could be quickly improvised, should be used, and this is indicated in my sketch just referred to, by a cross at the extreme right of the diagram, immediately under the convention denoting [fol. 1564] a telephone. In all tests, after the original tube was discarded, oscillations were received by the Court listening through the telephone, and also by representatives of plaintiff and defendant.

To prove to all parties that the tube was generating the oscillations heard, the filament of the tube was disconnected

from the battery, thereby stopping the emission of electrons. When this was done, the oscillations ceased. The demonstration was as openly conducted as it could possibly have been, and I know that I personally paid particular attention to having everything that was used or done, clearly apparent to everyone. Dr. Miller's assertions and accusations are, therefore, without the slightest foundation.

* * * * * * *

On P. R., p. 760 and at other places in his deposition, Dr. Miller has characterized the two-electrode vacuum tube oscillation as demonstrated before Judge Mayer as operating in view of a falling characteristic, and as being, in fact, an arc, and has, in substance, said that I was not telling the truth when I said that "I do not call that an arc, nor do I call that anything which in any way resembles an arc."

Fundamentally, the whole matter seems to me to be immaterial, because, if a "soft" two-electrode tube when oscillating, is an arc, then, also, the "soft" three-element tube when oscillating, is an arc, and if the "hard" three-element tube when oscillating, is not an arc, then the "hard" two-electrode tube when oscillating, is not an arc, and since they both act in the same way when constructed with the same order of vacuum, and can both oscillate by either a specific mode of explanation, even if the vacuum is "soft", I am quite unable to see what difference it makes, whether one name or another is applied to them.

In point of fact, I do not find that Dr. Miller has stated any reason for asserting that the tube demonstrated before Judge Mayer was an arc other than that it had a "falling characteristic". By the same token the oscillating crystal would be an arc, which reduces Dr. Miller's criterion to an absurdity.

According to my definition, an arc is a flow of current through a gaseous path between electrodes, the gases being composed of volatilized material from one of the electrodes. On that understanding of an arc, it is, of course, nonsense to speak of either the two or the three-electrode tubes with a soft vacuum as being an arc, when oscillating in the presence of ionization effects.

If any conduction in a vacuum tube in which ionization plays a part is an arc, then we have no line of demarcation between an arc and electronic conduction, which is not an

arc. An X-ray bulb, for example, becomes an arc and in [fol. 1565] general, one never could tell whether an arc did, or did not, exist. This, again, seems to me to be nonsense. Everybody knows what an arc lamp is, and everyone is familiar at the present time, probably, with the Cooper-Hewitt green mercury arcs which are formed within a tube with a pool of mercury acting as one electrode and the vaporized mercury is the chief constituent of the arc-stream. Dr. Miller seems to admit that not all of these arcs are oscillators; in fact, he only seems to assert that the Poulsen arc is really an oscillator at high frequencies. Certainly, the two and three-electrode soft tubes do not in any way resemble anything that is customarily called an arc. While to say that anything that has a "falling characteristic" is an arc, would include crystals, which are not gaseous conductors at all, and hard vacuum three-electrode tubes (see Mr. Weagant's answer to Q. 250, P. R., pp. 1706-1710), as also arcs. As this is *reductio ad absurdum*, further comment seems to be unnecessary.

On P. R., p. 780, Dr. Miller refers to the circuits used in the test before Judge Mayer as being the circuits used with an oscillating arc. It seems sufficient in this connection to point out that they are also the circuits used with an oscillating crystal, and that, as has been shown in this case by demonstration and diagrams, there are numerous other circuits, which are not arc circuits in which the two-electrode tube will oscillate, whether the vacuum is hard or soft.

My understanding of the fact is that Judge Mayer was not interested in the theories by which oscillation of either the two-electrode or the three-electrode tube occurred. What he desired to have demonstrated was the fact. And the fact that the two-electrode tube did oscillate was so conclusively demonstrated, that nobody could deny it. It was after this demonstration of the fact that theorizing as to the causes came into prominence. My recollection is that Dr. Stone, in testifying before Judge Mayer on behalf of the defendant, stated that even the "blue glow" which existed in the first tube tested, was not an arc, but he expressed the opinion that the "blue glow" was necessary, in order that the two-electrode tube might oscillate, while it was not necessary in the three-electrode tube. It was then proven by demonstration that the "blue glow" was not necessary to the oscillation of a two-electrode tube. After this fact was conclusively demonstrated, my recol-

lection of the testimony is that all that Dr. Stone could say, in substance, was that he did not believe the two-electrode tube oscillator was very useful.

I have, many times, received signals with a two-electrode tube oscillating just as in the demonstration before Judge Mayer, without "blue glow" and with stable operation over long periods. When the two-electrode tube is oscillating as demonstrated by Mr. Weagant, by direct electrostatic action, and with a hard vacuum, it has all the stability and permanence that the hard vacuum naturally implies. If there had never been a three-electrode tube produced, I am convinced that the two-electrode tube would have been in widespread use as detector, amplifier and oscillator. We now know how to produce soft vacuum two-electrode tubes that oscillate steadily and reliably, as well as hard vacuum two-electrode tubes that have all of the stability and reliability of the best three-electrode tubes. But because the grid-electrode is so convenient a means of getting exactly the degree of electrostatic control desired, it is naturally used in preference to the less convenient two-electrode structure. Both [fol. 1566] forms, however operating, do so in virtue of that fundamental electrostatic control of electrons emitted from a hot filament which Fleming discovered and showed how to utilize. It has, of course, taken many years to unravel the intricate and mysterious actions going on within these tubes, and it is certain—in spite of the years of study by able men in great laboratories—that all is not known yet. It would also be the height of rashness to assert that all of the supposed knowledge corresponds to the true facts. We do know, however, how to use the bulbs and whether constructed in two-electrode or three-electrode form, they are enormously useful devices. It is also true that for every function and circuit of the three-electrode tube there is a corresponding function and circuit of the two-electrode tube, operating in substantially the same way, to produce the same results.

I feel certain that had Dr. Miller been more familiar with the facts, he would not have made the statements and accusations that he did, and it is certain that there was absolutely no foundation or valid excuse for them. It does not appear that he ever has had any experience with the two-electrode tube constructed for radio use. He used certain old DeForest tubes having three electrodes as two-electrode tubes, by connecting the grid and plate together, which is

a make-shift at best. Further, these old DeForest tubes were extremely poor tubes by present-day standards, or by any standards that have existed for many years. They were made with the knowledge of their day, and were very inferior. They were, as I understand it, of the same general sort that were before Judge Mayer. In that case certain three-element tubes were produced by the DeForest Company, defendant, and when first demonstrated before Judge Mayer, gave both detection and amplification effects. It appeared from the testimony that, before the end of that litigation, these tubes had ceased to act as amplifiers, although they still acted as detectors, and would repeat the received signals, but only with a "loss" and not with an amplification. I was personally present at the test when this fact was ascertained, and the testimony as to that fact was not a "quibble", and it is not true that these tubes, or either of them, was "a smashed, burned-out, or defective three-electrode tube" so far as careful visual inspection could determine. Dr. Miller's statement to this effect at the bottom of P. R., p. 813, is without foundation.

* * * * * * *

Q. 19. Major Mauborgne, as justification for his answer to Q. 17 of his deposition (P. R., pp. 1224-1230), refers to tests made by him with some three-electrodes tubes, ten of [fol. 1567] which were found in the garret of the Bureau of Standards, and another three-electrode tube which he says was a "DeForest amplifier" tube, and in all of the tests, the grids and plates were connected together, and some of which oscillated under certain conditions and some did not. Have you any criticisms or observations to make regarding those tests and the tubes used in them, as being a basis for the conclusion expressed on page 1230 of his deposition? If so, please state them.

A. The record of unsuccessful tests is never impressive. Especially is this the case when the object of the tests appears to have been merely to prove something already demonstrated before the Court, in the presence of opposing Experts and affirmatively decided by the Court. I have, so many times, used two-element tubes as oscillators, both in the manner demonstrated before Judge Mayer, and that demonstrated by Mr. Weagant in this case, that there seems to be no comment possible for me to make, except to say that there was probably something wrong, either with Major

Mauborgne's tubes, or his methods. I do not know where he found authority for his statement that what he calls the "two-electrode tube of the Fleming type" "was a gas-filled tube". Certainly he did not get it from the Fleming patent, which lays special stress on having a high vacuum; for example, at page 1, line 96, the patent states:

"As a very high vacuum should be obtained in the bulb *a* and as a considerable quantity of air is occluded in the conductors, these should be heated when the bulb is being exhausted. The filament *b* can be conveniently heated by passing a current through it, while the cylinder *c* can be heated by surrounding the bulb *a* with a resistance-coil through which a current is passed, the whole being inclosed in a box lined with asbestos or the like."

Fleming also states that the cold element may be constructed of wire, with terminals brought out, in order that a current may be passed through it to heat it while it is being evacuated. If all of Major Mauborgne's testimony is as accurate, therefore, as his reference to the Fleming type as "a gas-filled tube", he certainly is not instructive.

The tubes which I have used were exhausted in an oven and with current passing through the filament, as instructed by Fleming, the oven heated as hot as it safely could be. They had a good incandescent lamp vacuum, and were not in any sense "gas-filled". On the contrary, they were gas-emptied, in accordance with the ordinary commercial practice of exhausting incandescent lamps at the date of the Fleming patent, with the added precautions specified by Fleming.

Since it has been conclusively proven that the two-electrode tube is not "simply a detector or rectifying device", but can display all of the functions and capacities of the three-electrode tube, Major Mauborgne's assertions to the contrary, it simply shows that he was mistaken in his knowledge and understanding of the facts.

Q. 20. In connection with Dr. Miller's testimony as to the incapability of the two-electrode tube to oscillate, will you please state what the facts were in respect to Mr. Weagant's demonstrations and particularly what facts they established, in your opinion, in respect to the capacity of a so-called "hard" or "high vacuum" tube of the Fleming

patent to oscillate, to act as a radio and audio-frequency amplifier and as a transmitter of high-frequency oscillations for use in wireless communication; also please explain [fol. 1568] the circuits used in such demonstrations.

A. I was present at the tests referred to. The facts as to the tests are correctly stated by Mr. Weagant in his deposition given as the tests proceeded, and the diagrams and photographs accompanying that deposition, which are marked "Claimant's Exhibits 225, 228, 229 and 227" correctly illustrate the circuits and apparatus which he employed. These tests conclusively demonstrated the ability of the two-electrode vacuum tube to detect, amplify and produce oscillations in circuits analogous to those in which the three-electrode tube can produce the same results. I understand that the tubes employed were exhausted by modern methods and had the so-called "hard" vacuum.

Referring to "Claimant's Exhibit No. 225", which appears opposite P. R., p. 1527, the apparatus is that shown in the photograph "Complainant's Exhibit No. 228". This circuit is the same as that shown in the Fleming patent, with the exception that a battery P, is shown in the tube circuit to furnish the energy for amplification and oscillation and the usual tuning condenser is inserted in the antenna circuit at R. I note, also, that telephones are employed instead of a galvanometer, for indicating purposes.

In the first test, music was heard, the tube acting as a detector. In the second test, the tube was caused to oscillate by adjusting the coupling between the coil *m*, *k*, and usual "squeals" or "whistles" denoting the oscillating state, were heard.

The two-electrode tube arrangement was then used as a generator of oscillations which were received in another receiver.

A second apparatus was then employed, containing four two-electrode tubes, all of which I understand were "high vacuum" or "hard" tubes arranged so that the first three tubes repeated and amplified the received signals, while the last tube rendered them audible. This apparatus was so arranged that by means of switches, either all four tubes could be used or only the last one. It was found that the signal was very much louder (about one hundred times, I should judge) when the four tubes were used, than when only one was used, thus demonstrating the capacity of the

tube to respond to, and amplify, high-frequency radio signals.

The circuit arrangement and apparatus are correctly illustrated in "Claimant's Exhibits Nos. 227 and 229". The construction of the tube is correctly described in his answer to Q. 9, P. R., p. 1529.

I have made extensive use of tubes of this general sort, both with extremely complete exhaustion, *i. e.*, very hard, and also with ordinary incandescent lamp exhaustion, and have found the same inherent capacity of the tubes to detect, amplify and oscillate, but I never used as many as four tubes in succession.

As explained by Mr. Weagant, these tests were performed with the tubes located so as to be directly influenced by the electrostatic field produced by a condenser or coil and the effect of the control of the electrons emitted by the filament was to cause the output circuit to respond as though a larger signal had been impressed directly on the plate.

The tests made by Mr. Weagant demonstrate the inherent capacity for that electrostatic control which Fleming discovered in the two-electrode tube, applied for all three purposes which Dr. Miller has referred to as special utilities of the three-electrode tube. They demonstrated that what DeForest did when he inserted the grid element, [fol. 1569] was simply to provide a more convenient way of utilizing this capacity or property of the electron-stream or, as it is, perhaps, better called, "cloud" emitted by the hot filament. DeForest added nothing to this fundamental property, but he did supply a very convenient and practical way of utilizing it. Subsequent use and study has led to the development of a variety of useful circuit arrangements whereby the production of amplification and the generation of oscillations is possible, and when these methods are applied to the two-electrode tube, similar results are obtained. Mr. Weagant's tests demonstrated these facts, and show that Dr. Miller was mistaken in such of his statements as imply the contrary.

Q. 21. The opinions of the lower court and of the Court of Appeals in the suit of Marconi Wireless Telegraph Company of America vs. DeForest Radio Telephone and Telegraph Company on the Fleming patent state, in effect, that DeForest in producing the three-electrode tube or audion,

began with the heated gas theory and ended by employing a commercial vacuum, but before he finally produced the audion he learned of the Fleming invention. Will you please refer to any facts within your personal knowledge, or any patents or publications on the subject, and briefly explain them?

A. The case of Marconi *vs.* DeForest, referred to, was brought by the Marconi Company on Fleming patent No. 803,684; the defendant answered and set up a counterclaim under a number of patents to DeForest, some of which were subsequently withdrawn. The patents remaining in the counterclaim case included patents 824,637, 836,070, 841,386, 867,876, 867,877, 867,878, and 979,275.

In the testimony adduced by defendant, there was naturally a great deal relating to the history of DeForest's work, and on the basis of that history, the decision referred to in the present inquiry, was reached, as I understand the matter.

Of the several patents referred to just above, patent 979,275 issued on an original application of February 2, 1905, and, although latest in date, was earliest in date of application. Patents 867,876, 867,877 and 867,878 all issued on applications filed as divisions of the original application of February 2, 1905, just referred to. All of the devices of these four patents were avowedly heated-gas devices, and all of the figures of all of the patents are contained in the patent 979,275, which issued on the original application. There are six figures in this patent, of which Figs. 1, 3, 4 and 5 show gas flames and circuits variously arranged, while Figs. 2 and 6 show devices in which the heating is done by an electric current from the dynamo G. So far as I know, Figs. 2 and 6 are totally inoperative. In any case, all of these devices depended upon flame or other production of intensely heated gases, according to the disclosure of the specification, and the patent points out that different salts might be introduced into the flame to vary its sensitiveness.

Patent 824,637, issued on an original application filed January 18, 1906 and patent 836,070, issued on a division of said original application. During the trial of the case DeForest patent 823,402 was offered in evidence. It was filed December, 1905, and contained a reference to, and a description of, the Fleming "electric valve" which has

been fully described by J. A. Fleming in a paper published in the *Proceedings of the Royal Society of London*, March [fol. 1570] 16, 1905, to which reference may be had for a more complete description thereof than need be set forth herein."

Patent 824,637 was referred to in the Marconi *vs.* DeForest case as the "gas oven patent". Figs. 1, 2 and 3 are described as depending upon heating of the gas. The same is true of the single device of patent 836,070, which corresponds to Fig. 3 of the last-considered patent.

Patent 841,386, while showing tubes containing a heated and a cold element in an evacuated bulb, was held to be invalid, because inoperative.

It was shown to the Court by these several patents that DeForest was working on a theory that heated gas could constitute a radio detector, and that he did not come to a vacuum tube type of device until after he had full knowledge of Fleming's device, the construction and operation of which was completely disclosed in the Fleming paper which DeForest mentioned in his patent 823,402, filed December, 1905, and issued June 12, 1906, both dates being earlier than the application for any patent by DeForest showing the two-electrode tube as an evacuated bulb, or any patent showing a three-electrode tube as an evacuated bulb. DeForest's earliest patent for a three-electrode tube was 841,387, filed October 25, 1906, and his next three-electrode tube patent, wherein the grid-type of tube was disclosed, was patent 879,532, which was applied for January 29, 1907.

It was on the basis of this evidence, as well as some other evidence, including some correspondence of DeForest, produced by defendant, that the Court found that DeForest began with the heated gas theory and ended by employing a commercial vacuum.

* * * * * * *

The evidence in that case showed that the DeForest three-electrode bulbs, which were offered in evidence by plaintiff and defendant respectively, had a commercial vacuum corresponding to the ordinary commercial electric lamp vacuum of Fleming's day. This was stated by plaintiff's witnesses and not denied.

One of the evidences of this fact was the instruction sheet which came with the DeForest apparatus and which

was offered in evidence in that case. I find the same or a similar instruction sheet in evidence in this case as "Plaintiff's Exhibit No. 108" (printed opposite P. R., p. 224). This instruction sheet contains a diagram of the tube in the P N circuit, and I find thereon the following statement:

"If too much 'Battery B' voltage be applied a blue glow will appear in the bulb, or tube, or may be seen merely around the edges of the 'plate' or wing. When this blue [fol. 1571] glow fills the bulb it is usually not very sensitive, and can be removed by simply reducing the B voltage by the 4-point switch on front of box."

This statement shows that the tubes referred to had the so-called soft vacuum and not the extremely high vacuum that is employed in some present-day tubes.

Comparison of these tubes, which I personally made with commercial two-electrode tubes which were in commercial radio receiving apparatus, showed that the vacuum was substantially the same.

Mr. Vaill: Counsel for claimant offers in evidence the binder containing DeForest patents Nos. 824,637, 836,070, 841,386, 867,876, 867,877, 867,878 and 979,275, and requests that the same be marked "Claimant's Exhibit No. 327".

(Said exhibit marked as requested.)

Q. 22. In answer to Q. 16 of his deposition regarding the gaseous two-electrode tubes (P. R., p. 785), Dr. Miller stated as follows:

"In the present state of the radio art, it is impossible to construct tubes to have gas present to a definite degree as to have such tubes exhibit characteristic, depending upon the gas, with any certainty whatsoever."

Will you please state whether or not you agree with Dr. Miller as to this portion of his answer?

A. I do not. I know of no authority for any such statement. It is well known to users of radio tubes that tubes having a relatively soft vacuum have been constantly on the market, and as to the tube known as UV 200, with which I am very familiar, I know that these tubes are consistent performers and oscillate readily, either with ionization

effects or with direct electrostatic control. I do not think that Dr. Miller's statement is true.

As I have pointed out, the production of the three-electrode tube followed very soon, as the result of Fleming's disclosure, and the two-electrode tube also is capable of oscillating reliably and satisfactorily without any gas phenomena whatever. For these reasons it has never been necessary for manufacturers to produce tubes oscillating in virtue of gas ionization. It is, therefore, doubtful whether anyone could say with positiveness what could be accomplished in large-scale production, where oscillation was the sole criterion of definiteness.

I do know from personal experience, that the UV 200 tubes were reliable tubes and that they oscillated readily and stably in the ways in which they were intended to function, and also oscillated readily and with good, although not equal, stability with the grids and plates connected together to act as two-element tubes. Of course, this is not an ideal or advantageous way to produce a two-element tube, and it has not operated to change the two-element tube performance on this basis. I understand, however, that Dr. Miller has had no other experience, and that he has only used the old DeForest tubes, which were notoriously erratic, whether used as two-element tubes or as three-element tubes. The production of tubes which are definite in their characteristics in the sense that they are all alike, is a distinctly modern achievement, and even at the present time large numbers have to be rejected on final test. Dr. Miller has produced no authority for his statement, and my personal belief is that there is none.

[fol. 1572] Q. 23. Referring to Dr. Miller's answers to Qs. 19 and 22 of his deposition (P. R., pp. 793 and 811), which relate to "Plaintiff's Exhibits Nos. 85 and 110"—Audion Amplifiers Used by the Navy—will you please refer again to these exhibits which you considered in your direct testimony (P. R., pp. 225-227), and state, according to your knowledge of the matter, to what extent the arrangements of vacuum tubes shown in those exhibits comply with the substance of the Fleming invention as recited in claims 1 and 37 of the Fleming patent in suit, stating your reasons, if any, why you do not agree with Dr. Miller's answers?

A. As I understand the matter inquired about, the question is, whether a combination of tubes used to produce an

audible radio signal by several times amplifying the signal is a detector. Looking at "Plaintiff's Exhibit No. 110", a signal received on the antenna A is transferred to the associated circuits in the form of radio-frequency oscillations. In order that these may affect an indicating instrument, it is necessary that their frequency should be changed to an audio-frequency. In "Plaintiff's Exhibit 110" this is done by the first Tube T^1. If the signal is then readable, that is, audible, in the telephones, no further tubes are necessary. If, however, it is not readable, then the tube T^2 may be used. If it is still not readable, the tube T^3 may be employed.

As I understand the matter, Judge Mayer held that under these circumstances all of the tubes together constituted the detector. This is the usual use for such an amplifier. In "Plaintiff's Exhibit 85" no frequency-converting tube—which I understand Dr. Miller has referred to as a detector in the limited sense—is shown. Of course, there would be no difficulty in so using the first or either of the tubes of this arrangement, that it would act as the frequency-converter or detector in the limited sense. It may also be used in association with some other frequency-changing device, in which case it becomes a part of the detector in the broad sense of the term. So far as the condenser *h* is concerned, Dr. Miller is, of course, correct in saying that that condenser would not be employed if the terminals X Y were to be connected in the plate-circuit of a preceding tube of either two or three electrodes, if used with a local battery. For use with such a battery, the condenser *h* would, of course, be short-circuited by the user, that is, cut out of circuit in accordance with the ordinary knowledge of the art.

Q. 24. Will you please refer to Dr. Miller's answer Q. 7 (P. R., p. 768), and Dr. Millikan's answer to Q. 17 (P. R., p. 847), to the effect that the operation of the two or three electrode bulbs is not changed when operated at high frequency, or by radio currents, as distinguished from their operation at low-frequency or by audio-frequency currents, and state your understanding of the matter?

A. That, of course, was one of Fleming's great discoveries. Fleming discovered that the extremely feeble and enormously high frequency electrostatic fields produced by radio-frequency currents, were able to accurately and exactly control the electrons emitted by the hot filament. I agree that up to the highest frequencies used in radio, this is true. Of course, it is not true of devices in general. There are, for

example, efficient low-frequency rectifiers that are quite useless at high frequencies.

All that Dr. Miller does is to give what at present is as- [fol. 1573] sumed to be the explanation of the fact, while, so far as I remember, Dr. Millikan merely states it as a fact.

There is, of course, nothing new about that statement in this case. It was fully understood by Judge Mayer, as was also the fact that this was one of Fleming's great contributions to the knowledge of the art.

Q. 25. In view of your knowledge of the art as it existed at the date of the Fleming invention (*viz.*, November 16, 1904, the date of the application for the Fleming British patent), was it

"known to one skilled in the radio art that a rectifier would act as a detector or that anything that would rectify oscillations of low-frequency would rectify waves of the order used in radio-communication"?

In answering this question, please consider the substance of the patents and the publications referred to by Dr. Miller in his answer to Q. 28 (P. R., p. 823), and his conclusions reached as indicated on pages 826 and 827 of the Printed Record.

A. It is not a fact today, and was not, at the date of the Fleming invention, that anything that will rectify oscillations of low-frequency will rectify waves of the order used in radio-communication. Any statement to that effect would be untrue. It was known that some of the devices that were used as so-called detectors, acted as rectifiers in that anomalous use of the term "rectifier", which means merely that the device conducts in one direction better than in the other, or is conductive in one direction and not conductive in the other. At least, it was surmised that this was the fact as to some of these detectors. As Fleming himself points out in his patent 803,684 in suit, there were well-known forms of rectifiers that were useful for low-frequency currents that would not act with high-frequency currents. The same is true today. No generalization such as Dr. Miller indulges in, is true, therefore; for example, Dr. Miller says (P. R., p. 827):

"It was known to men skilled in the art at the date of Fleming's application that a rectifier would act as a detector."

This statement is quite untrue. It was not true as of Fleming's day, and it is not true today. All that can be said is that it was well known that there were rectifiers that would not act as detectors, and it was known that there were some detectors which acted as rectifiers. The state of facts in this regard has not altered.

As to Dr. Miller's statement made near the foot of P. R., p. 826 and again near the top of P. R., p. 827, that a scientist or one skilled in the art "could predict" or "would have been able to predict that the Edison device would rectify waves of the order used in radio communication" that is merely assertion, with the evidence of history completely against it. The fact is that no one did predict it prior to Fleming's discovery, and since Fleming's discovery an enormously useful art has developed as a direct result.

As I pointed out in a former answer, DeForest's work followed almost immediately after Fleming's announcement, and DeForest in his patent 823,402, filed December 1905, specifically refers to Fleming's Royal Society Paper of March, 1905. DeForest's development of the three-electrode tube was, therefore, a direct outgrowth of Fleming's discovery, and Dr. Miller's assertion as to what one skilled in [fol. 1574] the art would have been able to predict is, at the best, merely his opinion, which seems to be emphatically contradicted by the historical facts.

Neither of these conclusions of Dr. Miller's on P. R., p. 827, is supported by the references to which he refers.

I believe it to be true that about 1883 Edison discovered the so-called "Edison Effect", and it is true, as Dr. Miller states, that this discovery aroused wide-spread interest and that the literature which followed in the next few years as a result of investigations by many scientific men reached a very wide-spread audience of scientists and engineers. In short, it attracted very wide attention and was studied extensively, and in spite of the study, it was twenty years before it was discovered that it had any usefulness in radio or in connection with the study of electro-magnetic waves.

The study of these waves began, I believe, about 1887, and radio began with Marconi's work, which, if I remember rightly, was about 1895. Fleming's British patent was applied for November, 1904. Wehnelt, who was a very distinguished scientist, disclosed, and, if my memory serves me, also patented the use of a tube similar to that of Edison.

with a special type of filament, known today as the oxide-coated filament, but it did not occur to him that the electrostatic control that could be produced by radio-frequency currents would be effective to produce rectification of currents of that frequency. Since the date of the Fleming patent, however, immense numbers of tubes with these Wehnelt oxide-coated filaments have been used for radio purposes. They were so used, however, as the result of Fleming's discovery. It did not occur to Edison or to Howell, or to Preece, or to Kennelly or, indeed, to Fleming, until after he had worked for many years and published many articles, that these tubes would be useful for radio purposes.

The Edison patent to which Dr. Miller refers, showed only a suggestion by Edison of the use of a tube applied to a direct-current power-circuit for the purpose of voltage regulation, a use which, so far as I can ascertain, has never proved practical.

Dr. Miller refers to the French patent to Valbreuze, No. 328,687, and says:

"In fact, Figure 2 of the Valbreuze patent is identical with Figure 2 of the Fleming patent."

It is not. Valbreuze shows vacuum tubes of the Geissler tube type, without heated or heatable filaments, and he shows a polarized relay useful only for low-frequencies. The Valbreuze tubes contain two electrodes, one of which is a sphere and the other of which is a point. Such tubes are essentially useless.

The testimony of E. H. Armstrong, referred to by Dr. Miller, was given in the case before Judge Mayer, many years after Fleming's discovery, and in the light of that discovery.

The several scientific publications of Townsend, C. T. R. Wilson, J. J. Thompson and H. A. Wilson, referred to on P. R., p. 825, are scientific publications having to do with the ratio of the charge of an electron to its mass, and have nothing whatever to do with the subject of two-element or three-element tubes for radio use. They relate to pure scientific investigation.

[fol. 1575] In point of fact, it was shown to Judge Mayer by similar scientific literature, published between the date of the Fleming patent and the date of the hearing before

Judge Mayer, that during the interval scientists had held various opinions as to the action of a heated filament and that there was no unanimity of opinion.

The fact, of course, is that today scientists are unlearning much of the supposed positive knowledge of a few years ago. The results of investigations published within the last six or eight weeks have, in fact, proved conclusively that much of the supposed knowledge regarding electrons was incorrect. What a scientist, from supposed knowledge, would be able to predict, would be quite as likely to be wrong as right. It is the discovery of the facts that advances the practical arts, and it was Fleming who made the discovery of the possibility of the electrostatic control of whatever it is that is emitted by the hot filament in the electron tube, and on this discovery the radio art, so far as it rests on vacuum tubes, is founded.

The publications to which Dr. Miller refers have nothing whatever to do with the subject. No one of them relates to the subject, nor do they all together constitute a disclosure of that which was disclosed by Fleming in his patent No. 803,684 in suit.

* * * * * * *

[fol. 1576] Q. 26. From your knowledge of the suit entitled Marconi Wireless Telegraph Company of America *vs.* DeForest Radio Telephone and Telegraph Company on Fleming patent 803,684 in the Southern District of New York and in the Court of Appeals for the Second Circuit, will you please state to what extent the patents and publications referred to in your last answer were before the Court in the trial of the DeForest case.

A. All of the subject-matter referred to by Dr. Miller in his answer to Q. 28, beginning at P. R., p. 823, which was inquired about in the last question, was before the Court. All of the publications referring to the "Edison effect", including the Edison patent, and the articles by Houston, Preece, Kennelly and Fleming, were before the Court in that case; also the Wehnelt publication and certain other Wehnelt patents and publications in addition. Also the work of Richardson, J. J. Thompson was presented in the testimony and the substance of all of the several publications by Richardson, Thompson, Townsend and Wilson, were before the Court. Most of these latter referred to by Dr. Miller, have to do with the ratio of

charge to the mass of the electrons and, obviously, have nothing to do with this case. They merely are referred to by Dr. Miller as authority for his statement that "During the decade prior to this date [November 16th, 1904], there was also taking place a tremendous revolution in the knowledge of mankind with respect to physics and the laws of physics." As a result of this "tremendous revolution" Dr. Miller now thinks that it would have been obvious to anyone, in view of the knowledge of the radio of the charge to the mass of the electrons that the "Edison effect" would enable the Edison bulb to act as a rectifier at all frequencies. In my opinion, nothing could be more absurd. The very publications which he cited show contradictory phenomena and, in my judgment, make it perfectly clear that no such conclusion would have been likely to have been drawn and historically, the fact is, as I have pointed out, that it was not drawn, nor was it known to anyone until it was discovered by Fleming.

The court was fully advised, also, as to the Valbreuze patent and the opinions of Armstrong, who, in fact, testified before Judge Mayer in the case inquired about. In short, Judge Mayer was fully informed as to the work of Edison and the very extensive scientific discussion and controversy to which it gave rise. He was fully informed as to the work of Wehnelt and Valbreuze and the value of the charge of the electron and of the mass of the electron were stated to him, not only as of the knowledge at a date prior to Fleming, but as to the knowledge at the date of the trial. I believe that there is no knowledge on the subject-matter of my last answer which is before this Court, which was not also before Judge Mayer in the case inquired about.

Q. 27. I invite your attention to Dr. Miller's answer to Q. 5. of his *disposition* (P. R., p. 763), and Dr. Millikan's answers to Qs. 21-23 (P. R., p. 850), and will ask you to state to what extent you agree or disagree with said answers, and to what extent, if any, the Hewitt patents Nos. 781,001 and 781,002 have a bearing upon the Fleming patent [fol. 1577] in suit. Also please state to what extent, if any, the decisions of the District Court and the Circuit Court of Appeals for the Second Circuit in the case of Hewitt *vs.* American Telephone & Telegraph Co. affect your opinion in respect to these Hewitt patents as bearing upon the Fleming patent in suit.

A. In my opinion, the Hewitt patents referred to, have no bearing whatever upon the Fleming disclosure of the Fleming patent in suit, either as shown in the drawings or referred to in the specification or claims. The decisions of the District and Circuit Courts also, in my opinion, do not influence, and have no bearing upon, this conclusion.

The Hewitt device comprises a tube having two electrodes, one of which is a pool of mercury and the other of which is a solid metal electrode. To start such tube in operation it requires some special means, or else an extremely high electrical voltage. In operation, the metal of the negative electrode is vaporized, and this metal vapor constitutes the current-path. In other words, this tube differs from an ordinary arc in that one of the electrodes is a liquid metal, viz., mercury, and the electrodes are enclosed in a tube.

These Hewitt patents do not show any device having an incandescent filament and a cold electrode enclosed in an evacuated bulb, nor do they disclose any device for operation with radio-frequency currents. It follows, of course, that they in no way disclose the control of the flow of electrons emitted by a heated filament in response to the electrostatic fields produced by radio-frequency or any other potentials applied between an incandescent filament and the cold electrode contained in an evacuated bulb. There is, in short, no resemblance between the disclosure of the Hewitt patents and the disclosure of the Fleming patent in suit.

When Dr. Miller states (P. R., p. 764):

"In other words, the Hewitt inventions referred to in the question are the same thing and oscillate for the same reason as the gaseous two-electrode device demonstrated before Judge Mayer in the record referred to in your Question 3."

he makes a wholly incorrect statement. How anyone with any sense of responsibility, either to the Court, or to the truth, can make such a statement, I am entirely unable to understand.

The Hewitt device is a long slender tube with a pool of mercury in one end and a solid electrode in the other. When in operation, the tube is filled with mercury vapor, the pressure is of the order of atmospheric pressure, and

the current flowing in this vapor produces an intense ghastly greenish light.

The Fleming device is an ordinary incandescent-lamp type of structure, having a filament arranged to be operated at incandescence and, in close proximity to the filament, a cold-plate electrode. The device demonstrated before Judge Mayer was of this type; it was a small incandescent lamp having in close juxtaposition to the filament a circular metallic plate with a lead-wire brought out through the glass. The air originally in the tube had been exhausted to an ordinary incandescent-lamp vacuum and when in operation, such a vacuum continued to exist. The current was not carried by metallic vapor, but, on the contrary, electrons were emitted and these electrons under the influence of the applied battery and of the oscillations pro- [fol. 1578] duced, were suitably controlled to pass in varying impulsive flow from the filament to the cold plate.

It is therefore evident that there is no part of Dr. Miller's above-quoted statement which is true; the devices are not the same and the current is not conveyed in the same manner.

Dr. Miller has persistently called the two-electrode tube, when oscillating as demonstrated before Judge Mayer, an arc. Dr. Millikan evidently recognized the distortion of meaning involved in such a terminology, for he says (P. R., p. 849) merely that he proposes to call it "an arc". He says:

"Any space between two electrodes in which it is possible to obtain a large amount of ionization of gases or vapors may under suitable conditions exhibit the peculiar property of having what is often termed a negative resistance. * * * If we insert such *a space, which for brevity I will henceforth term an arc*, in series with a circuit otherwise containing a battery and a resistance." * * * (Italics mine.)

In other words, the sole basis for such a terminology and for Dr. Miller's sweepingly untrue statement is that they are able to detect a property known as negative resistance, which they say is also characteristic of an arc. It would be as logical to say that a man and a dog are identical and operate in the same way, because both consume animal food.

I have already pointed out that the property of developing negative resistance is equally possessed by the three-element tube. Of course, neither tube alone develops such a resistance, but each develops it in appropriate circuits, under appropriate operating conditions, and by virtue of phenomena which, as I have already pointed out in previous answers, are fundamentally the same.

On P. R., p. 850, Dr. Millikan says:

"A three-electrode tube such as is used in the modern tube amplifier does not have a negative resistance at all. It amplifies as I have said above, by the relay principle, that is by tapping energy of an entirely new circuit" * * *

Does Dr. Millikan here mean to imply that a two-electrode tube, when amplifying (or, of course, when oscillating, which involves amplifying), does so without "tapping" an external source of energy? Such a statement would be a most amazing one; it would, in fact, be perpetual motion. I do not know what the statement was intended to imply, if it was *not* such a contrast.

It is, of course, true that arcs, including the Hewitt patent arcs, can amplify. It is true that two-electrode and three-electrode tubes can amplify. And as a result of their amplifying power they can produce oscillations in suitable circuits. This, however, does not make a two-electrode or a three-electrode tube out of a Hewitt mercury arc, nor does it make the two classes of devices "the same thing", as Dr. Miller states. Whether such a device as shown in the Hewitt patents can oscillate at radio-frequencies, I do not know with certainty, but my understanding is that it cannot, and I do not find any assertion by Dr. Miller that it can. If it cannot, as I believe to be the case, then it is most certainly untrue to say that they are "the same thing", and also untrue to say that they "oscillate for the [fol. 1579] same reason", unless that statement is accompanied by highly essential qualifications—nowhere hinted at, much less stated, by Dr. Miller.

I have, in a preceding answer, pointed out that a two-electrode or a three-electrode tube may operate in virtue of an electrostatic control influenced by gaseous ionization or, in other words, in virtue of a falling characteristic. This oscillation, however, occurs by virtue of the electrostatic control of electrons emitted by the heated filament in the highly-evacuated tube. Such is *not* the operation of

the Hewitt devices; these devices conduct electricity and operate in virtue of the conductivity of metallic, *i. e.*, mercury vapor given off by the liquid mercury-electrode of the arc.

I am unable to see that the decisions of Judge Mayer, or of the Court of Appeals, have any bearing whatever upon this matter. As I understand those decisions, the issues in that case were not the same nor similar to the issues in this case. In that case the question was, whether a licensee under the Hewitt patents, who had also used three-electrode tube amplifiers, should pay a license to the owners of the Hewitt patents, because of such use. The Court held that because of the nature of the disclosure and claims of the Hewitt patents, the licensee was not obligated to so pay. The statements of the Court, made after the hearing on that issue, would seem to me to have no bearing upon any issue in this case. Judge Mayer, in his opinion, held that the defendant's three-electrode tube in that case amplified, not because of any characteristic of the conducting space, as in the Hewitt inventions, but because of the use of the DeForest grid. Judge Mayer also held in the case of Marconi *vs.* DeForest, that three-electrode tubes having the DeForest grid, operated in virtue of the Fleming electrostatic control of electrons emitted by a hot filament in an evacuated bulb and constituted infringements of the Fleming patent here in suit. There is no inconsistency between these decisions. On the contrary, they are entirely consistent, whether the tubes are considered with reference to their so-called detecting, amplifying or oscillating functions.

The tubes demonstrated before Judge Mayer were normal examples of the Fleming two-element tube, constructed without any reference to their use, before Judge Mayer. They were, in fact, constructed before there was any such litigation pending; they were constructed for ordinary radio detecting use. They were demonstrated openly in the presence of highly skilled scientific men and of counsel for both sides. Judge Mayer had all of the information possessed by any of the witnesses, and nothing could be further from the truth than the implication implied by the testimony, that Judge Mayer was not fully informed.

Q. 28. Referring further to Mr. Loftin's answer to Q. 31 of his deposition (P. R., p. 1113), which you considered

in answer to Q. 9 of your present deposition, I will ask your attention to "Plaintiff's Exhibit No. 119", particularly to pages 27-33 thereof, inclusive, and to state in what respects this publication of the Signal Corps of the United States Army tends to confirm your previous opinion as to the similarity between open-gap transmitters and quenched-gap transmitters, and also your opinion as concerns the question of resonance of such transmitters.

A. "Plaintiff's Exhibit No. 119" confirms me in the views that I have expressed. On page 27, Paragraph 60, it is stated:

"60. To produce a nearly pure wave, it has been found [fol. 1580] necessary to add another circuit in a radio transmitter to the one shown in figure 11. This is shown in figure 15, which is a typical diagram of a transmitter. It shows three distinct circuits which have been divided by broken lines in the diagram. Circuit A is the power circuit. Circuit B is the spark-gap circuit. Circuit C is the radiating circuit."

Looking at figure 11 referred to, it is found to be the circuit of the Marconi re-issue patent and has a radiating circuit comprising an antenna containing a spark gap and a power-supply circuit or induction coil. Referring to figure 15, which is said to be a typical diagram of a transmitter, it will be seen that the spark-gap has been removed from the antenna and placed in a local circuit containing a condenser, and that this circuit is associated with the antenna by means of an oscillation transformer. The circuit of figure 15 is, therefore, the circuit of the Marconi tuning patent No. 763,772. In other words, in April, 1921, the date of "Plaintiff's Exhibit No. 119", the circuit of this Marconi patent was still the "typical diagram of a transmitter".

On page 27, Paragraph 62, it is stated:

"62. A study of the action of the circuits in figure 15 shows more clearly the function of each. A *special* spark gap is supposed to be used in circuit B. Circuit A furnishes the power to circuit B, which acts as a trigger circuit. It stores up the energy until the spark gap breaks down and this breaking down allows the oscillations to occur. These oscillations are transferred to circuit C where they are radiated. Figure 16 shows this."

It will be seen that this is the same explanation as that given in the Marconi patent; in looking at figure 16, it is seen that the figure is essentially the same as that which I have given to illustrate the operation of the transmitter of the Marconi patent, and the same as that which Mr. Loftin has given as illustrating the operation of the so-called quenched-gap transmitter.

On page 28, Paragraph 64 points out that when the energy of the reservoir-circuit has once been transferred to the antenna circuit, it should not be re-transferred back to the reservoir circuit, because if this happens, a great deal of the energy is wasted.

Paragraph 65 then states as follows:

"65. There are two types of spark gaps in common use which prevent this retransfer of energy. Energy cannot get back in the B circuit if that circuit is broken immediately after it has transferred its energy to the antenna circuit. In other words, no oscillations can be set up in a broken circuit. Both types of spark-gap work on this principle."

This statement corresponds exactly with the testimony which I have given on this subject, and describes the operation which I understand is characteristic of the transmitter of the Marconi patent, when properly operating; that is, when operating to the best advantage.

Paragraphs 66 and 67 then describe by reference to figure 17, a multiple spark-gap similar to that shown in the Seibt patent and referred to as a quenched-gap, and an open gap of the rotary type. As stated in Paragraph 65, both of these gaps give the operation characteristic of the Marconi patent, illustrated in figure 16.

[fol. 1581] Beginning at page 31, Paragraph 75, the consideration of the tuning of the circuits to resonance is begun, and in Paragraph 78 it is stated:

"78. So in circuits that are coupled together. If they are not in resonance the effect of one on the other is very little. If the circuits are in resonance, very much energy is transferred from one to the other. So that, when circuits are coupled it is necessary to have them tuned with each other in order to transfer energy from one to the other."

Thus, this Signal Corps publication, "Plaintiff's Exhibit No. 119", states exactly what I have stated, that the reason

why it is possible to transfer a large quantity of energy from one circuit, viz., the reservoir circuit of the Marconi patent, to the other circuit, viz., the radiating circuit of the Marconi patent, in a few oscillations, as illustrated in figure 16 of this exhibit, is the reason which I have given, viz., because the circuits are in resonance with one another, and "resonance" is defined as meaning the same natural frequency or time-period.

I find, therefore, that this Signal Corps publication, being "Radio Communication Pamphlet No. 1," dated April, 1921, marked in evidence herein as "Plaintiff's Exhibit No. 119", exactly confirms the views and understanding of the operation of the transmitter of the Marconi patent which I have expressed.

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Q. 29. Testimony has been given in this suit by defendant's witness Loftin (P. R., p. 1137), concerning the open-gap transmitting set on the United States battleships connected with the British Grand Fleet in the North Sea during the World War. Will you please consider the testimony of Mr. Lehr in his answers to Qs. 3-29, inclusive (P. R., 1472-5), and "Claimant's Exhibit No. 207", mentioned therein in respect to the open-gap 126-meter set used by the United States Navy, and state how it compares in construction and mode of operation with the transmitting apparatus of the Marconi tuning patent No. 763,772 in suit, and particularly recited in the transmitter claims thereof, Nos. 1, 3, 6, 8, 11 and 12, giving your reasons for any opinion you may express? In this connection you might also consider the testimony of Mr. G. H. Clark concerning the same sets, given on pages 1472-5 of the Printed Record.

[fol. 1582] A. I have read the testimony referred to in the question and examined the diagrams. I produce a simplified diagram illustrating the transmitter referred to. In this diagram (Claimant's Ex. 328), I have indicated the various parts by the same legends as found on Mr. Lehr's diagram, "Claimant's Exhibit No. 207", using conventional indications for the coils and condensers and their variability by an arrow drawn across them.

This is an open-gap wireless transmitter having an open antenna circuit, which is a good radiator; this comprises the elevated conductor marked "Ant.", a coil marked "Loading Coil", a variable condenser marked "Coupling",

a measuring instrument marked "H. A." and an earth connection indicated by the usual symbol. There is a closed circuit, which is, therefore, a persistent oscillator, which comprises two condensers C, C, and two inductances P, P, the condenser marked "Coupling" and a spherical terminal spark-gap marked "Gap", all in series. The power is supplied by ~~a power~~ transformer marked "Trans.", the energy for which is derived from an alternating-current generator G, under control of a key marked "Main Key." The latter is a magnetically operated key worked by a hand-key. These details being shown in "Claimant's Exhibit No. 207", I have not reproduced them.

Specifically this differs from the literal showing of the Marconi tuning patent 763,772, in that the oscillation transformer *d, d'*, is replaced by a coupling condenser marked "Coupling". In other words, instead of the inductance in the closed circuit being made the coupling means, the capacity of the local circuit is made the coupling means. The local or closed circuit of this 126-meter transmitter has inductances marked P, which correspond in the make-up of the oscillatory circuit to the inductance *d* of Fig. 1 of the patent, while the condenser *e* of the latter is represented by the three condensers: C, "Coupling" and C, in series. It is quite common to subdivide condensers in this way, as it makes them less likely to be broken down by the voltages employed. One of these condensers, viz., that marked "Coupling" is used to couple the closed primary circuit to the radiating secondary circuit, the arrangement being known as "capacitative coupling" or "capacity coupling". The degree of coupling is determined in the usual manner, being the ratio of the mutual reactance to the square root of the product of the individual reactances. The same formula applies to the illustration of Fig. 1 of the patent and to the 126-meter transmitter in question. Where the circuits are inductively coupled, as in the patent, it is, of course, the inductive reactances that are considered in applying the formula; whereas, in a capacitative coupling it is the capacity reactances that are used in applying the formula.

Having reference to the descriptive matter of claim 1, there is, in this 126-meter transmitter, a signaling instrument which comprises an inductance coil, being the transformer, the secondary circuit of which includes a condenser. It is this condenser that is represented by the condensers

C, C, and the coupling condenser which, together, are the equivalent for the purposes of the primary circuit of the single condenser *d* of Fig. 1 and similarly serve to store the charge of energy supplied by the transformer. This condenser combination is arranged to discharge through the spark-gap which is specifically like that shown in the Marconi patent, according to the testimony of Mr. Lehr, being a so-called spherical gap. Since, according to the [fol. 1583] testimony of Mr. Lehr, the two circuits are separately put in resonance by having their electrical time periods made alike, the discharge of this spark-gap automatically causes oscillations of the desired frequency. There is also an open circuit, which is electrically connected with the oscillation-producer, which circuit includes the variable inductance marked "Loading Coil". The result is that upon the discharge of the spark-gap, oscillations are produced in the primary or closed circuit, the energy of which is transferred by resonance to the open or secondary circuit. The operation is, therefore, that which is set forth in the Marconi patent, and the arrangement is substantially that described by the language of claim 1. The recitation of claim 3 includes a generator for supplying the inductance coil and means for varying the primary circuit of the inductance coil, which is the operating key in this instance. In other respects the claim is similar to claim 1 and, hence, claim 3 is substantially descriptive of this 126-meter transmitter.

With respect to claims 6, 8, 11 and 12, I note that these claims refer to a "transformer whose secondary is connected to an open circuit". In the arrangement of this 126-meter transmitter the coupling is effected by the condenser arrangement which I have described, which is the operative equivalent of the particular form of oscillation transformer shown in the patent, and diagrammatically indicated in Fig. 1. Such an arrangement is, in fact, sometimes called a condenser transformer. I note that claims 11 and 12 refer to an "oscillation transformer" and "the secondary coil of the oscillation transformer". Of course, this 126-meter transmitter does not have an oscillation transformer which has "a secondary coil", although it has, as just stated, its operative equivalent. The application of claims 11 and 12 to this transmitter therefore, depends upon the interpretation which the Court gives to these claims. Claims 6 and 8 call for "a transformer whose secondary is connected to an

open-circuit including a radiating conductor at one end and capacity at the other end, and whose primary is connected to a condenser circuit discharging through a means which automatically causes oscillations of the desired frequency," this language is not, in terms, restricted to a transformer arrangement having coils and it would be proper usage to apply it to the condenser transformer arrangement of this 126-meter transmitter. As I have said, the operative results are the same, whether the inductance element of the primary and secondary effect the coupling, or whether it is done by the condenser element. As to claims 6 and 8, therefore, the structure of this transmitter is both equivalent to that shown and disclosed by the Marconi patent and recited in claims 6 and 8, when read literally on the structure shown in Fig. 1, and it is also properly within the language from a technical standpoint. I, of course, have no opinion as to any legal aspect that the question may have.

Mr. Vaill: Counsel for claimant offers in evidence the sketch produced by Mr. Waterman in answer to the last question, and requests that the same be marked "Claimant's Exhibit No. 328."

(The Notary marks said exhibit as requested.)

[fol. 1584] Cross-examination.

By Mr. Edwards:

XQ. 30. Is it your opinion today that if the bulbs demonstrated before Judge Mayer had been bulbs having a high vacuum, that is to say, of the order of ten to the minus six power millimeters that, using the bulbs in the circuits which were used before Judge Mayer that the oscillations which were there demonstrated would have occurred?

A. I am not able to answer as to the particular order of vacuum. I assume that any change in the bulb would very likely have required a re-adjustment of the circuit constants or battery, and speaking with my present-day information on the subject, I am of the opinion that if the vacuum were high enough, the oscillation with that circuit would not occur.

XQ. 31. Do you admit that, in order to attain the oscillations demonstrated before Judge Mayer in the circuits there used, it was necessary that the tube contain some residual gas?

A. Subsequent study of the matter indicates that the electrostatic control of the electron stream, in virtue of which the oscillations took place, was one in which the residual gas played a part, although there was no visible evidence of any such effect. Of course, it is not necessary that the residual gas should play a part in order that oscillations might have been obtained with the tubes used. Of course, there is always residual gas in all tubes, which may, or may not, play an important part in the operation.

XQ. 32. Do you admit that, using the circuits which were used in the demonstration before Judge Mayer, it was necessary, in order to obtain the oscillations, that the tube contain residual gas—at least, to such a degree as to exercise the electrostatic control to which you refer?

A. No, I do not say that it is necessary; what I said was I believe that the residual gas did, as a matter of fact, play a part in the operation.

XQ. 33. Do you claim today that if the bulbs used before Judge Mayer had been of such a high order of vacuum as ten to the minus six power millimeters, using the circuits which you used before Judge Mayer, that you would have succeeded in demonstrating the oscillations which were there demonstrated?

A. There would have been no difficulty in demonstrating oscillations with tubes having any degree of vacuum, as long as it is high enough. Of course, there must be an electrostatic control and, if that electrostatic control were not exerted through the residual gas, it would have to be exerted in some other way. I do not undertake to say whether the particular degree of vacuum mentioned would, or would not, give appreciable control of space-charge by the action of the gas.

XQ. 34. Do you admit that, using the circuits used in the demonstration before Judge Mayer, it was necessary, in order to obtain the oscillations, that there be enough residual [fol. 1585] gas to exert the necessary electrostatic control? In other words, that you would not have obtained the oscillations unless the tubes contained sufficient residual gas to exert the electrostatic control?

A. No, what I said was that while the tubes had a very good order of vacuum, it is my present opinion that the residual gas probably did play a part in the electrostatic control which produced the oscillations. I do not say, nor do I believe, that oscillations could not have been produced

without that effect. It would merely have been necessary to produce the control in some other way. As, for example, by placing the tube near enough to some part of the circuit so that it experienced a sufficient electrostatic field to effect the control.

X Q. 35. Are you to be understood as saying, or intimating, that the oscillations would have been produced in the circuits which were used before Judge Mayer, without there being present in the tube sufficient residual gas to exercise the necessary electrostatic control?

A. Yes, as explained in my preceding answers.

X Q. 36. That is to say, you do assert now, that the oscillations would have been produced in the circuits which were used before Judge Mayer, even though there was not present in the tube sufficient residual gas to exercise the necessary electrostatic control?

A. Yes. It is only necessary that the tube be sufficiently near to some element to experience an electrostatic field responsive to the oscillations produced.

X Q. 37. Please note that in all of my questions, beginning with X Q. 30, I have been limiting my questions to the circuits which were used before Judge Mayer. I do not understand that any claim has been made heretofore to the effect that any of the elements in or about those circuits were sufficiently near to the tube to exercise control over the electrostatic field. With this understanding of the circuits used before Judge Mayer, will you answer my preceding questions X Qs. 35 and 36?

A. I had noted that the questions referred to the circuits used before Judge Mayer, and my answers were made with that understanding. Changing the location of the tube on the table does not alter the circuit, and I did not have in mind such a limitation to the questions. I stated in my answer to X Q. 34 that my present opinion is that it would have been necessary to place the tube near enough to some part of the circuit, so that it experienced a sufficient electrostatic field to effect the control.

X Q. 38. Then, will you agree with me to the extent that it was necessary to exercise the necessary electrostatic control, either by the presence of sufficient residual gas, or by placing the tube near enough to some part of the circuit to experience sufficient electrostatic field to effect the control? I am still assuming that the tube is a two-electrode tube.

A. Yes, with the reservation that I have no other thought in mind at the present moment. There are too many things that happen in such a tube to indulge in unrestricted generalization.

X Q. 39. Then will you admit that, so far as your present knowledge goes, the demonstration before Judge Mayer would not have resulted in the production of oscillations, unless there was present in the tubes there used, a sufficient amount of residual gas to exercise control upon the electron stream, or unless the tube had been placed near enough to some part of the circuit to experience sufficient electrostatic field to effect the control?

A. I do not get the force of the word "admit". It is my [fol. 1586] understanding of the fact, and I have expressed it as my opinion, that the two things disclosed by Fleming, viz., the emission of electrons and their control by an electrostatic field, are necessary. I state that as my opinion and understanding of the fact. I do not understand that the tube will function in any way whatever as a radio device, except as both of these effects are present. I therefore express it as my opinion that there was, in the demonstration before Judge Mayer, and must necessarily be in any such demonstration, an electrostatic control produced either with, or without, the aid of residual gas. If the residual gas effect was absent or insufficient, then there must certainly be some other way of exerting it.

X Q. 40. Then I will change my last question by substituting the word "agree" for "admit", and ask you to answer the question. As changed, the question will read as follows: "Then will you agree, that so far as your present knowledge goes, the demonstration before Judge Mayer would not have resulted in the production of oscillations, unless there was present in the tubes there used, a sufficient amount of residual gas to exercise control upon the electron stream, or unless the tube had been placed near enough to some part of the circuit to experience sufficient electrostatic field to effect the control?"

A. I agree that it was necessary to the demonstration, that the electrostatic control should be exercised either through the existence of a certain amount of residual gas, or in some other manner, such as placing the tube near enough to some part of the circuit to experience the necessary control.

X Q. 41. Is there any reason why my last question, X Q.

40, could not be answered by a simple negative or affirmative?

A. Yes, I thought so; therefore, I changed the phraseology slightly in answering.

X Q. 42. What was the difficulty with the question that prevented you from giving a simple affirmative or negative?

A. I merely thought that the phraseology of my answer more exactly expressed my meaning.

X Q. 43. Well, will you not give a simple affirmative or negative to X Q. 40?

A. Yes, if I may understand it as having the same meaning as the answer which I did give.

X Q. 44. Will you not either answer X Q. 40 with a simple Yes, or No; or, if there is any difference between the meaning of your answer and the meaning of the question, please say so.

A. As I understand X Q. 40, my answer only differs from the statement in referring to the placing of the tube near enough to some part of the circuit as illustrative. I regard it as an essentially affirmative answer without, however, being subject to the restriction that altering the position of the tube itself was the only thing that might be done.

X Q. 45. Do you know of any other way at present?

A. Yes, I have done it by simply bending the wire a little closer to the tube.

X Q. 46. Is not that merely another way of placing the tube near enough to some part of the circuit to experience the necessary control?

A. Yes, but it does not involve moving the tube, as the question might imply.

X Q. 47. It is, of course, immaterial to me which way you answer X Q. 40, but I have followed the matter through in order to demonstrate the difficulty experienced by counsel in getting from you a simple affirmative or negative as to a matter concerning which there does not appear to be any essential controversy. Therefore, now that we have succeeded in getting you to say in answer to X Q. 44, that you [fol. 1587] regard your answer as "an essentially affirmative answer", I will ask you again if you cannot answer X Q. 40 with a simple affirmative or negative without the accompaniment of conditions, or explanations involving hidden meanings, or otherwise?

A. Since my answers have clearly stated my understanding of the fact, I am compelled to regard the insistence of

the cross-examiner upon my adopting his language, as evidence that that language probably means something to counsel that it does not to me; therefore I answer it in my own words, that my meaning may be clear.

X Q. 48. Regardless of what my purpose may be, are you able to answer X Q. 40 with a simple affirmative or negative?

A. If the answers which I have given are not a satisfactory answer to the question, then I certainly am not able to answer it categorically.

X Q. 49. Please note that even your last answer is conditional. Can you not tell me whether or not you are able to answer X Q. 40 with a simple affirmative or negative?

A. For the reasons stated, I am not.

X Q. 50. That is to say, you cannot answer X Q. 40 with a simple affirmative or negative; is that so?

A. For the reasons stated, I cannot.

X Q. 51. The only reason that I can discover in your testimony, is that in X Q. 47 you seem to fear adopting my language. Will you tell me if there is anything about X Q. 40 that is not clear to you?

A. If it differs in meaning from my answer to X Q. 40, then I do not know what it means. I answered the question as I understood it, in the first instance.

X Q. 52. Do you think that X Q. 40 does mean anything more than what you have said in your answer to the question?

A. No, I regarded my answer to X Q. 40 as being a complete and satisfactory answer to the question.

X Q. 53. The only difference between your answer and my question, so far as I can discover, is that in answer to X Q. 44 you pointed out that you are not subject to the restriction that altering the position of the tube was the only thing that might be done, and yet my question was limited to your present knowledge, and I have yet to see why X Q. 40 could not be answered by a simple negative or affirmative. If there is any substantial reason, I wish you would point it out.

A. I have pointed out the only difference that I had in mind in my answer to X Q. 44.

X Q. 54. Do you regard that difference as of any consequence?

A. I have not undertaken to form any opinion on that matter. I am simply answering the questions as clearly as I am able.

X Q. 55. Do you understand that in the demonstration before Judge Mayer the tube actually was placed near enough to some part of the circuit to experience the necessary electrostatic control?

A. My recollection is that the tube stood quite apart from the other circuit elements, and I assume that it was not influenced in any high degree, by the presence of those elements through the medium of their electrostatic field.

X Q. 56. Then, assuming that to be true, does it not follow that, so far as your present knowledge goes, the demonstration before Judge Mayer would not have resulted in the production of oscillations, unless there was present in the tubes there used, a sufficient amount of residual gas to exercise control upon the electron stream?

[fol. 1588] A. As I have several times stated, my present belief is that the electrostatic control in that demonstration was probably exercised by the changing ionization of residual gas, although there were no visible evidences of it. If that is what the question means, I answer it in the affirmative.

X Q. 57. I think the question clearly means to inquire whether or not, so far as your present knowledge goes, the demonstration before Judge Mayer would not have resulted in the production of oscillations, unless there was present in the tubes there used a sufficient amount of residual gas to exercise control upon the electron stream. Is there any ambiguity about the question which renders it impossible for you to answer it with an affirmative or a negative?

A. I understand that you are asking for my opinion, and as there is no possible way of proving the matter, I can only state what I regard as the probable fact, which I have done.

X Q. 58. The question only asked you so far as your present knowledge goes, whether or not the demonstration would have resulted as set forth in the last question. Is there any ambiguity about my X Q. 57 that makes it impossible for you to answer with a simple affirmative or negative?

A. So far as I can see, the question and the answer may be taken as having the same meaning, except that my an-

swer makes it clear that I am only stating what I regard as the probable fact in that test.

X Q. 59. Yes, but I am asking you whether or not, so far as your present knowledge goes, the demonstration before Judge Mayer would not have resulted in failure to produce oscillations unless there had been present in the tubes there used, a sufficient amount of residual gas to exercise control upon the electron stream. I am not asking you to split hairs about the shade of meaning to be given to your answer, or to compare the answer with the question. I am asking you now if there is any ambiguity about the question that makes it impossible for you to answer it with a simple affirmative or negative. It would seem to me that you could answer this question with a simple Yes, or No; if you can, please do so.

A. My present opinion is that the tube used would probably not have given rise to oscillations in the circuit, if the vacuum had been of the highest possible order, without some other manner of producing the electrostatic control, as I have already stated.

X Q. 60. Will you not say whether or not there is any ambiguity in my X Q. 59, which renders it impossible for you to answer that question with a simple affirmative or negative?

A. I take it that the reference is to the meaning of X Q. 56. I did answer that question in the affirmative, without any qualification, except that I understood it as asking for my opinion, and not for an assertion of fact.

X Q. 61. Then, is it not your opinion today, so far as your present knowledge goes, that if the bulbs which were demonstrated before Judge Mayer had been of a high order of vacuum, such, for example, ten to the minus six power millimeters, using the circuits which were used before Judge Mayer, the oscillations which were demonstrated to Judge Mayer would not have occurred?

A. As I have stated, I do not have in mind at the moment any facts that enable me to pass on the specific vacuum that the question names, but I do believe that it would be possible to produce a bulb of otherwise similar construction with vacuum so high that it would not have oscillated, without some other association which would give the necessary electrostatic control.

X Q. 62. Do you agree that in the so-called demonstration [fol. 1589] made by Mr. Weagant in this case, and testified

to by him at pp. 1526 to 1531 of the Printed Record, the amplification and oscillation attained by Mr. Weagant, would not have been attained by him, unless the coils and condensers located adjacent to the tube, and employed for the purpose of controlling the electron stream, had been so employed?

A. I understand that the reception and oscillation that was demonstrated by Mr. Weagant was brought about by the combination of circuits and tubes, and that the electrostatic control which existed was influenced by the association of the elements that he pointed out, and that it would not have been attained except for that, or some equivalent association.

X Q. 63. And am I correct in understanding that, in your view, the control of the electron stream by residual gas, or by an outside coil or condenser, such as employed by Mr. Weagant, or by a grid inside the tube, as in the ordinary three-electrode tube, are all, broadly speaking, equivalents?

A. In the sense that they are all means of exemplifying the same sort of electrostatic control at radio-frequencies, as disclosed by Fleming, yes.

X Q. 64. Was this your understanding at the time of the demonstration before Judge Mayer?

A. I would find it difficult to recall precisely what my understanding was at that time. I can only say that I do not remember that my understanding at that time was any different.

X Q. 65. In your answer to Q. 17, you set forth your understanding of the function of the gas in the paragraph beginning "In either the two-electrode or three-electrode tube", and in the following paragraphs of your answer. Were the views set forth in these paragraphs known to you, or understood by you, at the time of the demonstration before Judge Mayer?

A. At the time of the demonstration before Judge Mayer, I knew that an electrostatic control was necessary, but I did not know how it was brought about. There was no visible evidence of any gas action in the tube used, except as to the first one, which was replaced, and I did not know what it was, or what the mechanism of the action was, but I did know that under the conditions of the test the oscillations were produced.

X Q. 66. At the time of the test before Judge Mayer, was it your view that the higher the vacuum, other conditions

being equal, the more favorable would be the conditions for oscillation?

A. Yes. My experience had been that, in general, the higher the vacuum, the better the tubes worked.

X Q. 67. In the light of your present knowledge, do you consider that that view was erroneous?

A. In one sense no, and in another sense yes. At the present time it is known that for certain purposes it is advantageous not to have the vacuum in a three-element tube too high and, consequently, what are called "hard" and what are called "soft" tubes are made, the difference being in the degree of the vacuum. The same thing seems to be true of the two-element tubes; that is to say, there are some usages in which the gas present, even though an extremely small percentage of one per cent. of atmospheric pressure, does play a part in the operation. Tubes of about the order of vacuum described by Fleming, would now be classed as "soft", while tubes exhausted by the improved methods which are available today, have a higher vacuum, and are called "hard". There is, of course, no sharp dividing line. I did not know these facts at the time of the demonstration before Judge Mayer.

[fol. 1590] X Q. 68. If I understand you correctly, it is your present view that the degree of vacuum is not controlling, but that the controlling essential, in order to attain oscillations, is that if the vacuum is low enough to leave residual gas, the gas will control the electron stream, and, on the other hand, if the vacuum is too high, there will be not enough gas to control the electron stream, and resort must be had to some other means, such, for example, as a grid within, or some element outside of the tube, to exert control upon the electron stream. Is that, in substance, your present-day view?

A. Yes, I think that states it, with the addition of the fact that I pointed out in my direct examination that, regardless of the precise degree of vacuum, as long as it is sufficiently good, the control by an impressed electrostatic field is effective, whether it be produced by one of the circuit elements influencing the tube, or by a grid-element within the tube.

X Q. 69. By referring to page 33 of "Claimant's Exhibit No. 171", I note that in the testimony before Judge Mayer,

when you were asked to give all of the elements necessary to make the two-electrode tube oscillate, you stated:

"The two conditions are that you should be able to heat the filament to a high temperature and that you should have a high vacuum in the bulb."

Do you not think, in the light of your present knowledge, that the reference to the "high vacuum" in this quotation, should be qualified somewhat as indicated in your last answer?

A. The next sentence on page 33 of "Claimant's Exhibit No. 171" goes on to say:

"Now, it is true that not all bulbs, as far as I know, which have a high vacuum, and which can have the filament heated to a high temperature within the limits of the apparatus, will oscillate, and what the critical condition that makes them oscillate is I do not know."

I went on to explain further what I meant. From my present point of view, with fuller knowledge, the statement of my last answer is a proper extension of the answer that I then made.

XQ. 70. In the light of your present knowledge, it would not be proper to understand the reference to a high vacuum in the testimony before Judge Mayer, just referred to, to mean that to attain oscillation with a two-electrode tube in the absence of some such means for controlling the electron stream, as a grid or outside means such, for example, as used by Mr. Weagant, one should make the vacuum as high as possible, would it?

A. If I understand the question, I should say no. I had a good deal to do with the making of many of the tubes that I used, and I did have them made with the highest vacuum possible with that apparatus, but, so far as I know, tubes that have the highest possible vacuum obtainable today, have to have an electrostatic field impressed on them from some part of the associated apparatus. Speaking only of the perfection and stability of the oscillations, it is still true that up to the highest vacuum I have any knowledge of, the higher the vacuum the better.

[fol. 1591] XQ. 71. In your answer to Q. 4 you state in effect that Mr. Loftin accuses you of suppressing the alleged fact that with a very tight coupling two oscillations would

exist. Is there any dispute between you and Mr. Loftin regarding the fact of whether or not if two tubed circuits are tightly coupled, the resultant frequency will have a double hump?

A. The subject under discussion in the testimony referred to was Mr. Loftin's Criticisms of my "Sketch No. 2", found opposite page 151 of the Printed Record. That sketch shows a coupling of about ten per cent., which is not within any ordinary usage of the term "tight coupling". In Mr. Loftin's correction, as he chooses to regard it, found in his "Sketch K", opposite P. R., p. 1057, he indicates a coupling of about three and a half per cent., which, as I pointed out, is lower than the illustrations of the Marconi patent, and would rank as an exceedingly loose coupling. As I stated in my answer referred to in the question, such a coupling as my "Sketch No. 2" shows, does not necessarily result in two frequencies; that is to say, in a double humped resonance curve.

As I pointed out in my direct examination, whether any given coupling does, or does not, result in two humps in the resonance curve of the wave transmitted, depends, primarily, upon the behavior of the spark-gap, and this is true, without regard to the particular construction of the gap.

X Q. 72. In discussing this same subject, you criticised Mr. Loftin's "Sketches G and H." Am I correct in understanding that the basis of this criticism is that you think Mr. Loftin has the erroneous impression that the current in a circuit can be going in two directions at once?

A. Yes, that is what Mr. Loftin appears to say. I refer, for example, to P. R., p. 1040, where Mr. Loftin describes his sketch G.

X Q. 73. Is it not common, in the consideration of coupled circuits of this character, to describe them as though there are as many currents as there are circuits?

A. Certainly not in the way that Mr. Loftin does it, or with the understanding that he seems to express. Further, his sketches are incorrect, if I understand them from any view whatever.

X Q. 74. Do you question the fact that in coupled circuits there will be as many frequencies as there are circuits?

A. The question is too broad to be accurate, if it can be interpreted as being a correct expression at all. Speaking of two circuits, which is what we are interested in, it is correct to say that two coupled circuits have two degrees of free-

dom. It is also correct to say that the current flowing when one such circuit is excited by the other, that the current flowing in each has not a simple sine wave-form, but is such a current as would result from the application to each circuit simultaneously, of two damped oscillation-frequencies. The current that results is a single current, and the com- [fol. 1592] ponent e. m. f.'s that produce it are damped oscillations, and in no case do they bear any resemblance to what Mr. Loftin has illustrated in his Figs. 2 and 2*a*, for example, of his sketch G.

X Q. 75. Will you point to the precise question or language on page 1040 of the Printed Record, to which you referred as indicative of Mr. Loftin having the impression that current in a circuit can be going in two directions at once?

A. I refer particularly to the last two paragraphs of that page. I may note, also that in Mr. Loftin's Figs. 2 and 2*a* of his sketch G, he shows the two currents that he describes in the next to the last paragraph on page 1040, as both of them waxing and waning. Such an assertion is an absurdity, which is only consistent with the interpretation which I placed upon it. In other words, the language and the sketch both lead to the same conclusion. If Mr. Loftin had been trying to show theoretical component currents, neither one would have waxed and waned.

X Q. 76. I do not find in the paragraph to which you refer any statement to the effect that the current in a circuit can be going in two directions at once. If you do find such a statement, will you please call my attention to it, and if you do not, please call attention to such statements, if any, that you think may justify the inference that Mr. Loftin's testimony is based upon such an impression?

A. What I said was:

"Mr. Loftin seems to have the erroneous impression that the current in a circuit can be going in two directions at once."

This, as I have pointed out in my last answer, is the necessary deduction from his showing two waxing and waning, or, as Mr. Loftin, I believe, has sometimes called them, "beating" currents in each circuit. If he had been trying to illustrate a theoretical analysis of a single waxing and waning current into two components of different frequencies, he would have shown two simple damped oscillation

trains, neither of them waxing and waning, as he illustrates. On Printed Page 1040 he says:

> "Figure 2 represents the oscillations in the lateral circuit and Figure 2*a* the oscillations in the antenna or elevated conductor circuit derived from the interlinkage of the two circuits."

Again he says:

> "Examining Figure 2 it will be seen that the oscillations start strong but soon die down to practically zero at the point I have marked *p*. Examining Figure 2*a* it will be seen that the oscillations at first are weak, and gradually build up to a maximum at the point p^1, opposite the point *p*, or point of minimum energy in the lateral circuit."

The quoted statements admit of no other interpretation, if I understand them, than that the green and red lines were intended by Mr. Loftin to represent currents flowing in these circuits, and looking at the figures it will be seen that there are many places where his two supposed currents are going in opposite directions at the same moment.

X Q. 77. Do you think it is incorrect or misleading to describe the operation of these coupled circuits as though there are, in fact, at least as many currents as there are circuits?

A. I prefer to stick to two circuits, for the reason that [fol. 1593] when we get to the consideration of three or more, we arrive at complexities not easily summarized. Understanding the question as referring to two circuits, it is incorrect to state and illustrate the matter as Mr. Loftin has done in his sketch G, for example, found on page 1040 of the Printed Record. It would not be incorrect or misleading to analyze a complex wave-form into two components, which would jointly result in the single complex current actually flowing. This, however, is not what Mr. Loftin shows, or what he describes, but, on the contrary, he does make an entirely erroneous showing, and his language leads one to infer that he is purporting to illustrate facts, which he does not.

X Q. 78. In the endeavor to meet your preference, I will re-phrase my question to read as follows:

Do you think it is incorrect or misleading to describe the operation of these two coupled circuits as though there are,

in fact, two currents, one in each circuit, and reacting upon each other?

A. No, not if the description is correct. There are, of course, two currents, one in each circuit, at the start, and while this condition exists, they do react on one another. What Mr. Loftin shows is two currents in each circuit, both showing evidence of reaction, which is impossible and incorrect, from any point of view.

X Q. 79. Do you question that there will be two currents in each circuit?

A. Yes, there will not; there will be a single current, of more or less disturbed wave-form, so long as both circuits are active and coupled. The single current of either circuit may, as a matter of analysis, be regarded as split into two components, but neither of these components will bear the slightest resemblance to either of Mr. Loftin's currents in either Fig. 2 or Fig. *2a*.

X Q. 80. In your answer to X Q. 74, you said it is correct to say that two coupled circuits have two degrees of freedom. That expression "two degrees of freedom" is taken from the Stone patent 714,756, is it not?

A. No, if the expression occurs in the Stone patent, it is simply because we were both speaking the language of the art.

X Q. 81. You testified in the case of Radio Corporation of America *vs.* Splitdorf Electrical Company in the District of New Jersey, in or about June, 1926, did you not?

A. Yes.

X Q. 82. And there you testified concerning the operation of the Stone patent 714,756, did you not?

A. I testified regarding a Stone patent, but I do not remember the number.

X Q. 83. In that case did you not testify as follows:

"By the Court:

Q. 143. This is the Stone patent?

A. Yes.

(Answer to Q. 142 continuing): This illustrates what happens when it is undertaken to transfer energy from one circuit to another after the manner illustrated in Fig. 3 of the Stone patent, and in this case the two coils used are placed close together, side by side. The upper line of curves marked A shows what was happening in the first circuit, shows the first circuit; in other words, in action, in select-

ing, prior to the closure of the second circuit. The fact that the second circuit was not closed is indicated by the horizontal line B where that horizontal line terminates the [fol. 1594] second circuit was closed and the violent and irregular oscillations which thereafter occurred in both circuits illustrate the interaction effects with which Stone's mathematical theory deals, and illustrate the truth of his statement that there will be when two circuits are so coupled, there will be in those circuits as many currents as there are circuits, at least at a minimum.

"These circuits interacting upon one another produce reaction phenomena, and these at first are violent in view of the creation there of the effects referred to by Stone at various places, as, for example, the second column of page 2, and first column of page 3, which are known as transients, and thereafter as these transients gradually die out we get a small ineffective double frequency current in each of the circuits, in other words, the effectiveness of both circuits is destroyed."

A. The question appears to correctly quote a portion of my reported testimony in that case. To understand it, it is necessary to know that I was talking about a certain oscillogram which was in evidence and that the oscillogram illustrated a single current in each circuit, of complex form, which I said might be analyzed in accordance with Stone's statement. I note that Stone, in the passage referred to, makes the statement that if a number of simple circuits are associated by conductive or inductive connections, that the oscillations, that is, the current in each circuit, will consist of the superposition of at least *n* currents; that is, as many currents as there are circuits. This is not necessarily a true statement, although it might be, under some circumstances, but the subject is a highly involved one, and there seems to be no point in going into it.

X Q. 84. The oscillogram to which you referred, was oscillogram CD120089, shown on Printed Record page 229 of the Splitdorf record, which page I now show you, was it not?

A. Yes.

X Q. 85. And the Stone patent to which reference was made, was the Stone patent No. 714,756, was it not?

A. Yes.

X Q. 86. And you also testified in the same case as follows, did you not:

"I had agreed with the statement of the Stone patent, which I understand is also accepted by Mr. Loftin, that in the arrangement of the Stone patent, when the coils of two successive circuits are associated so as to pass on received energy from one to the other, there inevitably results the development of two different frequencies, neither of which is the frequency of the received signal, and I had agreed with Mr. Loftin that they can be minimized in the way that I showed by separating the coils. * * *"

Mr. Vaill: Same objection.

[fol. 1595] A. Yes. To understand it, it must be borne in mind that I was talking about receiving circuits and their performance as illustrated by the oscillograms, and not an organization wherein one of the circuits contains a spark-gap, and I said in the same context:

"It can be shown mathematically that when two circuits are coupled, in accordance with the Stone patent, the effect of one upon the other is to always alter the effective tuning of the circuit away from the desired signal and toward the interfering signal."

It will be seen that the subject-matter under discussion was subject-matter germane to the issues of that case.

Mr. Edwards: Defendant's counsel ask the Notary to mark for identification the copy of the oscillogram CD120089 shown the witness as "Defendant's Exhibit For Identification CD120089".

(The Notary so marks said oscillogram.)

X Q. 87. And you also testified in the Splitdorf case as follows, did you not:

"This sketch, Exhibit D-11-a, made by Mr. Loftin, illustrates the same sort of performance ascribed to the Alexanderson arrangement, which I have just described with reference to Stone".

And also:

"The presence of the double hump and the spreading out of this curve is due to the fact that in the Stone arrange-

ment, we have, I believe, the question asked me to refer to Fig. 9—a circuit L, I will say, associated with a circuit L′. Both of these circuits are resonance circuits, having the inductances I L and I′₂ and the condenser C in the circuit I have called L, and the inductance I_1', L′, I_2'', and the condenser C′ in the second of these circuits, which I take for illustration. Each of those two circuits is tuned to the frequency of the desired signal, which would tend, of itself, to give a resonance curve of the form shown in A′ in the curve sheet that I have just referred to."

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A. Yes, in comparing the curves of "Plaintiff's Exhibit No. 22" in that case with "Defendant's Exhibit 11-A".

Mr. Edwards: Defendant's counsel asks the Notary to mark "Plaintiff's Exhibit No. 22" and "Defendant's Exhibit 11-A", which were referred to by the witness as "Plaintiff's Exhibit No. 22 for Identification" and "Defendant's Exhibit 11-A for Identification".

(Said Exhibits so marked by the Notary.)

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Last Question by the Notary: Do you know of any other matter relative to this claim?

Answer. I do not.

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[fol. 1596] Deposition of GEORGE H. CLARK, recalled as a witness on behalf of the claimant in rebuttal, taken at New York, N. Y., on the 26th day of March, A. D., 1928.

Direct examination.

By Mr. Cosgrove:

Q. 144. Are you the same George H. Clark who has heretofore testified herein?

A. Yes.

Q. 145. Will you briefly describe the construction of the vacuum tubes purchased and used by the United States Navy subsequent to June, 1910, and prior to the time you left the Navy in June, 1919, and which you particularly re-

ferred to in paragraph 9 of your answer to Q. 2, found on page 234 of the Printed Record herein?

A. These consisted essentially of a sealed glass tube from which the air had been withdrawn to as great an extent as possible, before sealing. Mounted in this tube was a filament, a grid, and a plate. The grid was always between the filament and the plate, and sometimes consisted of a spiral surrounding the filament, and sometimes a zig-zag form in one plane between filament and plate. In some forms of tubes, the filament was in duplicate, so that when one side burned out, the other could be substituted. The earlier forms of tube had one plate and one grid, both of these being plane surfaces. Later types re-duplicated the grid and plate on the other side of the filament, so that there were two plates and two grids, each in parallel. Still later came the form where the grid was spiraled around the filament and the plate was a cylinder with the filament at its center. This last form is the modern type, modified sometimes by the plate being rectangular or oval. The first tubes we obtained were called "audions"; later the term was changed to vacuum tubes, valves, and still later to trade names, such as the "radiotron".

Referring back to the construction, the filament, the grid, and the plates were connected by terminals leading out of one sealed end of the tube and connected on the outside of the tube to suitable terminals held in place by a base, into which latter the sealed glass tube was cemented. In the earlier types, no base was used, in the form above described, but instead the two filament terminals were brought out to a screw-type base of the form used in American incandescent lamps, while the grid and plate wires came out at the other end of the tube. These latter wires were usually covered with insulating tubing colored to indicate the terminal they represented. The grid was always green and the plate red.

Q. 146. Your attention is called to "Plaintiff's Exhibits Nos. 72 to 76", inclusive, contracts for the year 1914 between the Radio Telephone and Telegraph Company and the United States, represented by officials of the Navy Department, calling for certain radio apparatus, including bulbs. Have you any knowledge of the delivery to, and use by the United States Navy, of any of the bulbs called for in any of these contracts and, if so, please state what that

knowledge is, specifying the dates of delivery and use of the bulbs, approximately?

[fol. 1597] A. Yes, I have knowledge of both delivery and use by the Navy of these bulbs. I saw the amplifiers and bulbs in process of manufacture before they were delivered to the Navy, and these were delivered at the Navy Yard, New York, and I had a number of these amplifiers and bulbs officially transferred to the Washington Navy Yard for study, and then for use in the Service. The bulbs which I used at the Washington Yard both to study, and often to use in receiving from distant Naval stations, were of the type described in my previous answer, more especially referring to the first type therein described. I recall that this actual use of amplifiers and bulbs by me was in the early part of 1914. In August, 1914, I prepared an official bulletin for the Navy, in which these amplifiers and bulbs were described and pictured, and their use in the Service explained. The amplifiers and bulbs were delivered at the Washington Navy Yard from the New York Yard early in 1914, certainly before May.

The foregoing covers "Plaintiff's Exhibits Nos. 72 and 73".

"Plaintiff's Exhibit No. 74" covers ultra-audion receivers, detectors, and bulbs, which were delivered at the Washington Navy Yard, and which were personally passed and inspected by me. These were immediately sent out from the Washington Navy Yard by order of the Bureau for installation on ships and at shore stations, and I used them myself on various ships and at shore stations in 1914 and thereafter. The bulbs submitted on the requisition covered by "Plaintiff's Exhibit No. 74" likewise are described by my answer to Q. 145, the type of bulb being that having two grids and two plates.

"Plaintiff's Exhibit No. 75" calls for five generating sets complete, of the audion type, and for the spare bulbs for the same. These bulbs were specified as of 20-watt capacity. These sets and bulbs were delivered at the Washington Navy Yard. I inspected them and passed them, and later used at least one of them in 1915 and thereafter, for official work at the Washington Yard. Other sets and bulbs under this contract were shipped from the Washington Yard by order of the Bureau to other Navy Yards. I recall, especially, one that was shipped to Mare Island Navy Yard, and I saw that in use later at that point.

"Plaintiff's Exhibit No. 76" calls for Hudson filament 6-volt bulbs with double grid and plate. These were delivered at the Washington Navy Yard in the latter part of 1916, and were inspected and passed by me. They were subsequently issued to the Service for use in receivers on ships and at shore stations.

Referring back to "Plaintiff's Exhibit No. 74", I recall that these devices were delivered in June or July of 1914, probably the latter. I recall this because my first recommendation for purchase of these devices was made in May, 1914, and I recollect that we got them in about two months.

Referring to "Plaintiff's Exhibit No. 75" again, the delivery of this apparatus was either in the latter part of 1914, or very early in 1915. I recall, and have records of, using this apparatus very early in 1915, thereby fixing the date as at least prior to that time.

Q. 147. Do you know what is meant by "double bulbs" or bulbs with "Hudson filaments", mentioned in some of these contracts?

A. The term "double bulb" was sometimes used to express an audion with two filaments, and sometimes to mean an audion with two plates and two grids. The term was loosely used. The expression "Hudson filament" refers to [fol. 1598] a modified type of filament brought out by an inventor named Hudson, and used in some of the audions supplied to the Navy by the Radio Telephone and Telegraph Company.

Q. 148. I show you a bulb forming part of "Plaintiff's Exhibit No. 82"; if you recognize it, will you please state what it is, and how it compares with any tubes delivered to, and used by the Navy, subsequent to June, 1910, and prior to the time when you left the Navy?

A. I recognize this bulb as one of the audions of the type purchased and used by the Navy in 1914. It is the double filament type with a single grid and plate on one side of the filament.

Q. 149. Will you please answer the same question with respect to the bulb now handed to you?

A. I recognize this as an audion of the type purchased by the Navy in 1914, and used in the Service that same year and thereafter. It is of the double plate and grid type, and was designed for a filament-voltage of twelve volts.

Mr. Cosgrove: I offer in evidence the bulb last referred to by the witness and the same is marked "Claimant's Exhibit No. 329", Type of Navy Audion Bulb."

(Said exhibit so marked.)

Q. 150. I also show you two more bulbs and ask you to answer Q. 149 with respect to these bulbs.

A. The first that I pick up I recognize as a type of vacuum tube which was purchased by the Navy and of which I have personal knowledge of the use of, about the year 1916. This tube is type CG-886, the letters showing that it was made by the General Electric Company.

I similarly identify the second tube, marked CG-1162, as a type of tube made by the General Electric Company for the U. S. Navy, and which I have personal knowledge of use of by the Navy aroand the years 1916 or 1917. The specifications and official inspection for vacuum tubes being specially under my charge at that time. The delivery of the first tubes of these two types were about the same dates as those I have given for use thereof, viz., 1916, and 1916 or 1917, respectively.

Mr. Cosgrove: I offer in evidence the tubes last identified by the witness as "Claimant's Exhibits 330-A and 330-B, G. E. Navy Tubes".

(Said exhibits so marked.)

Q. 151. Please examine "Plaintiff's Exhibit No. 82, De-Forest P N Audion used by the Navy", and state what, if any, relation it bears to audion control boxes and audion detectors, referred to in paragraph 11 of your answer to Q. 2 found on P. R., page 235 herein.

A. The exhibit is identical with the type which I described in section 11 of my answer to Q. 2, where I referred to such devices under the type "P N". The modified design of audion control boxes which I testified were made up by me later, were of the same general nature and circuit as "Plaintiff's Exhibit No. 82".

Q. 152. What was done by the Navy with these control boxes, after they were obtained in May, 1914, as stated by you in paragraph 11 of your answer to Q. 2 of your previous deposition?

A. The P N detectors obtained in May, 1914, were used by the Navy in Service on ships and shore stations. I

recall, and have record of, taking one of these P N detectors [fol. 1599] from the Washington Navy Yard, where it had been in service use, to the high power station at Tuckerton, in September or October, 1914. This station had just been taken over by the United States Navy and when I arrived there and used the detector to copy signals officially, the station was in charge of a Naval Officer of the United States Navy.

Q. 153. Referring to "Plaintiff's Exhibit No. 74", and the ultra-audion receivers mentioned in your answer to Q. 146, will you please state what, if any, relation those ultra-audion receivers had to the audion receivers referred to in paragraph 6 of your answer to Q. 2, found on P. R., page 232, of your previous deposition, the circuit of which you stated was illustrated in "Plaintiff's Exhibit No. 109"?

A. The ultra-audion receivers contained under item 3 of the requisition forming "Plaintiff's Exhibit No. 74", are identical with those referred to in my answer referred to in the question. It is a fact that the detector and receiver in item 3 were specified to be in separate cabinets and, at first sight, it might appear that these receivers did not coincide with the type mentioned in my answer above referred to, in which "the vacuum tubes are located in the receiver itself". However, examination of item 3 shows that the receiving cabinets were specifically designed for use with, and to include, the ultra-audion detector, and hence, in use, the receiver and detector were wired up together, side by side, and remained in this way. They thus fulfilled all requirements for inclusion in the type of receiver I mentioned in paragraph 6 of my previously quoted answer. "Plaintiff's Exhibit No. 109" correctly represents a receiving cabinet wired up to its associated ultra-audion detector, as called for in item 3 of "Plaintiff's Exhibit No. 74".

Q. 154. Referring to "Plaintiff's Exhibits Nos. 72 and 73", and the amplfiiers mentioned in your answer to Q. 146, will you state when those amplifiers were delivered to the Navy Department and what, if any, relation they bear to the audion-amplifiers referred to by you in paragraph 12 of your answer to Q. 2, found on P. R., pages 235 and 236 of your former deposition, and illustrated in "Plaintiff's Exhibit No. 85"?

A. The amplifiers referred to were delivered at the New York Navy Yard, and I have no personal knowledge or data at hand to state just when they were delivered. I can

say that they were delivered between January, 1914, when I saw them in process of manufacture, and May, 1914, when I used these devices after they had been forwarded from the New York Yard to the Washington Yard. I recall these amplifiers very clearly, because they were the first purchased by the Navy. They consisted of three stages, the box being narrow and tall, and I have clear recollection of, and considerable data concerning, the interior circuits of these devices.

Referring to the last part of your question, these amplifiers I have just referred to, are identical with those mentioned by me in my answer referred to in the question. "Plaintiff's Exhibit No. 85" shows correctly the wiring of these first amplifiers which I tested at the Washington Yard.

Q. 155. Referring to the vacuum tube transmitters illustrated in your drawing "Plaintiff's Exhibit No. 116", and mentioned in paragraph 14 of your answer to Q. 2, found at pages 236-238 of the Printed Record of your previous deposition, will you state, if you know, whether any of such transmitters were actually used by the Navy, and if so, when, where and how?

A. The Navy purchased and used a large number of sets [fol. 1600] of the type referred to in your question, which were manufactured by the Western Electric Company, and were known as CW 936. The wiring diagram for these is identical with that shown in "Plaintiff's Exhibit No. 116" under the title "Western Electric". These sets were purchased and installed by the Navy about 1917, in large quantities, and were particularly used on sub-chasers. I operated a great many of these transmitters and their associated receivers myself, as installed on such boats.

Q. 156. Referring to the three-stage audion amplifiers, which you have testified to in your present deposition, can you mention any station where you used, or saw used, such amplifiers, and about the time when you used them, or saw them used?

A. I used this type of amplifier myself at the Washington Navy Yard and at the Bureau of Standards, both in 1914.

Q. 157. Referring to paragraph 5 of your answer to Q. 2 of your former deposition, found on P. R., page 232, do you wish to explain any part of that answer? If so, please do so.

A. My answer incorrectly limited such receivers to those arranged for use with the oscillating audion, as well as with

other detectors, and therefore left out of the picture a large number of receivers purchased by the Navy prior to May, 1914, when the oscillating audion first came into notice, and subsequent to June, 1910.

To make my answer complete, the following should be added at the end of paragraph 5 of my answer to Q. 2, of my deposition on R., p. 211:

* In addition to the foregoing, the Navy purchased between the dates of June, 1910, and May, 1914, a number of receivers which fall under the classification of paragraph 5 of my answer to Q. 2, but which were designed for use with detectors of the type common in the Navy prior to the oscillating audion. These detectors, for example, were of the crystal type, the electrolytic detector, the magnetic detector, and the tickler. These receivers differed from those described above only in that no feed-back coil was employed.

* The schematic drawing which I showed in "Plaintiff's Exhibit No. 113" is correct for the type of receiver that I am describing, providing that in this exhibit the coil marked J', the wires leading from it to the two binding posts respectively shown above and below the word "tickler", these binding posts, and the word "tickler" itself, are removed.

* Referring back to my answer to Q. 2, on P. R., page 232 of my previous deposition, paragraph 5 should be amended by changing the words "purchased by or made by" to "purchased by and made by". It should further be modified by changing the word "being" to "beginning", and also the words "bought by or manufactured by" to "bought by and manufactured by", both of these changes occurring in the second last line of paragraph 5. The date "1915" in the last line of paragraph 5, should read "1914".

Coming back to the additions and changes which I have just made to my previous paragraph 5, these additions and changes beginning on typewritten page 1577 of my present deposition, the receivers referred to were used by the Navy in service. I have personal knowledge of their use in 1910 [fol. 1601] and subsequent years, by having used them myself in many ships and shore stations of the Navy, and in having witnessed their use by operators in these locations.

The receivers which I described in my answer to Q. 2,

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* These are additions and changes to paragraph 5 of my answer to Q. 2, Record, page 211.

paragraph 5, as amended by my answer to this present question, were likewise used by the Navy officially in their various stations ashore and afloat. This I know, because I used them myself between the dates of July, 1914 and my leaving the Navy in June, 1919, and likewise saw them being used by Naval operators in regular work.

Q. 158. Referring to paragraph 7 of your answer to Q. 2 of your former deposition, found on page 233 of the Printed Record herein, what was done with the receivers mentioned therein after they were purchased or made by the Navy, from about the middle of 1915, until you left the Navy in 1919?

A. This type of receiver was similarly used by the Navy in Naval service. I have operated a number of them during inspection and test, and have witnessed the operation of many others by Naval operators in the course of their duties, during the period above referred to.

Q. 158-a. What part of 1910 were the synchronous rotary-gaps referred to in paragraph 1 of your answer to question 2, found on Printed Record page 231 of your former deposition, purchased from the National Electric Signaling Company and when were any of them delivered to and installed and used by the United States Navy?

A. I do not know the date of contract of any of these sets, but I do know that some were installed later than the year 1910. Specifically, the 100 kilowatt rotary-gap transmitter at the Arlington Radio Station, Radio, Virginia, was installed in 1912, for I helped put in this set myself. In December, 1912, while en route to the Panama Canal Zone on the U. S. S. Arkansas, I made some comparisons of the signals from this 100 kilo-watt spark station at Arlington and a temporary arc installation at the same point. In February, 1913, I was in charge of the acceptance test of the Arlington Station, carried out on the U. S. S. Salem, and after my return the original contract was modified and the station paid for. I operated the station a number of times after acceptance and saw it in operation by the enlisted personnel many times thereafter.

I also know that a number of synchronous gap sets were obtained from this same company by the Navy, for use on torpedo boat destroyers and the like, and the installations of these were subsequent to 1910. I have tested these sets on several ships, and I have a record of one of these sets having been installed on the U. S. S. McCall in the New

York Navy Yard on December 1911, this set remaining in use until January, 1916.

Referring again to the Arlington Station, during the acceptance tests in February, 1913, both arc and spark signals were copied, but the former were for technical information only. The whole purpose of the voyage was to see if the 100 kilo-watt rotary-gap installation at Arlington was satisfactory and met the terms of the contract.

I do not know the exact date of delivery of the rotary-gap set at Arlington, but I do know that it arrived there in first-hand condition and that it had not been used before.

Q. 159. Can you state approximately when the radio apparatus of the Arlington station, including the rotary spark gap installed there, was paid for?

A. Not for a considerable time after the acceptance test [fol. 1602] in February, 1913. There were many changes in the list of apparatus to be submitted, and I recall that it was probably not until the end of 1913 that payments were made.

Q. 160. What relation has the synchronous rotary-gap set which you helped to install at Arlington with the one illustrated and described by Captain Bullard's article on pages 421-431 of "Plaintiff's Exhibit No. 81"?

A. It is the same set that I helped to install in 1912 at Arlington and that I operated after its acceptance.

Q. 161. Where and when were the open-gap and non-synchronous rotary sets, referred to in paragraph 1 of your answer to Q. 2 of your previous deposition, found on P. R., page 231, used?

A. They were used for service communication between the various installations of the Navy, between the years 1911 and the time when I left the Naval service in the middle of 1919. I have used this type of set myself at times during the above period and have often seen these sets operated by the enlisted men.

Q. 162. Does your drawing, "Plaintiff's Exhibit No. 112, Typical Rotary-Gap Set", represent the circuit arrangement of both the synchronous and non-synchronous spark-gaps, concerning which you have testified in this case?

A. Yes, it correctly represents the circuit arrangements for both types of gap. However, the drawing as shown signifies a non-synchronous gap, in that no physical connection is shown schematically between the gap and the

alternator. To represent correctly in schematic form a synchronous gap installation, a dotted line should be drawn between the center of the circle marked G, and the center of the inner circle of the two concentric circles marked A.

Q. 163. You have stated in paragraph 2 of your answer to Q. 2, found on page 231 of the Printed Record herein, that quenched-gap sets manufactured for the Navy came into use around 1911. In your second deposition, found on page 1493 of the Printed Record herein, you state that about 1910 the open-gap began to be replaced by the quenched-gap. Commander Loftin in his deposition states (P. R., pp. 1112 and 1137) that the quenched-gap transmitter was not available in May, 1910, and that it became an assured success about 1911, was at once adopted by the Navy, and that many sets of the Telefunken type as illustrated in "Plaintiff's Exhigit 79", were purchased. Will you state, if you can, somewhat more definitely when, and from whom, the United States Navy obtained its first quenched-gap radio transmitting set?

A. The Navy obtained its first quenched-gap sets from the Telefunken Company, the date being September, 1910.

Q. 164. Had the United States Navy obtained, or been in possession of, or had it used any quenched-gap transmitting apparatus, prior to July, 1910?

A. No.

Q. 165. How do you know that the first quenched-gap wireless transmitting apparatus was obtained by the United States Navy from the Telefunken Company in September, 1910?

A. The introduction of this type of gap was quite novel and when the circuit drawings of the first sets installed in the Navy reached Washington I studied them very carefully. I recall that the data that I thus obtained was used by me in July, 1911, in tuning up a similar set in the Navy and later in installating several. The exact date of September, 1910, I establish by records which I have looked up.

Q. 166. What are these records you referred to in your last answer?

A. The records I referred to were of two general classes. [fol. 1603] First, were the annual reports made out on a regular form in a printed booklet by every Navy ship and shore station, telling the list of all radio apparatus at the station, with full data as to size, kilo-watts, voltage, etc., maker's name, requisition and contract, and date of in-

stallation of the whole set or of parts thereof. This form was gotten up by the Bureau of Steam Engineering and contained some 30 pages. The form for shore stations was known as "Form MSE 25-A" and for ships "NSE 25-B". These record books were sent to the Bureau and were kept on file for a number of years, when they were supposed to be packed up and either sent away or destroyed. These records were accessible to me and I constantly used them in carrying on my work with the Navy. It was to some of these records that I referred in my answer to the last question, since I examined some of these record books in January and February of this present year, 1928, and found therein data referring to these first two Telefunken installations of quenched-gap transmitters.

The second source of information that I referred to consisted of the regular files of letters, contracts and the like, covering Navy purchase, acceptances, installation, assignment of apparatus, and the like. These records are filed in various file rooms of the Bureau of Supplies and Accounts, the Bureau of Engineering, the various Navy Yards, and other places. I was familiar with these files, particularly with those in the Bureau of Steam Engineering (as the Bureau was known originally until change of the name to "Bureau of Engineering"), during my duties with the Navy Department, and examined many of them in January and February, 1928.

Included in these latter records I may add the files of Navy drawings and specifications available at the Bureau, and also a complete card record showing the assignment of every set purchased under every contract through specific ships, and their transfer from this ship to another if this were done. This also includes, of course, a record of assignment to shore stations as well.

Q. 167. How voluminous were the Ship and Shore Books referred to in your last answer, and how complete did you find them in your recent examination?

A. The current file for these record books would include several thousand. Many thousand other records of this kind, which are out of date, have been destroyed or filed away in some forgotten place. As a matter of fact, many of these books which I examined last January and February had been packed up to be sent away to storage and I happened to gain access to them just before they were sent away. The letter files take up a huge room but are not

destroyed in the same way as the record books. This is true for the Bureau files, but is not true for Navy Yard records. I found in a number of cases that payments had been made for radio apparatus through the New York Yard and that the records of payment had been filed in that Yard, but I was told by the Bureau authorities that these records at the New York Yard could not be obtained because they had been destroyed.

Q. 168. From your knowledge of the procedure in the Bureau of Engineering, could anyone seeking information from these records you have referred to, between 1916 and the time you left the Navy, have obtained information therefrom?

A. I do not think such information could have been obtained. The entire time of the personnel was taken up with work connected with the war, and I know that it was decided [fol. 1604] that no outside interference of this sort would be permitted, as it would seriously interfere with work in getting equipment ready for ships and shore stations.

Q. 169. Specifically, what records or data did you find in your recent examination of the Bureau of Engineering's records with respect to the first Telefunken quenched-gap sets obtained by the Navy in September, 1910?

A. I found the NSE 25-B report of the U. S. S. Reid for 1915, in which it was stated that the transmitter was a 2-k. w. Telefunken, obtained on Requisition 91 of 9-23-10, and that the transmitter was installed at the Norfolk Navy Yard on September 23, 1910. The receiver was also a Telefunken type KOHF, appt. No. 1237.

I also saw the NSE 25-B report of the U. S. Flusser for 1914. This stated that she had a Telefunken transmitter, which was installed in Norfolk in September, 1910. The receiver was a Telefunken Type GAH No. 1122. The report of this same ship, dated January, 1916, stated that the transmitter was a 2-k. w. Telefunken type, installed by representatives of the Telefunken Company and the ship's force at the Navy Yard, Norfolk, in September, 1910. The receiver is listed as above.

These two ships, the U. S. S. Flusser and the U. S. S. Reid, are the two that I recall very clearly as the two that had the first Telefunken apparatus installed in the U. S. Navy.

Q. 170. In your answer to Q. 165 you state that you used certain data in tuning up a similar set in the Navy and later in installing several. Will you please explain this statement a little more fully?

A. In July, 1911, I went to the Navy Yard in Portsmouth, New Hampshire, to adjust the 2 kilo-watt Telefunken quenched-gap transmitter, which had just been installed at that station. A little later, in August, 1911, I was instructed to install a Telefunken quenched-gap transmitter, which had just been purchased by the Navy on the U. S. S. Eagle, at the Portsmouth Navy Yard, and this I did. I operated the transmitter and receiver and then turned the set over to the ship's force, who operated it for some time under my supervision.

Q. 171. Has your recollection of the installation of the Telefunken quenched-gap set on the Eagle been confirmed in any way, and if so, in what way?

A. I have looked up my daily log records for that period and find numerous references to putting on this quenched-gap set on the Eagle in August, 1911.

The card records, which I examined at the Bureau of Engineering in January and February, 1928, showed that a Telefunken quenched-gap transmitter, purchased under Requisition 82, Contract 144, was installed on the U. S. S. Eagle. In the Bureau files I found a letter dated August 10, 1911, No. 45712-195-6 from the Bureau of Engineering to the Commandant, Portsmouth, New Hampshire. Re disposition of material obtained on NSA requisition 82 of August 7, 1911. The letter states that Item No. 2 of this requisition is intended for the U. S. S. Eagle, and requests installation on board the U. S. S. Eagle upon delivery from the Telefunken Company O. K.

Q. 172. I call your attention to "Plaintiff's Exhibit No. 46", part of which consists of a contract No. 144, dated September 20, 1911, between the Telefunken Company and the United States Navy, Requisition 82. What relation has this contract to that referred to in your last answer and to the apparatus you installed on the Eagle?

A. This part of "Plaintiff's Exhibit 46" is the contract [fol. 1605] which was based on Requisition 82 NSA, which I have just referred to in my answer to Q. 171. The contract specifies that Item 2 is one 1-k.-w. Wireless telegraph set. Under the heading "Delivery" it is stated that Item 2 is to be delivered to the General Store-keeper, Navy Yard,

Portsmouth, New Hampshire. It is to be noted that the requisition is dated August 7, 1911, installation was made toward the latter part of August, 1911, and the contract was not signed until September 20, 1911.

Q. 173. How do you explain the installation being made before the signing of the contract?

A. This was on account of some emergency, and during the time that I was with the Navy, such action was not uncommon. Drawing up of the requisition gave the Bureau authority to order shipment in advance of actual signing of contract.

Q. 174. I notice that "Plaintiff's Exhibit No. 46, Telefunken Contract 144", has attached to it a letter of the Telefunken of August 22, 1912, and a bid of the Telefunken Company of August 23, 1912, based on "Requisition No. 69". What connection has that bid and letter with the contract?

A. None. There is evidently a mistake in the assembly of "Plaintiff's Exhibit 46".

Q. 175. Please state, if you know, what, if any, connection there is between the bid of August 23, 1912 based on Requisition No. 69, and the Telefunken Company's letter of August 22, 1912, with a contract of September 4, 1912, between the Telefunken Company and the United States, based on Requisition No. 96, a certified copy of which contract I hand you—that is, if you know, will you explain the connection or relation between these documents?

A. To make this clear, I wish to tabulate "Plaintiff's Exhibit 46" in its three separate items:

(1) Contract 144, Requisition 82.

(2) Telefunken letter of August 22, 1912, explaining a bid.

(3) Bid of Telefunken Company on Requisition 69 dated August 23, 1912.

The sequence of operation as to these items and others to follow is this: The Bureau first drew up a requisition No. 69, for 2 5-KW quenched-gap transmitting sets. This was bid on by two companies, the Telefunken Company, and the Wireless Specialty Apparatus Company. The bid of the Telefunken Company is Item 3 above, and their letter of August 22, 1912 (Item 2 above) explained their bid.

The contract on Requisition No. 69 was awarded to the

Wireless Specialty Apparatus Company, the Contract being No. 133 of 1912.

Some short time thereafter it became necessary to make a quick installation on the U. S. S. Arkansas, which ship was due to get a Lowenstein set, but it was found that this set would not be ready on time. A new and up-to-date set had to be installed, because the President of the United States was due to sail on that ship within a short time for the Canal Zone.

In this emergency, the Bureau barked back to the previous bid of the Telefunken Company, Item 3 above, for it recalled that the Telefunken Company had stated that this apparatus was ready for delivery. Therefore a new requisition was drawn on Requisition No. 96, to obtain this set for the Arkansas, and at the same time its companion set for another ship. The contract awarded to the Telefunken Company waiving competition, for the two 5 KW quenched-gap sets on this new Requisition 96, was numbered 123, dated September 4, 1912, and it is a copy of this contract which you have just handed me.

[fol. 1606] The sets were delivered on Requisition 96, Contract 123, to the U. S. S. Arkansas and the U. S. S. Louisiana. I am fully conversant with the drawing up of these various contracts as I was in touch with this work at the Bureau. I also operated the set that was installed on the U. S. S. Arkansas under this requisition, and saw it being operated by the ship's radio force as well. I also later saw the set on the Louisiana.

I am especially conversant with the details of the installation on the Arkansas and with the type of apparatus delivered, because I was assigned by the Bureau to sail as Radio Expert Aid on that vessel when then President Taft was taken to the Canal Zone on her. It was during this period of time that I operated the set as explained above, and it was also during this cruise that I made tests with the rotary spark transmitter which had just been installed at the Naval Radio Station at Arlington, concerning which I testified in my answer to Question 158.

Q. 176. About when was the installation of the Telefunken set made on the Arkansas, and when did you sail on that vessel?

A. I sailed on the Arkansas on December 19, 1912. From records which I used in connection with the Hill-Clark-Lowenstein suit, with permission of the Navy Department, I find

a statement that the Telefunken 5 KW quenched-gap transmitter was installed on the Arkansas on September 30, 1912.

Q. 177. In referring to the Telefunken set installed on the Arkansas, did the installation or set include any receivers, and if so, what type of receivers?

A. The two receivers on the Arkansas, when I made the trip above referred to, were supplied by the Telefunken Company along with the 5 KW quenched-gap transmitter. One of these was the upright form, Type E-5, and one of these was the horizontal form, Type E-4. I operated both of these sets during the trip and am thoroughly conversant with them. The same receivers were supplied on the Louisiana.

Q. 178. In your answer to Q. 65 of your former deposition, found on page 1503 of the Printed Record, it appears that you sailed on the Arkansas in Christmas week of 1913. How do you explain this in view of your above answer to Q. 176?

A. The date 1913 is a typographical error, as I meant to have the date coincide with the preceding one of 1912, in my answer to Q. 65.

Q. 179. Has your recollection with respect to the installations of the Telefunken sets, referred to in your last few answers, on the Arkansas and Louisiana, been confirmed in any way, and if so, in what way?

A. The Bureau card records which I examined last January and February, show that the two sets delivered under Requisition 96, Contract 123, were installed on the U. S. S. Arkansas and the U. S. S. Louisiana, respectively. A letter in the Bureau files, dated January 9, 1913, No. 88002-121-6, from the Commanding Officer of the U. S. S. Arkansas, to the Bureau of Engineering, states that the apparatus is a 5 KW-500 cycle Telefunken quenched system, Contract 123, Requisition 96.

I also found, in the Bureau files, a letter dated October 12, 1914, No. 135300-268-6, from the Commanding Officer of the U. S. S. Louisiana to the Bureau of Engineering, stating that two Telefunken receivers are installed in the radio station of the ship.

[fol. 1607] Q. 180. What was the construction and operation of the Telefunken transmitter installed and operated on the U. S. S. Eagle and the construction and operation of the Telefunken transmitter and receivers you operated on the U. S. S. Arkansas, and how did they compare with

the Telefunken transmitters and E-4 and E-5 receivers, described by Mr. Graham, whose testimony you referred to on page 271 of the Printed Record herein, and which are illustrated in Mr. Graham's drawing, "Plaintiff's Exhibit No. 79"?

A. The transmitters on the U. S. S. Eagle and the U. S. S. Arkansas were both of the quenched-gap, two-tuned circuit type and were identical with the set described by Mr. Graham, as far as circuit and operation are concerned. "Plaintiff's Exhibit No. 79" correctly shows these two sets schematically, as far as the left hand drawing of "Plaintiff's Exhibit No. 79" is concerned.

The receivers which I used on the Arkansas were of the two-tuned circuit inductively-coupled type using crystal detectors, and were identical with those described by Mr. Graham. Both of these receivers are correctly represented schematically by the right hand drawing of "Plaintiff's Exhibit No. 79."

Mr. Cosgrove: I offer in evidence the Telefunken Contract of September 4, 1912, No. 123, Requisition No. 96, referred to by the witness, and the same is marked "Claimant's Exhibit No. 331, Telefunken Navy Contract of September 4, 1912."

(The Notary so marks said exhibit.)

Q. 181. In paragraph 2 of your answer to Q. 21 found on Printed Record, page 231 of your previous testimony, you referred to the quenched-gap transmitter described in the testimony of plaintiff's witnesses, Messrs. Graham, Langley and Weagant, and stated that in some cases you had seen the identical apparatus those witnesses referred to. Did you see the Telefunken transmitter described by Mr. Graham, illustrated in his drawing "Plaintiff's Exhibit No. 79", and which he testified he installed at the U. S. Naval Station in San Juan, Porto Rico, in 1914? If so, will you please explain the circumstances under which you saw it and where and when you saw it?

A. I saw this Telefunken quenched-gap transmitter in the New York Navy Yard several times prior to its shipment to San Juan. I think that the last time I saw it was early in 1914, and I know I saw it in 1913. I was going through the storeroom in the New York Yard on both these occasions checking up on the apparatus in store. From my

connection with the work at the Bureau I knew at the time and I now recall that this set had originally been purchased for use at Beaufort, N. C., but that the assignment had been changed for San Juan. I was much impressed by the fact that this set lay so long in the New York Yard before installation, knowing that all other sets were installed as quickly as they came, but I knew, however, that this particular set was being held, pending a decision as to dismantling or retaining the San Juan Naval base. I know that a working party was sent from the New York Yard to superintend the installation of this set in San Juan.

Q. 182. In your recent examination of the records of the Navy Department, did you find any reference to this Telefunken quenched-gap transmitter referred to in your last [fol. 1608] answer; if so, what did you find?

A. I found Report Book NSE 25-A for San Juan, January, 1914, which stated that a composite type of transmitter was installed at the station, and then I found in a NSE 25-A report for the same station, dated January 1, 1915, that a 5 KW Telefunken transmitter was then in the station. This establishes the installation date as some time between January 1, 1914, and January 1, 1915.

In the Bureau files I found a letter dated March 9, 1914, No. 119475-835-W from the Bureau of Engineering to the Bureau of Supplies and Accounts, requesting shipment from the Navy Yard, New York, to San Juan, of one Telefunken set with receiver, purchased under Contract 458 of 1911.

Also letter dated June 12, 1913, No. 99448-835 from Machinery Division, New York Yard, to Commandant. General Storekeeper reports 5 KW Telefunken set on Requisition 261, Contract 458, 1911, is in store reserved for Radio Station, San Juan.

Q. 183. Please look at the certified copy of the contract dated April 24, 1911, between the Telefunken Company and the United States, calling for certain wireless apparatus, and state, if you know, what, if any, relation there is between the apparatus called for in this contract and the Telefunken apparatus referred to in your last few answers, which was installed, or intended for installation at San Juan.

A. The document which you hand me is a copy of Contract 458 of 1911, Requisition NSE Bureau 261, the date of contract being April 24, 1911, for radio apparatus to be supplied to the Navy Department by the Telefunken Company. This requisition and contract number coincides with

that just given for the quenched-gap apparatus finally installed at San Juan.

Item 1 of this contract calls for one 5 KW wireless telegraph set and Item 2 calls for one generator and transformer for the same. The contract also contains the statement that these Items 1 and 2 are intended for the wireless telegraph station at Beaufort, N. C.

This completely identifies this contract as that calling for the Telefunken quenched-gap apparatus which, as I have previously testified, was originally purchased for Beaufort and finally installed at San Juan.

Q. 184. What was the condition of the Telefunken quenched-gap transmitter which you saw in the New York Navy Yard and which you say was intended for the San Juan station, the last time you saw it?

A. It bore no marks or other indications of ever having been used. I considered it as absolutely new.

Mr. Cosgrove: I offer in evidence the contract referred to by the witness in his answer to Q. 183, and the same is marked "Claimant's Exhibit No. 332, Telefunken Navy Contract of April 24, 1911."

(The Notary so marks said exhibit.)

[fol. 1609] Q. 185. Referring again to paragraph 2 of your answer to Q. 2 of your former deposition, found on page 231 of the Printed Record herein, you state:

"The Wireless Specialty Apparatus quenched-gap transmitter is correctly shown in schematic diagram in Plaintiff's Exhibit 87, and the description of the parts and the method of adjustment and operation by Langley, whose deposition I have read, is correct."

Will you please state, if you know, when the first Wireless Specialty Apparatus quenched-gap transmitter such as described by Mr. Langley in "Plaintiff's Exhibit No. 87" was purchased by, and delivered to the United States and used by it, and how you became familiar with such transmitter?

A. I know that the first quenched-gap sets made by the Wireless Specialty Company were the two which they made under Requisition 69, Contract 133, awarded to them about September, 1912. I saw the sets being manufactured at their factory in Boston, some time in the early part of 1913, and during October and November, 1913, I had a

great deal to do with the remodeling of these transmitters while they were on acceptance tests at the New York Navy Yard. I remember especially my inspection of the sets at Boston, because at that time I was impressed by the amount of white porcelain insulators used in the sets, other types of insulation having broken down. During the tests at the New York Yard I made frequent reports to the officer in charge of Radio in the Bureau of Engineering, Washington, concerning the changes made in these sets while on acceptance test.

I saw one of these sets on the U. S. S. Idaho some time later, probably two years later. This was at a time when I was inspecting a large number of ships, and I remember this particular installation because of the fact that the transmitter was much larger than any other one of similar rating and took up almost all the available space in the Idaho's Radio Room. I operated this set during this inspection, and I saw it being used for regular Naval work.

These two Wireless Specialty Apparatus quenched-gap sets which I have just described, are the same as those described by Langley and illustrated schematically by "Plaintiff's Exhibit No. 87".

Q. 186. In your recent examination of the records of the Bureau of Engineering, did you find any record relating to the installation at any Naval Radio station of the Wireless Specialty Apparatus referred to in the last question and answer; if so, what records did you find?

A. The Bureau of Card Records show that one 5-kilo Wireless Specialty quenched-gap transmitter purchased on Requisition 69, Contract 133, was installed on the U. S. S. Maryland, the name of which ship was later changed to the U. S. S. Frederick, and that the second set under the same requisition and contract was installed on the U. S. S. Idaho.

[fol. 1610] The Bureau files also contained the following information:

A telegram dated January 27, 1914, No. 116,100,280-6-W from Bureau of Engineering to Navy Yard, New York, asking when 5-k. w. Wireless Specialty set under Contract 133 will be ready for shipment and stating that same is needed for U. S. S. Maryland.

A letter dated January 28, 1914, No. 116,100-280-6-W from Bureau of Engineering to Bureau of Supplies and Account, requesting shipment of one 5-k. w. Wireless Specialty

set purchased under Contract 133, N. P. O., N. Y., from New York Yard to Mare Island Yard, for U. S. S. Maryland.

A letter dated February 2, 1914, No. 116,100-280-6-W from Bureau of Engineering to Commandant Mare Island Yard. Shipment has been made from Navy Yard, New York to Mare Island Yard of one 5-kilo-watt set from Wireless Specialty Company, Contract 133. This due Mare Island February 23, 1914.

A letter dated January 6, 1915, No. 143-768-280-6-W from Bureau of Engineering to Commanding Office of U. S. S. Maryland, stating that the Bureau records indicate that there are now on the Maryland one IP76-1914, and one IP76-1912, both supplied with 5-k. w. sets purchased on Contract 133. The fourth endorsement to this letter by the commanding officer states that IP76-1914, No. 116, and IP76-1912, No. 362, are on that vessel.

A letter dated January 28, 1914, No. 116,203-237-6-W, from Bureau of Engineering to Commandant Philadelphia Navy Yard, stating that the Bureau has requested shipment from New York Yard to Philadelphia Yard for the U. S. S. Idaho, one 5-k. w. radio set purchased from Wireless Specialty Company under Contract 133.

A telegram dated July 17, 1914, No. 130,145-237-6, from Commandant Philadelphia Yard to Bureau of Engineering. The installation of the 5-k. w. Wireless Specialty Apparatus Company set was completed on the U. S. S. Idaho on April 25, 1914.

Referring to references in the above letters to receivers IP76-1912 or IP76-1914, the data following this designation, in the form of the abbreviation "No." followed by a number, refers to the serial number stamped on the particular receiver referred to.

Letter dated July 20, 1914, No. 131,555-280-6, from the U. S. S. Maryland to the Superintendent Naval Radio Service, Washington. Report of heating of quenched-gaps on Wireless Specialty Company set.

The statement in the letter immediately preceding, indicates that the 5-k. w. Wireless Specialty Company set whose shipment to the U. S. S. Maryland was requested in the letter dated January 28, 1914, above quoted, had been installed prior to date of July 20, 1914.

I have examined ships radio inventory NSE 25-B for the U. S. S. Frederick, formerly the U. S. S. Maryland, stating the apparatus on board January, 1918. This in-

cludes a Wireless Specialty Company transmitter obtained on Contract 133, Requisition 69, N. P. O. 1913, a receiver type IP76, serial number 436, and a receiver type IP76, serial number 362.

Q. 187. I show you "Plaintiff's Exhibit No. 60". Will you state what relation that exhibit has with reference to the testimony you have given in regard to the Wireless Specialty Apparatus transmitters and receivers?

A. This is Contract 133 made by the United States Navy [fol. 1611] under Requisition 69 with the Wireless Specialty Apparatus Company on September 21, 1912, for the purchase of two 5-k. w. quenched-gap transmitters, each transmitter to have three IP76 receivers. This is the contract to which I have referred in my answer to the preceding question, and under which the sets were obtained concerning which I have just testified in my answer to the preceding question.

Q. 188. Will you please state to what extent the United States Navy purchased and used Wireless Specialty Apparatus Company IP76 receivers during the period from June, 1910, to June, 1919, specifying, if you can, the various kinds, types or models of such receivers which were purchased and used during that period?

A. During the period above-mentioned, a very large number of receivers of the general type known as IP76, were purchased by the United States Navy from the Wireless Specialty Apparatus Company, and installed and used by the former in ship and shore stations for regular service communication. More of these receivers were obtained during the above period, than of any other one type.

All receivers of the IP76 class were of the two-tuned circuits form. From time — time physical changes were made, and as the crystal detector art progressed, different crystal detectors were furnished with the receiver, and these minor changes, which did not affect the class of receiver as a whole, led to the use of the designations "IP76-1912", meaning that modification of the IP76 receiver brought out in the year 1912. Similarly, models IP76-1913, IP76-1914, and so on, were brought out.

I have personally used all the foregoing models of IP76 receivers in official work for the Navy Department, and have seen every one of these designs operated by the enlisted personnel in their regular duty. I installed many of these IP76 type receivers myself in various ships and

shore stations during my term of duty with the Navy Department, such as, for example, on the U. S. S. Eagle, in August, 1911, and at the Washington Navy Yard, at which latter point new models were installed for use as they were purchased by the Navy Department.

Q. 189. How did the IP76 receivers, referred to in your last answer, compare in construction and operation with the IP76 receivers described by Mr. Langley and illustrated in "Plaintiff's Exhibit No. 87"?

A. All the IP76 receivers which I have described in my last answer, are correctly illustrated by "Plaintiff's Exhibit 87" and are identical in construction and operation with those described by Mr. Langley.

Q. 190. I call your attention to "Plaintiff's Exhibit No. 62", Contract No. 170, "Plaintiff's Exhibit No. 63", Contract No. 19,882, both entered into in the years 1912 and 1913, with the Wireless Specialty Apparatus Company, calling for certain receivers. If you know, will you state what relation the receivers of these two contracts bear to the IP76 receivers, referred to in your answer to Q. 188?

A. The receivers called for in these two contracts are of the type included among those described in my answer to Q. 188.

Q. 191. In your recent examination of the records of the Bureau of Engineering, did you find any record relating to the purchase and installation of any type IP76 receivers [fol. 1612] called for by "Plaintiff's Exhibits Nos. 62 and 63"?

A. The records show hundreds of installations of IP76 receivers subsequent to June, 1910, and before June, 1919. Ship reports NSE 25-B and shore station reports NSE 25-B, contain a great many references to the possession by the station of IP-76-1912 receivers, for example, which are identical with the IP76-1912 type which was supplied under Contract 19,882. However, none of these records that I have examined, states specifically that such receivers were supplied under either Contract 170 or Contract 19,882, the reason being that name-plate data giving the contract and requisition number was rarely placed on receivers of this type and, hence, the operator making the report had no data available to include in his report.

Q. 192. Did you find any record in your recent examination of the files of the Bureau of Engineering, Navy Depart-

ment, relating to the apparatus called for in "Plaintiff's Exhibit No. 57", Contract No. 14,746, of April 22, 1911, with Fritz Lowenstein; if so, will you please state what such record disclosed in respect to such apparatus?

A. Yes, I did. For example, in the Bureau files I found a letter dated November 26, 1912, No. 83,482-488-6-W, from Bureau of Engineering to F. Lowenstein, New York, regarding Contract 14,746-5 k. w. set for Wyoming. The Bureau notes that one 5-k. w. transmitting set has been delivered at New York, and is ready for test. This set to be installed on the U. S. S. Wyoming if it passes test.

Letter dated January 1, 1914, No. 114,622-132-6-W, from Bureau of Engineering to Commandant, Navy Yard, New York. The 5-k. w. Lowenstein transmitter under Contract 14,746, now on board the U. S. S. Texas, temporarily installed, should be permanently installed for battle radio on that vessel.

Q. 193. Did you see any of the Lowenstein transmitters referred to in the last question and answer, and if so, when where and under what circmstances?

A. Yes, I saw one of the Lowenstein 5-k. w. transmitters of the quenched-gap type made under Contract 14,746, when it was on test at the New York Navy Yard in the latter part of 1912, probably in November or December. I saw this set early in 1913, after it had been installed on the U. S. S. Wyoming after acceptance, and used this set myself, and also saw it operated by the personnel attached to the vessel. This set I especially recall, on account of the fact that it was the first set submitted by Lowenstein which contained a quick wave-changer, a device in which the Bureau was very much interested, and it was for this reason that I inspected the set closely, when it was on test at the New York Yard, and also made very many tests with it on the U. S. S. Wyoming. I also passed on the specifications on which this set was made, and am very familiar with them. I discussed these specifications very many times with Lowenstein when he brought them up before the Bureau as a new form of transmitter, and I later recommended purchase of the set which I have already said was finally installed on the Wyoming.

Q. 194. What was the construction and operation of this Lowenstein transmitter in the arrangement of circuits, which you saw operated on the U. S. S. Wyoming? You

may refer to the contract or any other exhibit in the record with which you are familiar.

A. This transmitter was of the two tuned circuits, quenched-gap type and the circuits are correctly represented by drawing T-8, which forms a part of "Plaintiff's [fol. 1613] Exhibit No. 57". The construction of the transmitter was in accordance with Navy Specifications 16-T-5, as modified by Lowenstein in papers attached to Contract 14,746, forming "Plaintiff's Exhibit 57". The transmitter is correctly shown schematically by "Plaintiff's Exhibit No. 101", entitled "Waterman Sketch, Lowenstein Transmitter".

Q. 195. Mr. Graham, in his testimony herein, in answer to Q. 41, found on pages 54 of the Printed Record herein, which you previously stated you had read, testified that he recognized sheet T-8, forming part of "Plaintiff's Exhibit No. 57", as a Lowenstein transmitter which he saw in the New York Navy Yard in the latter part of 1914; whereas, you state that the Lowenstein transmitter of this contract was installed on the Wyoming in 1912. Have you any explanation of the different dates specified by you and Mr. Graham?

A. I saw the Lowenstein transmitter which Mr. Graham testified he saw in the New York Yard in 1914, and my inspection of it was at that same place, and during that same year. I clearly recall the type of quenched-gap described by Mr. Graham, and made tests of this gap while the set was on test at the New York Yard. This was a later type of set, designed by Lowenstein, and of later date than the set which he made and which was installed on the Wyoming, but schematically, the 1914 set, above referred to, is correctly represented by the diagram T-8, in "Plaintiff's Exhibit No. 57". The two sets referred to were electrically the same, but differed in mechanically construction.

Q. 196. Please examine "Plaintiff's Exhibit No. 55", Navy Contract 415, of June 5, 1913, with the Wireless Improvement Company, Requisition No. 98, calling for certain wireless telegraph apparatus, including one 5-k. w. high frequency quenched-gap transmitter, and state what knowledge you have of the delivery to, and installation and use by the United States Navy, of any of the apparatus called

for by that contract, specifying, if you know, the approximate date of delivery and use of such apparatus.

A. This contract calls for delivery of one 5-k. w. quenched-gap transmitter to the United States Navy Department at the U. S. Naval Radio Station, Radio, Virginia, by the Wireless Improvement Company. The contract is dated June 5, 1913, and delivery is specified within ninety days. The set is to conform to Specification 16-R-1, which same specifications I myself prepared at the Bureau. The station specified for delivery is commonly known as the Arlington Radio Station.

I know that in the latter part of 1913, or early in 1914, this 5-k. w. quenched-gap transmitter made by the Wireless Improvement Company was delivered at the Naval Radio Station, Radio, Va. I assisted in installing this set and calibrated it for wave-length after installation. I operated the set from time to time and saw it handled in the routine operation of Navy message work by the enlisted personnel during 1914 and thereafter, but not since leaving the Navy in June, 1919.

Q. 197. What, in general, was the construction and operation of the Wireless Improvement quenched-gap transmitter referred to in your last answer, and how did that construction and operation compare with the Wireless Improvement transmitter described in Mr. Langley's testimony, illustrated in "Plaintiff's Exhibit No. 88", referred to in paragraph 2, in answer to Q. 2, found on page 231 [fol. 1614] of the Printed Record herein?

A. The transmitter referred to in your question was a quenched-gap type, two-coupled circuits form of transmitter, each of the both circuits being tuned. The construction and operation were the same as described by Mr. Langley, and the schematic diagram corresponds with the circuit drawing shown in "Plaintiff's Exhibit No. 88".

Q. 198. Do you know anything about the delivery to, installation and use by the United States Navy of any of the apparatus called for in "Plaintiff's Exhibit No. 70", Contract No. 24,605, dated September 2nd, 1915, with Emil J. Simon; if so, state what you know.

A. The contract calls for 25 radio sets in accordance with schedule 8121. This schedule forms a part of "Plaintiff's Exhibit No. 70", and specifies the power of the sets to be ½-k. w.

I know that these sets were delivered an passed by the Navy Department and installed in various locations, chiefly on submarines. I operated many of these sets in submarines of the United States Navy, in 1916, and saw the sets being operated by the Naval personnel in regular work. I have no knowledge of the specific date of delivery, but delivery was made between the date of September 2, 1915, when the contract was signed, and some time in 1916, when I operated sets obtained under this contract.

Q. 199. What was the construction and circuit arrangement of the Simon transmitters referred to in your last answer, and how did they compare with the transmitters described by Mr. Langley, referred to in paragraph 2 of your answer to Q. 2 of your previous deposition, found on page 231 of the Printed Record herein, and illustrated in "Plaintiff's Exhibit No. 89"?

A. These transmitters were identical with those described by Mr. Langley. They were of the quenched-gap, two tuned circuits type, and schematically are correctly represented by "Plaintiff's Exhibit No. 89".

Q. 200. Did you find in the files of the Bureau of Engineering of the Navy Department any records of the installation of any of the Simon transmitters referred to in your answers to Qs. 198 and 199; if so, please refer to some of these records.

A. On examination of the Official inventory report of Naval ships, forming NSE 25-B, a number of records of Simon transmitters of the types referred to in my answers to the last two questions, were found. Among these were the following:

Report of radio equipment on board the submarine U. S. S. D-3, dated 1919, shows that the transmitter was a Simon ½-k. w. set installed March, 1916, and obtained on Requisition 233, Contract 24,605, September 2, 1915.

Report of the U. S. S. Barry, for 1917, states that the transmitter was a Simon ½-k. w. set installed February 2, 1917, and obtained on Contract 24,605, Requisition 233 of 1915.

Report of Fire Island Light Vessel No. 68, dated September, 1918, states that the transmitter was a ½-k. w. Simon set obtained on Contract 24,605, Requisition 233, and installed February, 1916.

Many other similar records were found showing the installation of Simon ½-k. w. sets under the above requisition and contract on U. S. submarines and other vessels.

Q. 201. Have you any knowledge relating to the radio sets called for in "Plaintiff's Exhibit No. 69", Contract [fol. 1615] No. 225 of January 31st, 1914, with the Kilbourne & Clark Manufacturing Company? If so, what was such knowledge?

A. This contract called for five 5-k. w. quenched-gap radio sets. I was present at the New York Yard under orders, in October and November, 1913, to test a sample transmitter submitted by the Kilbourne & Clark Company, in order to see whether or not it complied with specifications. My report was that it did not, and that the set was unsatisfactory. I was informed by the Bureau that despite these faults, a contract would be given to that company for five transmitters of 5-k. w. size. From my official work at the Bureau I know that this contract was awarded, and that the contract thus awarded is the one shown in "Plaintiff's Exhibit No. 69". I also know, for the same reason, that the sets were delivered and accepted, and that they were installed and used for actual service. I also know that I saw one or more of these sets in operation some time between the years 1915 and the date that I left the Navy, in June, 1919.

Q. 202. In your recent examination of the records of the Bureau of Engineering of the Navy Department, what, if anything, did you find as to the installation and use by the United States of any of the apparatus called for in "Plaintiff's Exhibit 69"?

A. The Bureau's card records indicated that the five 5-k. w. transmitting sets obtained from Kilbourne & Clark, Contract 225, Requisition 186, were installed on the S. S. Achilles, the S. S. Ulysses, the U. S. S. Saturn, the U. S. S. Denver, and at the Great Lakes Training Station. From the Bureau's files a number of letters were found, which are herein briefly abstracted:

Letter of March 13, 1915, No. 149,220-557-6-W, from Bureau of Engineering, to Industrial Manager, Navy Yard, New York, stating that the Bureau has assigned the second 5-k. w. set purchased from Kilbourne & Clark under Contract 225, to the S. S. Achilles.

Letter dated June 17, 1915, No. 158,390-557-6, from E. A. Drake, Panama Railroad Company, to Chief of Office, Pan-

ama Canal, Washington, stating that he recently visited the S. S. Achilles and the S. S. Ulysses, and recommends that the Kilbourne & Clark sets be removed from those vessels.

By reference to the foregoing two letters, it is clear that the installation on the Achilles of the 5-k. w. quenched-gap Kilbourne & Clark set, obtained on Contract 225, was between the date March 13, 1915, when the Bureau assigned this set to that ship, and the date June 17, 1915, when recommendation was made that this set be removed from that ship.

Continuing with the Bureau records:

Letter of December 19, 1916, No. 209,653-391-6, from Fleet Radio Officer, Pacific Fleet, to Bureau of Engineering. The U. S. S. Saturn was inspected December 19, 1916, and is equipped with a 5-k. w. Kilbourne & Clark set.

Letter dated November 26, 1916, No. 207,162-184-6, from Pacific Fleet Radio Officer to Commander-in-Chief, Pacific Fleet, and forwarded to Bureau of Engineering. Inspection of radio apparatus on U. S. S. Denver. This ship has just been equipped with a new 5-k. w. Kilbourne & Clark radio set.

Letter dated March 15, 1915, No. 148,991-557-6-W, from Bureau of Engineering to C. E. Dole, Purchasing Agent. [fol. 1616] Panama Canal, Washington, stating that the wireless equipment for the S. S. Ulysses and the S. S. Achilles will be supplied by the Navy Yard, New York.

Report of Expert Radio Aide Hallborg, dated June 11, 1915, No. 158,054-557-6, states that a 5-k. w. Kilbourne & Clark set was installed by him on the S. S. Ulysses, beginning May 18, 1915.

Q. 203. Did the Bureau of Engineering records contain any reference to the Maryland Steel Company or the U. S. Panama Canal Commission, having any connection with the installation of the Kilbourne & Clark sets on the S. S. Achilles and S. S. Ulysses, and if so, what did they disclose in this respect?

A. Yes, I found records which disclosed that two of the 5-k. w. sets purchased by the Navy Department under "Plaintiff's Exhibit No. 69" were delivered to the New York Navy Yard, and furnished by that Yard to the Maryland Steel Company with the approval of the U. S. Panama Canal Commission, for installation on the S. S. Achilles and Ulysses, which the Maryland Steel Company was building. It was stated that the cost was to be charged to "Miscellaneous Material, Purchases, etc., on Isthmian Canal. Pan-

ama Canal Colliers Ulysses and Achilles", to be deducted from the contract price of the vessels. Among such records were the following:

Letter dated September 4, 1914, No. 133,806-557-6, from Maryland Steel Company to Bureau of Engineering, requesting specifications of radio apparatus to be installed on Panama colliers Ulysses and Achilles.

Letter dated February 9, 1915, No. 146,812-557-6, from Maryland Steel Company to Bureau of Engineering, requesting delivery date of sets for Achilles and Ulysses.

Letter dated February 13, 1915, No. 146,812-557-6-W, from Bureau of Engineering to Maryland Steel Company, stating that proposed date of delivery of sets for Ulysses and Achilles probably cannot be anticipated.

Letter dated March 15, 1915, No. 148,991-557-6-W, from Bureau of Engineering, to C. E. Dole, Purchasing Agent, Panama Canal, Washington, stating that wireless equipment for Ulysses and Achilles is to be supplied by New York Navy Yard.

Letter dated April 3, 1915, No. 149,220-557-6-W, from Bureau of Engineering to Industrial Manager, Navy Yard, New York, stating that cost of radio sets for Achilles and Ulysses should be charged to "Miscellaneous material purchases, etc. on Isthmus, Isthmian Canal, sub head Panama Canal colliers Ulysses and Achilles".

Letter dated September 9, 1915, No. 162,976-557-6, from Maryland Steel Company to Bureau of Engineering, estimating decrease of $4,400 in contract price of Ulysses and Achilles because each vessel's radio set furnished by Government.

Letter dated September 1, 1915, No. 164,916-557-6, from Assisting Purchasing Officer, Panama Railroad Company, New York, requesting instruction books for Kilbourne & Clark sets on Ulysses and Achilles, books to be charged against Panama Railroad Company.

Letter dated November 10, 1915, No. 169,171-557-6, from Maryland Steel Company to Bureau of Engineering acknowledging letter reporting decrease of cost on Ulysses and Achilles.

Letter dated May 9, 1916, No. 187,284-557-6, from Josephus Daniels, Secretary of Navy, to Major E. I. Brown, General Purchasing Officer, Panama Canal, Washington, [fol. 1617] replying to their letter of the 15th ult., stating that Panama Railroad Company wishes Kilbourne & Clark

sets now on Ulysses and Achilles returned to Navy Department. States that sets were installed not at the instance of the Navy Department, but at the instance of the Maryland Steel Company, which stated to Bureau of Engineering that they could not provide sets for the Ulysses and Achilles, and requesting same to be furnished by the Bureau.

Navy Department invoice 22950-4282, dated January 21, 1916, giving the charge by the Navy Department to the Panama Canal for the Kilbourne & Clark sets and installations on Ulysses and Achilles.

Letter dated March 10, 1915, No. 148,542-557-6-W, from Bureau of Engineering to Maryland Steel Company, stating set for Achilles will be ready for shipment from New York about March 25.

Letter dated March 29, 1915, No. 149,220-557-6, from Bureau of Engineering to Bureau of Supplies and Accounts, stating that the cost of sets from Ulysses and Achilles to be charged to Panama Canal and deducted from contract price of colliers.

The Bureau records also show that in November, 1917, both Kilbourne and Clark sets were removed from the Ulysses and Achilles and were purchased by the Navy Department from the Panama Canal. $1,630.00 paid for set from Ulysses and $1,295.00 for set from Achilles.

Q. 204. What, if any, knowledge have you of the purchase by the United States Navy from the National Electrical Supply Company and delivery to, and use by, the United States Navy subsequent to June, 1910, and prior to June, 1919, of the field radio pack sets of the type described in Mr. Graham's testimony mentioned in paragraph 2 of your answer to Q. 2, found on Printed Record page 231 of your previous deposition, and illustrated in Mr. Graham's drawing, "Plaintiff's Exhibit No. 80"? Please specify the date of purchase, delivery and use, if you recollect them.

A. I personally know that between the dates 1910 and 1919, the United States Navy purchased, installed and used in regular Navy communication, many of the field or pack or portable sets of the type described by Mr. Graham, and illustrated in his schematic drawing, "Plaintiff's Exhibit No. 80". These sets were all obtained from the National Electrical Supply Company, in Washington, D. C. I prepared the Navy specifications for these sets, followed their construction at the factory, tested them for acceptance at the Washington Navy Yard, and recommended acceptance. I

know that all these sets between the dates mentioned were accepted and turned into store at the Washington Yard. With the exception of the last type delivered in 1918 and 1919, some of which were not taken from store, all preceding types I know were issued to the Service, and I have personally used in official work, and seen used by operators in Navy communication, all of these types of sets.

The types of pack sets which conform with that described by Mr. Graham, are type A, B, C, D, and E, with all of which I am familiar as stated above.

Q. 205. Did you find any record in the Bureau of Engineering as to any portable field pack set of the National Electrical Supply Company having been installed and used on the U. S. S. Georgia?

A. Yes, I did. During my search through the Bureau files in January and February, 1928, I found a letter dated May 16, 1914, No. 123,022-897-W, from the Commander-in-Chief to the U. S. S. Arkansas, Flag Ship of the Fleet, at Vera Cruz, giving the names of the vessels equipped with [fol. 1618] pack sets supplied to the Navy by the National Electrical Supply Company. This states that the U. S. S. Georgia had a type A, Serial No. 103 set. I also found a letter dated May 12, 1917, No. 227,924-771-W, from the Bureau of Engineering to the Commanding Officer of the U. S. S. Georgia, requesting that a pack set from the National Electrical Supply Company made on NSA Requisition No. 42 of July 6, 1912, be shipped to the New York Navy Yard, and stating that the shipment was made May 15, 1917.

The particular set type A, Serial No. 103, referred to in the preceding letters, is identical with that described by Mr. Graham, and its circuit diagram is the same as that schematically shown in "Plaintiff's Exhibit No. 80".

Q. 206. Did the files of the Bureau of Engineering of the Navy Department contain any records as to purchase by, and delivery to, the United States Navy, and the use by it, of any of the radio apparatus called for in "Plaintiff's Exhibit No. 71", Contract of March 5, 1914, No. 20,437, with the Radio Telephone and Telegraph Company, and if so, will you briefly state what those records disclose?

A. Yes, the inventory book NSE-25-B of the U. S. S. Olympia, dated March, 1918, states that the transmitter is a 5-k. w. set, obtained from the Radio Telephone and Telegraph Company on Contract 20,437, Requisition 180, of

March 5, 1914, and that this set was installed at the New York Navy Yard February, 1918.

This contract and requisition are the same as those contained in "Plaintiff's Exhibit No. 71", which calls for two k. w. sets for use on the U. S. S. Olympia and San Franciso.

Q. 207. Have you made any copies of any of the records of the Bureau of Engineering of the Navy Department which you have referred to, and if not, how have you been able to refer to these records in detail?

A. No. I have not made any copies of Navy documents for use in this deposition. My replies have been based on abstracts and notes which I made from such records and documents during January and February, 1928.

Q. 208. Have you read the testimony of Mr. Roy A. Weagant, at Printed Record, pages 101 and 102, herein describing certain radio apparatus which he testifies was manufactured by the Navy, and have you examined his drawing of such apparatus, "Plaintiff's Exhibit No. 93"?

A. Yes, I have.

Q. 209. Have you any personal knowledge as to whether such apparatus, described by Mr. Weagant, and illustrated by "Plaintiff's Exhibit No. 93", was manufactured by the Navy and, if so, please state approximately the date of manufacture and also use by the Navy.

A. Yes, I know that transmitters and receivers of the type described by Mr. Weagant, and whose circuit drawings correspond with the schematic sketch of "Plaintiff's Exhibit No. 93", were manufactured by the Navy and installed and used at ship and shore stations, and I have used this apparatus myself, and have seen it used in Service work by operators. This manufacture, installation and use began about 1915. I had much to do at the Bureau with getting up the system whereby each Navy Yard would manufacture a specific piece of apparatus, for example, the Philadelphia Navy Yard making non-synchronous gaps. The various parts were assembled at the New York Navy Yard and the Mare Island Yard, to form what was known [fol. 1619] as the "Navy Composite Set". The Washington Navy Yard designed and made receivers and audion control boxes, which were sometimes used with these composite sets. Not all the material in these sets was manufactured by the Navy, as different parts were, from time to time, purchased from contractors.

Q. 210. Who supplied the antenna for the wireless transmitters and receivers purchased and used by the U. S. Navy since June, 1910, and prior to June, 1919, concerning which you have testified herein, and which you used and operated, and how was the ground connection made in such apparatus, installed on United States vessels?

A. The Navy Department itself always supplied and constructed the antenna for the transmitters and receivers which it purchased, installed and used. In the case of vessels using such equipment, the ground was made to some part of the vessel if the hull were of metal, or to a metallic plate attached outside the hull, if the latter were of wood, or non-conducting material.

Q. 211. In your present and former depositions herein, you have referred to the purchase, installation and use by the United States Navy between June, 1910, and June, 1919, of various types of radio transmitters and receivers. Will you please state what the procedure was in the Navy Department in ordering and in distributing such radio apparatus to Naval stations, and for what purpose such apparatus was used during that period?

A. In some cases, transmitters and receivers were ordered in the same requisition, in other cases separate requisitions were made for transmitters and receivers. However, when both transmitter and receiver were ordered in the same requisition, it did not necessarily mean that both would be used in the same location. Assignments of each class of apparatus, that is, transmitter and receiver, were made independently of each other, to suit the needs of the Service.

For example, the set purchased by the U. S. S. Eagle, in 1911, included both transmitter and receiver of the Telefunken type, but when I installed the set on that ship, the transmitter only was put on, and a IP76 receiver was used with this Telefunken transmitter. On the other hand, the transmitter and receiver obtained together on Requisition 96, Contract 123, for the U. S. S. Arkansas, were installed together on that vessel.

As to use of such apparatus, a receiver of a given make, such as the IP76, whether installed at a shore or a ship station, would, in the course of official Naval work, receive messages from transmitters of all kinds for which it was adopted, such as transmitters made by the Telefunken Company, the Wireless Specialty Apparatus Company, the Wire-

less Improvement Company, the National Electric Signalling Company, and others. With the exception of some few sets remaining in store and not issued to the Service at the time I left the Navy in 1919, all sets, both transmitters and receivers, purchased, installed and used by the Navy, were so used for the purpose of radio communication between ships, between shore stations and between ships and shore stations.

Q. 212. Do you know whether the Navy Department ever sold, or offered for sale, any of the types of radio apparatus that you have referred to in your depositions herein, and if so, will you state generally what apparatus was sold, when it was sold, and the circumstances under which it was sold?

A. After the war, and before I left the Navy Department, preparations were made by the Navy to sell a large amount [fol. 1620] of radio apparatus, including tuned coupled circuit receivers, tuned coupled circuit quenched-gap transmitters, rotary-gaps, vacuum tubes, quenched gaps and other apparatus. Some of this apparatus was manufactured by the Telefunken Company, the Wireless Specialty Apparatus Company, the Wireless Improvement Company, Emil J. Simon, the National Electrical Supply Company and F. Lowenstein.

In December, 1920, a year and a half after I left the Navy, a mimeographed catalogue was issued, offering this material for sale. I have seen this catalogue, and from time to time I have visited one of the Navy Yards where this material was exposed for review of purchasers, and I know that large quantities of it was sold.

* * * * * * *

[fol. 1621] Last Question by the Notary: Do you know of any other matter relative to this claim?

Answer. No.

* * * * * * *

Mr. Vail: Counsel for claimant offers in evidence a photostat copy of pages 344 to 347, inclusive, of the Proceedings of the Royal Society of London, under date of June 10, 1902, which contain an article by G. Marconi, entitled "A Note on the Effect of Daylight Upon the Propagation of Electromagnetic Impulses Over Long Distances", and requests that the same be marked "Claimant's Exhibit No. 333".

Also, a photostat copy of page 86 of "Manual of Wireless Telegraphy for the Use of Naval Electricians", by Lieutenant-Commander S. S. Robison, U. S. Navy, 1906, as "Claimant's Exhibit No. 334".

Pages 239 to 241, inclusive, of "Robison's Manual of Radio Telegraphy and Telephony for Use of Naval Radiomen", by Commander (now Admiral) Robison, U. S. Navy, revised by Commander S. C. Hooper, U. S. Navy, 1924 (6th Edition), as "Claimant's Exhibit No. 335".

Pages 3, 4, 5, 6, 8, and page of diagrams accompanying the same, of book entitled "Wireless Telegraphy, Signal Corps, U. S. Army", by Major Edgar Russell, Signal Corps, U. S. A., Fort Leavenworth, Kansas, 1910, as "Claimant's Exhibit No. 336".

Pages 176 to 178, inclusive, of a book entitled "Wireless Telegraphy", by Dr. Zenneck, translated from the German by A. E. Selig, 1st Edition, 1915 (book from which "Defendant's Exhibits D-5 and H-6" were taken), as "Claimant's Exhibit No. 337".

Certified copy of contract dated June 29, 1912, between Captain R. J. Burt, Signal Corps, U. S. Army, and The Marconi Wireless Telegraph Company of America, No. 536, together with the additional papers appearing upon the records and files of the Returns Office, Department of the Interior, as "Claimant's Exhibit No. 338".

Certified copy of the Provisional Specification as lodged on the 2nd June, 1896, in connection with Marconi's application for patent No. 12,039, of 1896, filed on the 2nd June, 1896, as "Claimant's Exhibit No. 339".

Certified copy of the File Wrapper and Contents of the application for Letters Patent of Nikola Tesla, No. 645,576, granted March 20th, 1900, as "Claimant's Exhibit No. 340".

(The Notary marks all of said exhibits as requested.)

* * * * * * *

[fol. 1622] "Stipulation as to Foote-Pierson Pack Sets

"It is hereby stipulated and agreed, subject to correction should error appear, that one or more of the Field Wireless Pack Sets called for by Plaintiff's Exhibit No. 53, contract dated June 27, 1910, between C. De F. Chandler, Captain, Signal Corps United States Army, and the Foote-Pierson Company, Order No. 8503, and diagrammatically

illustrated in Plaintiff's Exhibit No. 102, Waterman Sketch Foote-Pierson Pack Set (P. R., p. 196) was or were delivered to, accepted and used by the Signal Corps of the United States Army in wireless communication subsequent to the date of said contract and some time in the latter part of the year 1910 and thereafter."

"Stipulation

"It is stipulated and agreed, by and between counsel for [fol. 1623] the respective parties herein, that if James J. Cosgrove were called as a witness in this suit and duly sworn, he would testify as follows:

"That he is a member of the firm of Sheffield & Betts, of 27 Cedar Street, New York City, who acted as solicitors for the plaintiff in the suit in the United States District Court for the District of Delaware brought against Radio Audion Company for infrinement of claims 1 and 37 of Fleming patent No. 803,684, and that he was familiar with the proceedings in that suit.

"That a motion for preliminary injunction was made in said suit to restrain the defendant therein from infringing upon claims 1 and 37 of the Fleming patent by manufacturing and selling three-element vacuum tubes for use in radio reception as detectors, amplifiers and oscillators.

"That said motion for preliminary injunction was argued on affidavits filed on behalf of both parties, and on or about January 20, 1922, the District Court granted a preliminary injunction restraining the Radio Audion Company from infringing claim 1 of said patent by the manufacture, use or sale of three-element vacuum valves or tubes for use in any arrangement wherein they constituted a detector of wireless waves, but the court was not satisfied on the record before it, consisting largely of *ex parte* affidavits, that the defendant's tubes, when used as amplifiers or as generators of high frequency oscillations, fell within the scope of either claim 1 or claim 37 of the Fleming patent. (See 278 Fed., 628.)

"That the said suit was never brought to trial, but that the amended and supplemental bill *were* dismissed therein without prejudice on plaintiff's motion, for the reasons that the Fleming patent had expired on November 7, 1922, the Radio Audion Company, as Mr. Cosgrove is informed and believes, had discontinued, in April, 1922, the manu-

facture and sale of the three-element vacuum tubes for use as detectors, amplifiers and oscillators, and said Radio Audion Company, on or about December 1, 1922, in a suit in equity in the United States District Court for the District of New Jersey, was placed in the hands of receivers, its liabilities being much in excess of its assets, so that any possible recovery by the plaintiff in the suit for the infringement of the Fleming patent in the District of Delaware would be so inconsiderable as not to compensate the plaintiff for the expense of prosecuting that suit through a trial to final judgment."

* * * * * * *

Counsel for Claimant offers in evidence the following certified copies of decrees obtained in prior litigation relating to the patents involved in the present claim against the United States, and requests that the same be marked as indicated:

"Claimant's Exhibit No. 341", *Marconi Wireless Telegraph Company of American v. United Wireless Telegraph Company and Clyde Steamship Company*, Southern District of New York, in Equity No. 7/200, final decree dated March 25, 1912, on Marconi Patent No. 763,772.

"Claimant's Exhibit No. 342", *Marconi Wireless Tele-* [fol. 1624] *graph Company of America v. National Electric Signalling Company*, Eastern District of New York, in Equity DE-23, final decree dated April 6, 1914, on Marconi Reissue Patent No. 11,913, and Lodge Patent No. 609,154.

"Claimant's Exhibit No. 343", *Marconi Wireless Telegraph Company of America v. National Electric Signalling Company*, Eastern District of New York, in Equity DE-31, interlocutory decree dated April 6, 1914, on Marconi Patent No. 763,772.

"Claimant's Exhibit No. 344", *Marconi Wireless Telegraph Company of America v. National Electric Signalling Company*, Eastern District of New York, in Equity DE-31, final decree dated November 18, 1914, on Marconi Patent No. 763,772.

"Claimant's Exhibit No. 345", *Marconi Wireless Telegraph Company of America v. Atlantic Communication Company, et al.*, Eastern District of New York, in Equity No. 161, interlocutory decree on order for preliminary injunction dated October 15, 1914, on Lodge Patent No. 609,154 and Marconi Patent No. 763,772.

"Claimant's Exhibit No. 346", *Marconi Wireless Telegraph Company of America* v. *DeForest Radio Telephone & Telegraph Company, Standard Oil Company of New York and Lee DeForest*, Southern District of New York, in Equity No. 12/23, interlocutory decree for preliminary injunction dated November 16, 1914, on Lodge Patent No. 609,154 and Marconi Patent No. 763,772.

"Claimant's Exhibit No. 347", *Marconi Wireless Telegraph Company of America* v. *DeForest Radio Telephone & Telegraph Company, et al.*, Southern District of New York, in Equity No. 12/31, interlocutory decree dated October 9, 1916, on Fleming Patent No. 803,684.

"Claimant's Exhibit No. 348", *Marconi Wireless Telegraph Company of America* v. *DeForest Radio Telephone & Telegraph Company*, Southern District of New York, in Equity No. 12/31, interlocutory decree, order re "Amplifiers", dated July 18, 1917, on Fleming Patent No. 803,684.

"Claimant's Exhibit No. 349", *Marconi Wireless Telegraph Company of America* v. *DeForest Radio Telephone & Telegraph Company*, Southern District of New York, in Equity No. 12/31, interlocutory decree re "Oscillions", dated July 9, 1919, on Fleming Patent No. 803,684.

(The Notary marked said exhibits as requested.)

* * * * * * *

(Stipulation of October 16, 1929, re Mr. MacConnach's Evidence)

The parties have entered into and filed herein a stipulation that if Mr. Lewis MacConnach were called as a witness herein, and duly sworn, he would testify as follows:

"1. That he has been Assistant Secretary of the Marconi [fol. 1625] Wireless Telegraph Company of America, the claimant herein, since July 11, 1918, that he was Assistant Secretary of the Radio Corporation of America from November 20, 1919, to December 1923, and from that date to the present time has been and now is Secretary of the Radio Corporation of America.

"2. That on November 20, 1919, the claimant entered into an agreement with the Radio Corporation of America, a copy of which is marked herein as Defendant's Exhibit C-7, and that said agreement has not been terminated or

cancelled, nor has it been modified except as stated below in carrying out some of the provisions thereof.

"3. That shares of stock of the claimant company have been and, when authorized by the Board of Directors of Radio Corporation of America, are from time to time being exchanged for shares of stock of the Radio Corporation of America, such exchange being made directly with stockholders instead of first issuing the shares of stock of the Radio Corporation of America to the claimant, as provided in Art. II, Sec. 2 of said agreement of November 20, 1919; that in such exchange of shares of stock which has taken place the stockholders' certificates of shares of stock of the claimant have been returned to such stockholders, and in any exchange of such shares of stock that may hereafter taken place, when and if authorized by the Board of Directors of the Radio Corporation of America, the stockholders' certificates of shares of stock of the claimant will also be returned to such stockholders, and that all of said certificates of shares of stock of the claimant which have been so exchanged and returned were stamped, and those that may hereafter be so exchanged and returned will be stamped, with a statement that they represent such stockholders' interest or share in the reserved assets referred to in said agreement of November 20, 1919.

"4. That in 1919 the claimant company had outstanding two million shares of stock of the par value of $5.00. Of this amount 1,965,310 shares were delivered by the holders thereof to the Radio Corporation; were stamped by the Corporation Trust Company of New Jersey as agents for the Radio Corporation and returned to the said stockholders. The stamp or imprint made upon each of these certificates was as follows:

"The holder and owner of this certificate by the receipt hereof thus stamped by the undersigned as agent of the Marconi Wireless Telegraph Company of America (herein called the Company) has assented to action of the stockholders' meeting of the Company held on November 20th, 1919, and, in accordance with the circular signed by Radio Corporation of America dated December 20th, 1919 (and as also set out in a stockholders' agreement dated December 20, 1919, which is assented to by the holder hereof), has (1) consented to the dissolution of the Com-

pany, (2) accepted shares of stock of the Radio Corporation of America in full of all claims against and rights in the Company of every kind except as to the Reserved Assets described in said stockholders' action (consisting of certain [fol. 1626] claims against the United States Government and certain private corporations and firms arising from the unlicensed use of apparatus covered by the patents of the Company) and certain claims against the British Government, (3) consented that such Reserved Assets be liquidated as set out in said stockholders' agreement of December 20, 1919 and (4) agrees that this certificate henceforth represents only the right of the owner hereof as stockholder of the Company to share in the proceeds of the liquidation of the Reserved Assets above described by the Trustees in dissolution of the Company.

'The certificate stamped and the interest represented thereby is not transferable upon the books or transfer books of the Company. The Trustees in Dissolution and Radio Corporation of America may treat the holder hereof as the owner for all purposes including distribution of proceeds of Reserved Assets.

Jersey City, January 2, 1920.

Corporation Trust Company of New Jersey,
Agent as above stated.'

"5. That there are now outstanding 34,690 shares of the capital stock of the claimant company not stamped as above.

"6. That the Radio Corporation owns no stock in the claimant company.

"7. That on August 2, 1920, the claimant caused to be filed in the office of the Secretary of State of New Jersey a certificate of dissolution of which a certified copy is offered in evidence by claimant and marked Claimant's Exhibit No. 359."

Pursuant to said stipulation the Notary marks the documents referred to therein as follows:

The agreement of November 20, 1919, between the claimant and the Radio Corporation of America as "Defendant's Exhibit C-7".

The certificate of dissolution as "Claimant's Exhibit No. 359".

Claimant's counsel objects to "Defendant's Exhibit C-7" as immaterial and irrelevant.

(STIPULATION OF OCTOBER 16, 1929, RE MR. COSGROVE'S EVIDENCE.)

The parties hereto have also entered into and filed herein a stipulation that if Mr. James J. Cosgrove were called as a witness herein, and duly sworn, he would testify as follows:

"1. I am by profession an attorney and counsellor at law and a member of the firm of Sheffield & Betts, attorneys and counsellors at law, of the Borough of Manhattan. Since November, 1914, I have been a member of said firm and its predecessors in business. Between 1899 and December ~~1~~5, 1914, I was a practicing lawyer associated with and employed as such by the predecessors in business of said firm of Sheffield & Betts.

"2. Ever since the organization in the year 1900 of the Marconi Wireless Telegraph Company of America, the claimant herein, and down to the present time, the said firm of Sheffield & Betts or its predecessors in business have [fol. 1627] acted as attorneys in law for said Marconi Wireless Telegraph Company of America, particularly in all matters relating to Letters Patent for inventions, such as applying for said Letters Patent, preparing licenses under or agreements relating to all Letters Patent for inventions owned by the claimant and prosecuting or defending suits for infringement of patents in which it was a party.

"3. I am familiar with the license agreements of the claimant herein during the period from the year 1899 to December 8, 1919, under United States Letters Patent in suit herein and the dates when said license agreements were cancelled or terminated.

"4. I am familiar with the proceedings in this suit. I have acted as counsel in introducing some of the evidence herein on behalf of the claimant, including its evidence relating to the types of radio apparatus manufactured by and/or for the United States and/or used by it which it is claimed herein embodies the inventions of one or more of the Letters Patent in suit herein. I am generally familiar with all of the types of radio apparatus manufac-

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tured by and/or for the United States and/or used by it and relied upon in claimant's evidence herein as embodiments of the inventions of one or more of the Letters Patent in suit herein, and the names of the manufacturers thereof or the contractors or others who sold or supplied such apparatus to the United States.

"5. That the claimant herein, during the period from 1899 to December 8, 1919, never granted any release of claims for infringement of any of the Letters Patent in suit herein, nor any license under any of the said Letters Patent, to manufacturers of the aforesaid radio apparatus referred to in paragraph 4 hereof, or any other radio apparatus, nor granted any such release or license to any contractors or others who sold or supplied said or any radio apparatus to the United States, except the written licenses and/or releases granted to the following corporations on the dates and under the Letters Patent in suit herein set opposite their names respectively:

"(*a*) National Electric Signalling Company, October 15, 1914. Lodge Patent No. 609,154, and Marconi Patent No. 763,772.

"(*b*) National Electric Supply Company, July 14, 1914, and April 15, 1915. Lodge Patent No. 609,154 and Marconi Patent No. 763,772.

"(*c*) Moorhead Laboratories, Inc., November 30, 1918; Fleming Patent No. 803,684.

"(*d*) Otis B. Moorhead and Moorhead Laboratories, Inc., April 30, 1919; Fleming Patent No. 803,684.

"(*e*) Otis B. Moorhead and Moorhead Laboratories, Inc., June 6, 1919; Fleming Patent No. 803,684.

"Copies of the foregoing instruments are offered in evidence by claimant and marked 'Claimant's Exhibit No. 360,' the making and execution of which being admitted by defendant.

"(*f*) General Electric Company, October 22, 1919; all patents.

[fol. 1628] "A copy of the foregoing instrument is offered in evidence by defendant and marked 'Defendant's Exhibit A-7', the making and execution of which is admitted by claimant.

"6. That a notice of cancellation of said license to the National Electric Signalling Company of April 15, 1915,

was given to the latter by the claimant on March 1, 1917, such cancellation taking effect on May 30, 1917; that the said license of July 14, 1914, to the National Electric Supply Company was limited to portable apparatus of approximately $\frac{1}{4}$ kilowatt capacity, and substantially as specified in the United States Navy Schedule No. 6896 and to apparatus manufactured for and sold to the United States and that said license to the National Electric Supply Company of April 15, 1915, was also limited to apparatus manufactured for and sold to the United States, and that notice of the cancellation of said licenses to the National Electric Supply Company was given to it by the claimant on December 24, 1915, such cancellation taking effect January 24, 1916; that said license of November 30, 1918, to Moorhead Laboratories, Inc., and said license of April 30, 1919, to Otis B. Moorhead and Moorhead Laboratories, Inc., were terminated on June 6, 1919, and that notices of the cancellation of said license of June 6, 1919, to Otis B. Moorhead and Moorhead Laboratories, Inc., were given to them by the claimant on January 30, 1919, such cancellation taking effect on July 30, 1920.

"7. That the claimant herein, during the period from 1899 to December 8, 1919, never entered into any agreement with the United States or its officers or employees relating to the manufacture, use or sale by the United States of any of the aforesaid types of radio apparatus referred to in paragraph 4 herein, or any other radio apparatus under any of the Letters Patent in suit hereing except such agreements between the United States and the claimant as relate to the manufacture for and/or sale to the United States of claimant's radio apparatus, generally entered into on proposals of the United States and bids by claimant, and an agreement of November 26, 1918, to take effect November 30, 1918, between the United States and the claimant, a copy of which agreement is offered in evidence by defendant and marked Defendant's Exhibit B-7, the making and execution of which said agreement is admitted by claimant."

The said stipulation also includes the following paragraphs:

"8. That it is stipulated by the parties that some or all of the transmitting apparatus of the stations referred to in said agreement of November 26, 1918, included the com-

bination of an elevated antenna, variable self-inductance or loading coils, inductively coupled tuned circuits, synchronous, non-synchronous and/or quenched spark gaps, and an earth connection, and that some or all of the receiving apparatus of said stations included the combination of an elevated antenna, variable self inductance or loading coils, inductively coupled tuned circuits, crystal detectors, and an earth connection; and it is further stipulated that some or all of said apparatus included the combination or combinations of one or more of the claims in issue of the Marconi Reissue Patent No. 11913, the [fol. 1629] Lodge Patent No. 609,154, and the Marconi Patent No. 763,772 in suit.

"9. That there appears in the Congressional Record of February 11, 1919, a copy of a letter dated February 3, 1919, relating in part to the purchase of the stations mentioned in said agreement of November 26, 1918, which copy is marked herein as Claimant's Exhibit No. 361, and it is further stipulated, subject to correction should error appear, that ex-Secretary of the Navy Josephus Daniels signed the original of the aforesaid letter and caused the same to be delivered to Hon. L. P. Padgett.

"10. That copies of said Defendant's Exhibits A-7 and B-7, and Claimant's Exhibits Nos. 359, 360 and 361, may be and hereby are offered in evidence with the same force and effect as the originals thereof."

Pursuant to the last-mentioned stipulation the Notary marks the documents referred to therein as follows:

The agreements between claimant and the National Electric Signalling Company, National Electric Supply Company, Moorhead Laboratories, Inc. and Otis B. Moorhead, as "Claimant's Exhibit No. 360".

Copy of letter dated February 3, 1919, from ex-Secretary of the Navy Josephus Daniels, to Hon. L. P. Padgett, as "Claimant's Exhibit No. 361".

Defendant's counsel objects to the latter exhibit as immaterial and irrelevant.

The agreement of October 22, 1919, between the General Electric Company and Marconi Company, as "Defendant's Exhibit A-7".

The agreement of November 26, 1918, between the claimant and the United States, is marked by the Notary "Defendant's Exhibit B-7".

Claimant's counsel objects to the latter exhibits as immaterial and irrelevant.

* * * * * * *

The parties hereto have entered into and filed herein a stipulation as follows:

"It is hereby stipulated that Claimant's Exhibit No. 36 herein (R. p. 321), a copy of which is annexed hereto, may be accepted in evidence herein as a correct translation of the judgment of the Civil Tribunal of the Seine, in the action of Marconi's Wireless Telegraph Company, Limited, *vs.* Societe Generale Des Transports Maritimes, Compagnie Radio electrique and Others, subject to correction should error appear."

The parties hereto have also entered into and filed herein a stipulation reading as follows:

[fol. 1630] "WHEREAS, a stipulation was entered into herein on July 7, 1925, appearing at pages 756 and 757 of the record herein, which stipulation provides that certain Letters Patent and certain publications offered by the defendant herein may be received in evidence as exhibits with the same force and effect as the originals thereof, and that they were published on the dates printed on said exhibits respectively or stated in the record at the time of the introduction thereof in evidence herein."

"Now, it is further stipulated and agreed that said provisions of said stipulation shall apply to copies of all patents and publications which have been or may be introduced herein by either the claimant or the defendant herein, subject, however, to correction if error should appear, and subject to objections as to relevancy or pertinency of the exhibits (other than on the grounds that they are copies or that the date of publication thereof had not been proved) which were or may be made at the time of the introduction of said exhibits."

Claimant's counsel offers in evidence certified copies from the United States Patent Office of three instruments in writing between claimant and Radio Corporation of America, one of which instruments bears no date, the other two

bearing date as of December 8, 1919, and the same are marked "Claimant's Exhibit No. 362".

It Is Hereby Stipulated that the foregoing certified copies of instruments may be used in evidence herein with the same force and effect as the originals, and it is further stipulated, for the purposes of this case only, and subject to correction should error appear, that the undated instrument was executed and delivered on November 20, 1919, by the claimant, and that the instruments dated November 8, 1919, were executed and delivered by the claimant on that date.

Claimant's counsel offers in evidence the following:

A printed copy of the report of the Committee on Military Affairs of the House of Representatives, on the Bill for "Settlement of Damages for Infringements of Radio Patents", which is to be marked "Claimant's Exhibit No. 363".

A copy of the final judgment in the case of *Marconi Wireless Telegraph Company of America* vs. *Kilbourne & Clark Manufacturing Company*, the same to be marked "Claimant's Exhibit No. 364".

(The Notary marks said exhibits as requested.)

(Stipulation of November 26, 1929, as to Atlantic Communication Company and DeForest Company Apparatus)

At the request of the claimant herein, it is hereby stipulated, subject to correction should error appear, as follows:

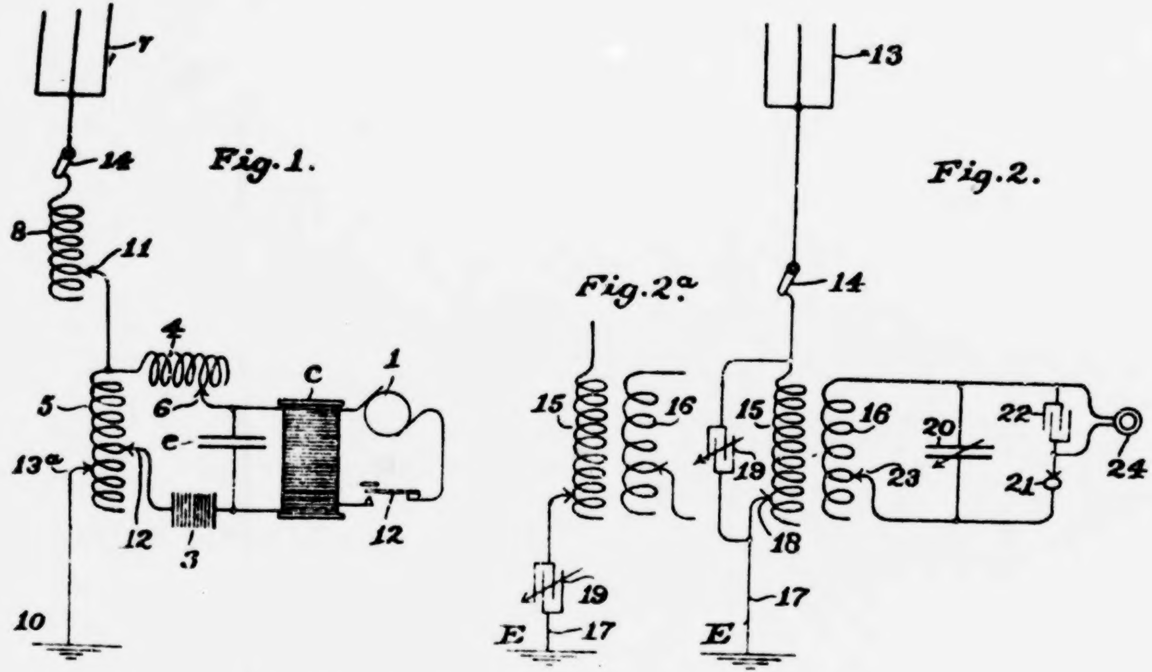
1. That an examination of the record in the Atlantic Communication case shows that the drawings hereto annexed marked "Waterman Exhibit B, Defendant's Sayville Apparatus, Atlantic Communication Case", and "Martin Drawing 'Herman Frasch' Set" represent, diagrammatically, the apparatus which was charged in said suit to be an infringement of the Lodge and Marconi Patents [fol. 1634] in suit herein, and the apparatus that is referred to in the order for preliminary injunction in the suit against the Atlantic Communication Company, *et al.*, heretofore offered in evidence herein as "Claimant's Exhibit No. 345".

2. That the transmitting apparatus represented in the said drawings "Waterman's Exhibit B, Defendant's Say-

ville Apparatus, Atlantic Communication Case" and "Martin Drawing 'Herman Frasch' Set" were the quenched gap type of transmitting apparatus.

3. That an examination of the record in the DeForest case shows that the drawing hereto annexed marked "DeForest System—S. S. Vesta" represents diagrammatically the apparatus which was charged in said suit to be an infringement of the Lodge and Marconi patents in suit herein and which apparatus is that referred to in the order for preliminary injunction in the said against the DeForest Radio Telephone & Telegraph Company, *et al.*, heretofore offered in evidence herein as "Claimant's Exhibit No. 346".

4. That the transmitting apparatus represented in the said drawing "DeForest System—S. S. Vesta" was the quenched gap type of transmitting apparatus.



Waterman Exhibit B Defendant's Sayville Station Apparatus
Atlantic Communication Case

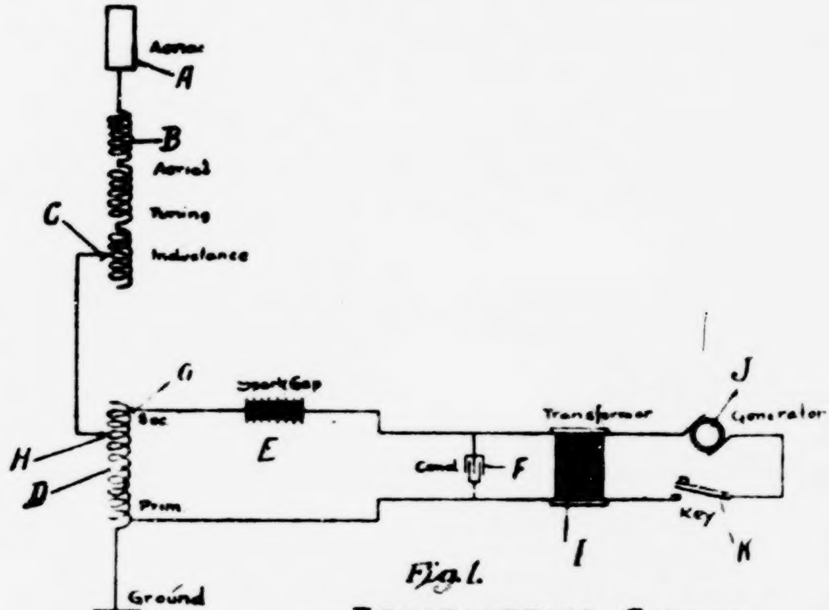


Fig. 1.

— TRANSMITTING CIRCUIT —

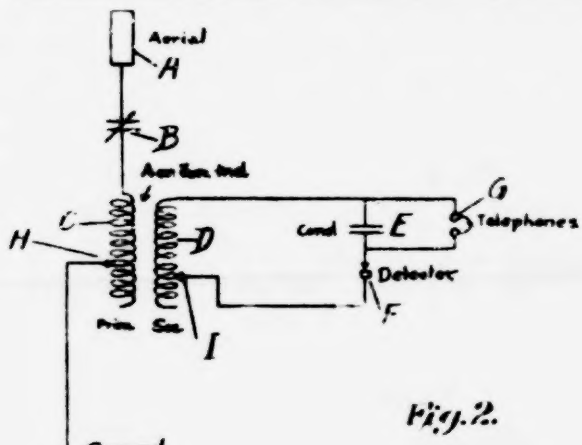
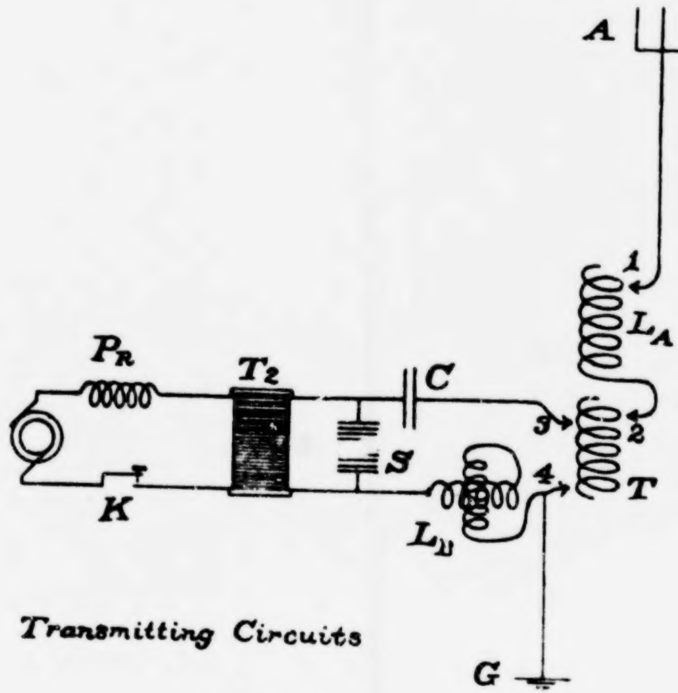


Fig. 2.

— RECEIVING CIRCUIT —

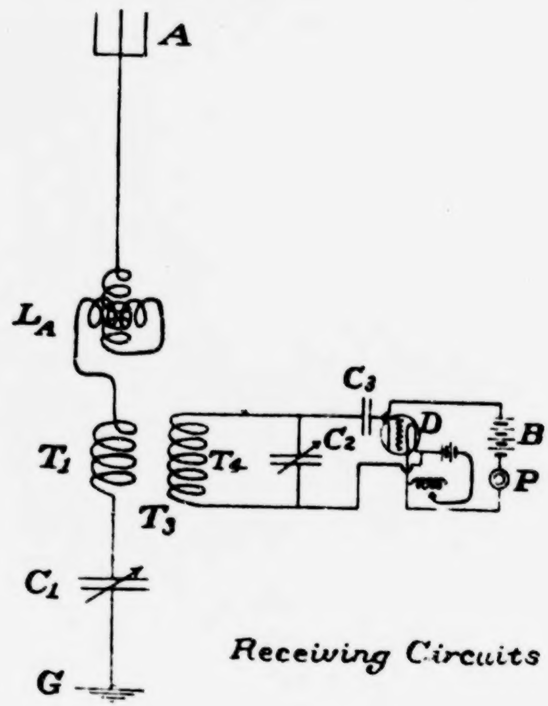
Martin Drawing "Herman Frasch" Set.

DeForest System-S.S. Vesta



Transmitting Circuits

Fig. 1.



Receiving Circuits

Fig. 2.

(Stipulation of December 10, 1929, Re Evidence Relating to Apparatus of General Electric Company on Issue of Compensation.)

Whereas, the claimant relies herein upon certain apparatus made by the General Electric Company and substantially similar apparatus made by others as infringements of one or more of said patents;

Now, Therefore, it is hereby stipulated, subject to the approval of this Court, that the evidence as to what particular apparatus made by the General Electric Company and purchased or used by the defendant, the Defendant's Exhibit A-7, an agreement between General Electric Company and the Marconi Wireless Telegraph Company, Claimant, of October 22, 1919, does or does not apply, and or evidence that the defendant has or has not guaranteed or agreed to assume all responsibility, indemnify or hold harmless the General Electric Company against any claim or demand in respect to the infringement of any of the patents in suit by the purchase or use by the defendant of any apparatus made by the General Electric Company, may be and hereby is postponed until the further order or direction of this Court and until after the hearing and determination of the issues of validity and infringement, and until after the determination as to whether or not said Defendant's Exhibit A-7 constitutes any release of the payment by the defendant to the claimant of reasonable compensation for damages and profits on any apparatus made by the General Electric Company and purchased or used by the defendant.

[fol. 1635] Court of Claims of the United States

No. 33,642

Marconi Wireless Telegraph Co. of America

v.

The United States

Order

It is ordered by the Court this 4th day of February, 1930, that this case be and it is hereby referred to Hayner H. Gordon, one of the commissioners of this court, for an ascertainment and report of the facts to the court.

The commissioner shall ascertain whether both parties have closed their proof; and if not, shall call upon either or both of them to produce witnesses within such reasonable time as the commissioner may prescribe. Where deemed necessary by him, he shall have the witnesses summoned. He shall fix the time and place for the taking of testimony, giving notice to either side thereof; and having concluded the taking of testimony he shall make a report of the facts to the court, filing with his report the evidence adduced by the parties. In the event both parties have concluded the taking of testimony at the time of this reference the commissioner shall proceed to ascertain the facts from the evidence on file, making his report, as already stated. Upon the filing of the commission's report the clerk shall mail notice to the parties of the filing of the same.

Each party may have 30 days from the date of the filing of the commissioner's report to file exceptions thereto. Each exception, at the end thereof, shall have appropriate references to the parts of the record relied upon for its support. The plaintiff may have 15 additional days from the date of the filing of exceptions within which to file briefs, and the defendant may have 15 days thereafter within which to file briefs.

The commissioner may rule upon the relevancy and admissibility of any evidence, any exceptions by either party to his rulings to be noted at the hearing by him.

All other questions are reserved.

It is further ordered that the former order referring the above-entitled case to a commission be and the same is vacated, set aside and withdrawn.

By the Court.

F. W. B., *Ch. J.*

*   *   *   *   *   *   *

George H. Clark, a witness recalled on behalf of the Claimant, having been previously sworn, testifies as follows:

Direct examination.

By Mr. Vaill:

Q. 213. Are you the same George H. Clark who has heretofore testified in this case?

A. Yes.

Q. 214. Will you look at the cabinet or apparatus which is on the table before you, and state if you know what it is?

A. I recognize this apparatus as a complete receiver in testing of two-circuit tuner, detector system, and associated control apparatus. It is a receiver of several types manufactured by the Wireless Specialty Apparatus Company and I personally have inspected and passed for acceptance and use in the United States Navy a large number of receivers of this class, during the time that I was employed as Expert by the Navy Department in Washington, D. C. I have used in service conditions a great many of these receivers on Naval vessels and at Shore Stations.

The receiver is of the IP76 type, and may be further described as belonging to a sub-classification "1912". The receiver known as the IP76-1912 was very commonly used in the Navy, and appeared in a number of slightly modified forms, none of which, in any way, changed the radio side of the apparatus, or its operation. The changes I recall were of the nature of substituting a key for a push-button in handling the test buzzer; the providing of a key-switch to control the battery supply to the potentiometer in some cases, and its omission in others; and the placing of the "send-receive" key in some cases on the right of the tuner, and in other cases on the left. Incidentally the sub-designation 1912 was more of an identification of type than a rigid date of manufacture, as I know that this type of receiver was in Naval use at least a year before 1912.

Q. 215. Will you please state how the apparatus before you corresponds with those which you worked and which you operated as concerns the type of detector or detectors used?

A. The receiver before me contains portions of two detectors, which I clearly recognize as the two standard ones furnished with this type of receiver. Either of the two detectors might be used, and a switch determined this. The left hand detector is the Perikon-Electra type, which originally had a single crystal on the movable or searching mem- [fol. 1637] ber, and a number of crystals embedded in the fixed member. I note that in this receiver before me the movable crystal has been replaced by an obviously home-made pointed piece of metal.

The right hand detector is the Pyron. It had a metal point on an adjustable steel spring for the one element, and a crystal held in an alloy for the other element. In the receiver before me, the crystal is not present. Both of the detectors I see before me have sufficient of their original material to enable me to identify them completely. These two types were purchased by the Navy Department in large quantities, and I passed very many of them for payment while I was on duty at the Washington Navy Yard, and I used both these types in actual work in the Navy.

Q. 216. How does the IP76 receiver before you compare with any of the IP76 receivers which you stated in your previous testimony were purchased and used by the Navy subsequent to June, 1910?

A. It is the same, except for obvious missing parts, with the receiver referred to in my testimony. I note, for example, that the rack normally furnished for moving the secondary coil back-and-forward, is present, but the pinion wheel for this purpose is missing. The switch for throwing the antenna loading coil in-and-out of service, is missing. Binding-post for attaching the detector battery, for the buzzer battery, and for the telephone receivers, are missing, though the holes in the hard rubber top for these still bear the markings indicating the use of the binding-posts that were once there. Some of the binding-posts still present are obviously not the type originally submitted with the instrument. None of these lacking devices make it difficult in any way to identify the instrument.

Mr. Vaill: The apparatus referred to by the witness in his previous answers made at this session is offered in evidence as "Claimant's Exhibit No. 366."

The Commissioner: Without objection, it is so accepted.

Q. 217. Will you please state whether or not a considerable number of these receivers similar to "Claimant's Exhibit No. 366" were purchased by the Navy subsequent to June, 1910, and prior to some subsequent date?

A. Up to the middle of 1919, at which time I left the Government service, a large number of these receivers had been purchased by the Navy, and were in use.

* * * * * * *

Q. 219. How does the circuit arrangement of the IP76 receiver before you compare with the circuit of the receiver illustrated in "Plaintiff's Exhibit No. 87", Wireless Specialty Apparatus type?

A. The circuits are identical, if it be understood that the device shown symbolically at B in the exhibit is a battery, and not a condenser. The symbol used in the drawing is universally understood as a condenser, but it is obvious to [fol. 1638] me that the device, shown connected to the potentiometer P, must be intended to represent a battery.

Q. 220. I hand you a photograph which I have had taken of the IP76 receiver before you, and to which you have, at my request, applied certain reference numerals and letters. Will you please designate the various controls and parts shown thereon by reference letters, and point out how these controls and parts relate to the diagram of the IP76 receiver shown on the right hand portion of "Plaintiff's Exhibit No. 87"?

A. Referring first to the letters and figures in the receiving diagram of "Plaintiff's Exhibit No. 87", and their reproduction on the photograph of the IP76 receiver, which is "Claimant's Exhibit No. 366", the antenna and its lead-in A on the drawing is not physically represented with the actual receiver, and on the photograph the binding-post to which the antenna was connected in practice, is marked "Antenna 'A' to here". In the same way the ground E on the drawing is shown as "Ground E to here" with an arrow pointing to the binding-post where the ground lead was connected in practice. The loading coil in the primary circuit of the diagram, g', is represented in the photograph by the same designation, and an arrow leading to one of the contacts which is connected by wire to the actual loading coil. The primary coupling coil marked j' in the diagram is shown by the same designation in the photograph, with two arrows leading respectively to one of the two

series of contacts connected by wires to the actual primary coil. One of these series of contacts varied turns, ten at a time, and the other series one at a time. The two knobs 5 and 6 in the diagram represent the knobs on the apparatus which control the two series of primary contacts just referred to, and these knobs are so numbered on the photograph. The secondary, movable coil, j^2 in the diagram, has that same designation on the clearly seen secondary coil of the photograph, and the contact 7 in the diagram for changing the number of active turns of that coil, is applied to the knob of the similar switch in the photograph. The variable condenser h', shown as a tuning condenser in the secondary circuit in the diagram, has the same designation in the photograph, with an arrow pointing to the handle provided for moving the condenser. The detector T in the diagram, has the same letter in the photograph, with one arrow pointing to the Perikon-Electra detector, and another arrow to the Pyron detector. The stopping condenser j^3 cannot be seen in the photograph, though it is present in the actual receiver which I have here, "Claimant's Exhibit No. 366". The telephone receiver R in the diagram, is represented by arrows running to the holes in the cover, where the telephone receiver binding-posts were ordinarily attached, and which is clearly marked "Tel." in "Claimant's Exhibit No. 366". The detector potentiometer P in the diagram, is indicated by an arrow pointing to the switch controlling that potentiometer, in the photograph. Finally, the battery B in the diagram is shown by two arrows pointing to the two holes in the receiver cover, where the binding-posts for attaching the external battery were located in practice. These binding-posts in "Exhibit 366" have the word "Bat." stamped between them, which I see clearly in the exhibit before me.

For additional clearness in pointing out parts of the receiver, a few further letters have been added to the photograph, which do not appear in the diagram. These are: the letter S has been added to indicate the switch for select-[fol. 1639] ing either one of the crystal detectors. The rack along which the coupling coil j^2 moves, has been lettered K, the two flexible leads from this coupling coil to the binding-posts connecting with the variable condenser h' are marked M, M. The knob of the secondary variable condenser is marked N.

Mr. Vaill: A copy of the photograph referred to by the witness in his preceding answer is offered in evidence as "Claimant's Exhibit No. 367".

The Commissioner: The same is accepted.

Q. 221. Your attention is directed to Fig. 111A, page 179 of "Plaintiff's Exhibit No. 100, Navy Manual of 1911", Fig. 101 on page 154 of "Plaintiff's Exhibit No. 78, Navy Manual of 1913", and Fig. 101 on page 155, also a part of "Plaintiff's Exhibit No. 78, Navy Manual of 1915". Will you please state what type of apparatus is illustrated in the figures just referred to?

A. All three show a form of the IP76 receiver, of the 1912 type. All three photographs represent receivers identical in characteristics with the receivers I have referred to in my previous testimony, and in my testimony today.

Mr. Vaill: The three Navy Manuals referred to in the previous question having already been offered in evidence, as parts of "Plaintiff's Exhibits No. 100 *and* No. 78", notice is given that such exhibits will be specifically referred to in connection with the testimony of the present witness at the hearing of this case.

* * * * * * *

Cross-examination.

By Mr. Edwards:

X Q. 222. When did you first see this "Claimant's Exhibit No. 366"?

A. About three weeks or a month ago.

X Q. 223. You have no personal knowledge of when or where it was made, have you?

A. No.

X Q. 224. So far as you have personal knowledge, the exhibit is merely typical of the IP76 receiver, such as diagrammatically indicated in "Plaintiff's Exhibit No. 87"; that is correct, is it not?

A. Yes.

* * * * * * *

Redirect examination.

By Mr. Vaill:

* * * * * * *

R. D. Q. 226. According to your knowledge of this apparatus, "Claimant's Exhibit No. 366", would you be able to say whether or not the appearance of this exhibit would indicate that it was one of the original apparatus manufactured at about the time you have testified to, that is, subsequent to June, 1910?

[fol. 1640] A. "Claimant's Exhibit No. 366" appears to me to be, without doubt, a relic of an operated receiver which has seen much service and abuse. I am strengthened in this belief by the fact that when I first saw this receiver it was in the usual carrying wooden case which was provided with all receivers of this type, and that I noted on this case a name-plate with the familiar insignia of the Wireless Specialty Apparatus, the name IP76 Receiver, and a serial number, which I recall as being less than 1,000. This at the time led me to believe that I was looking at one of the earliest receivers built by the Wireless Specialty Company of the IP76-1912 type.

Mr. Vaill: It is requested that the wooden case referred to by the witness in his last answer be considered as a part of "Claimant's Exhibit No. 366", and the same is offered as such.

The Commissioner: It is so accepted.

R. D. Q. 227. Will you kindly look at the label on the case which is now a part of "Claimant's Exhibit No. 366", and explain, if you can, what the serial number is to which you have referred?

A. The serial number is 152.

* * * * * * *

Deposition of Joseph O. Mauborgne (recalled), for defendants, taken at New York City on the 20th day of December, A. D. 1927

Direct examination.

By Clifton V. Edwards, Esq.

JOSEPH O. MAUBORGNE, being recalled as a witness on behalf of the defendant, testifies in answer to interrogatories as follows:

1. Question. Are you the same Joseph O. Mauborgne who has heretofore testified in this case?

Answer. I am.

* * * * * * *

[fol. 1643] 4. Question. Have you read the testimony of Mr. Roy A. Weagant, as given on printed pages 1526 to 1542 of the record in this case?

Answer. I have.

5. Question. Do you agree that Mr. Weagant succeeded in demonstrating the inherent capability of the 2-electrode tube to amplify and oscillate?

Answer. From the statement contained in the record made by Mr. Weagant, and from the description of the demonstrations made in connection with this case by Mr. Weagant, I do not agree that Mr. Weagant succeeded in demonstrating the inherent capability of the 2-electrode tube to amplify and oscillate. Mr. Weagant produced for these experiments 2-electrode tubes specially manufactured for the purpose of showing that such a 2-electrode tube with the addition of other apparatus outside of the tube and affecting the tube or its action could be made to amplify and oscillate in a circuit such as he used. That he made the tubes he used with the circuit arrangements he set up operate, I do not doubt, either as amplifiers or as oscillators. The arrangement he used was a poor equivalent of a 3-electrode tube, and he knew it to be such, for his testimony in various places practically admits the fact. These 2-electrode tubes had their filaments and plates separated to such an extent that it was possible by another part of the circuit which approached as closely as possible to the tube to cause the electron stream between the filament and the plate

to be electrostatically operated upon in such a way that amplification or oscillation resulted in the circuit. I might define this as a trick circuit not at all proposed by Fleming, Marconi, or anybody else but Mr. Weagant, who I understand has taken out patent applications for just such circuit arrangements, providing that the tubes used in such circuit are the special design enabling the control of the electron stream in the way which he demonstrated to the court. The device producing the electrostatic control was the equivalent of the grid of a 3-electrode tube, and in my opinion the patents taken out by Mr. Weagant covering these arrangements were designed to get around the 3-electrode tube, by producing the equivalent of the third electrode in such a way that it was not sold with the tube.

Had Mr. Weagant tried to make this demonstration, using the type of tube shown in the Fleming patent in the case where the plate of the 2-electrode tube shown completely surrounds the filament, he would have failed completely in his demonstration, in any opinion. He likewise would have failed with any other 2-electrode tube in practical use in the art, where the plate entirely surrounds the filament, for this effectively screens the space between the filament and the plate from the action of the third element which Mr. Weagant has brought as closely as possible to the tube in his experiments. I therefore must conclude that Mr. Weagant has not succeeded in demonstrating the inherent capability of a 2-electrode tube *per se* to amplify and oscillate.

* * * * * * *

[fol. 1644] 6. Question. In these tests of Mr. Weagant what was the effect of causing another part of the circuit to approach as closely as possible to the tube, and how does that action compare with the action of the 3-electrode tube?

Answer. The approach of the condenser in the one case or the coil of wire in the other case shown in the demonstrations was to exercise control electrostatically of the electron flow between the filament and the plate in such a way as to cause a reinforcement in the case of amplification, and, practically, if the coupling were close enough, and the space between the electrode wide enough, or of the proper width, to produce oscillation. The action was somewhat similar to that taking place in a regenerative circuit of ordinary type using the 3-electrode tube, and in

fact, as I stated previously, turned the arrangement into what might be said to be a poor 3-electrode tube arrangement with what corresponded to the grid outside the glass tube instead of being placed between the filament and the plate, as in the ordinary type of tube used in the art to-day.

7. Question. Please refer to Mr. Weagant's answer to question 154, at page 1557 of the printed record, and say if you agree with him that the Tesla system operates upon a different principle from that of the Marconi tuning patent 763772 here in suit.

Answer. I think that Mr. Weagant's answer does not make a comparison of the Tesla arrangement and the tuning arrangements of the Marconi patent 763772, but rather is concerned with the arrangements of Tesla as compared with those of Marconi given in the Marconi reissue patent 11913.

8. Question. Please refer to the answer to question 211 of Mr. Weagant's testimony, beginning on page 1643, and say if you agree with this view of the Tesla patent.

Answer. I do not agree with Mr. Weagant concerning his view of the Tesla patent. Mr. Weagant has quoted at least two paragraphs which to me indicate that he did have in mind the practicability of tuning the secondary or output circuit. He speaks of proportioning the wire in the secondary in such a way as to produce resonance between the primary circuit or the circuit next to the secondary, or what we might call the antenna circuit, if the wires used were of the right length, and uses the expression quoted by Mr Weagant on page 1645 of the record, "By such an adjustment or proportioning of the length of wire in the secondary coil or coils the points of highest potential are made to coincide with the elevated terminals D, D', and it should be understood that whatever length be given to the wires, this condition should be complied with in order to attain the best results." To anyone skilled in the radio art this means that the tuning of the antenna system, whether to a fundamental or the harmonic of an antenna in such a way that a potential loop is produced at the upper extremity of the antenna. To me this clearly means tuning, and I can see no difference between the tuning as he has described it, and as shown in the Marconi tuning patent 763772 here in the suit.

9. Question. Please refer to the testimony of Mr. John V. L. Hogan, on behalf of the claimant, particularly the

[fol. 1645] answers to questions 5 and 6 thereof, and to the testimony of Mr. Weagant in answer to question 208 at page 1636 of the printed record, and say if you agree with Mr. Weagant that the Lodge patent is inoperative to produce transfer of energy to the antenna with an impulsive rush.

Answer. I do not agree with Mr. Weagant that the Lodge circuit is inoperatve to produce transfer of energy to the antenna with an "impulsive rush." Mr. Weagant assumes that the spark gaps marked h^6 and h^7 of the Lodge patent No. 609154 "would have to cool so rapidly that they would be extinguished after the first half of the oscillation of the condenser circuit." I do not believe that this would have been necessary for the functioning of the circuit on the "impulsive rush" idea of transferring energy from the closed circuit to the open circuit of the transmitter. Mr. Weagant has just assumed that this is necessary. My understanding of Figure 4 of the patent quoted is that the spark gaps h^2 and h^3, as pointed out by Loftin in his direct testimony, might be removed and the gap closed, as disclosed by the inventor in the patent. I see no reason why the coil shown as k in said Figure 4 placed in shunt to the spark gap terminals of the transformer or induction coil or rather in shunt to the outer terminals of the Leyden jars whose inner terminals were connected to the spark gap mentioned, as shown in Figure 4, should not function as an ordinary inductance in an oscillating circuit and not as a short circuit, as Mr. Weagant has pointed out in his testimony. By the happy choice of the proper coil k and the inductances shown in the antenna circuit of Figure 4, I see no reason why Lodge should not have had in this device three coupled circuits tuned to approximately the same frequency. The circuit h^{10}, h^{11}, j, j, k, forming one of the circuits, the circuit k, h^8, h^7, h^1, h^4, h^6, and h^8, forming another circuit, corresponding to the lateral circuit which ordinarily supplies energy to an antenna circuit, and finally the antenna circuit itself h, h^4, and h^1. In order that the antenna circuit last mentioned might have a sufficiently low resistance so that the oscillations might not decay at too rapid a rate after this system had been energized by impulsive rush from the intermediate circuit, advantage could be taken of Lodge's proposal to short-circuit the gap h^2 and H^3. I am of the opinion that such a circuit could be made to operate very satisfactorily in the manner described

by the inventor in that it would supply energy by "impulsive rush" to the antenna circuit, and then, due to the cooling of the gaps h^6 and h^7, permit a more persistent oscillation in the antenna than that in the circuit immediately coupled thereto in which the oscillation would be comparatively rapidly damped out, due to the high resistance of the gaps h^6 and h^7.

10. Question. In a system such as shown in the Lodge Figure 1, would all of the energy be transferred from the charging circuit to the antenna in one swing or one alternation of the charging current?

Answer. I think not. The gaps of those days did not permit of such rapid quenching, but that is no reason why we should not accept Lodge's term of charging by impulsive rush, since, I believe, that by that he meant that there would be a comparatively rapid cessation of the oscillations in the charging circuit and a comparatively persistent set of oscillations maintained in the antenna circuit.

11. Question. Please refer to the testimony of Mr. Weagant in answer to question 200 at page 1623 of the printed [fol. 1646] record, and say if you agree with Mr. Weagant's testimony to the effect that there is a resonant transfer of energy in the defendant's quenched gap systems.

Answer. I do not agree with Mr. Weagant's testimony to the effect that there is a resonant transfer of energy in the defendant's quenched gap system, or any other quenched gap system. My direct testimony covered my viewpoint of how this is accomplished. (Refer to answers to questions 20 and 21, p. 1232 of the record.) Mr. Weagant apparently has attacked my testimony on the ground that I admit that there must be a certain amount of so-called tuning done in order to place the primary and secondary circuits in resonance with each other. He has also attacked my argument that if the frequencies of the lateral and antenna circuits of a quenched spark transmitter after having been carefully adjusted to radiate but one wave should be examined separately, that they would not be found to be identical, and states generally that the effect of coupling the antenna circuit to the lateral circuit would produce such a final result that if the measurement of frequencies which I indicated were made the results would not indicate the condition of affairs in either circuit when adjusted to give the greatest efficiency of transmission. I am willing now to admit that his statement is correct, but later I shall

be very glad to make use of this same statement in connection with a statement concerning the method of tuning a quenched gap transmitter, which I hope to make at a later time.

I maintain that there is not a resonant transfer of energy in a properly tuned quenched gap system affording the highest efficiency of transmission, as I stated in may direct testimony heretofore. When these circuits are tuned or rather adjusted to produce the optimum effect, notwithstanding the fact that one might follow the rule of first adjusting the primary circuit, then the secondary circuit, then the coupling and the gap, I am still satisfied that there is a difference in frequency between the primary circuit and the secondary circuit. In fact, as I have pointed out in my direct testimony, the difference between these frequencies is such as to cause beats which assist in the quenching action of the gap, and I am quite satisfied that if methods were found to examine the various frequencies in these different circuits that my statement would be verified. I will not repeat what I gave in my direct testimony, as what I consider the methods of transfer of energy from the lateral circuit to the antenna circuit. The reference to that testimony has already been given. I do not admit that my statement that the primary and secondary are more or less in tune is sufficient to justify the statement that I believe there is a resonant transfer of energy from the primary to the secondary circuit of a quenched gap system. Mr. Weagant has quoted me as saying that it is not necessary to adjust the product of L and C in each of these circuits of a quenched gap transmitter to be the same and quotes the file wrapper of the Seibt patent and the Manual of Radio Telephony and Telegraphy by Robison and Hooper, 1924 edition.

I should like at this time to state that the Manuals of the Navy Department and the Manuals of the War Department concerning the adjustment of various pieces of apparatus, including those of the quenched gap systems, were primarily designed for the use of practically unskilled technical personnel, and hence can not be quoted as containing [fol. 1647] an exact technical description of some of these circuits; for example, the quenched-gap circuit. I shall, perhaps, have occasion to refer to this matter again concerning certain data introduced by the witness, Mr. Cram.

* * * * * * *

12. Question. Please refer now to the testimony of Mr. Cram, beginning at page 1450 of the record, and say if Mr. Cram's testimony regarding the tuning of quenched gap or other sets is in accordance with practice in the Signal Corps.

Answer. I do not believe that the tuning of quenched-gap sets described by Mr. Cram represents the best practice of the Signal Corps. (I should have said, "I do not believe that the *method of* tuning quenched-gap sets described by Mr. Cram.") I am satisfied that the correctly tuned Signal Corps transmitter has a slight detuning as between the primary and the second circuits, which I have mentioned heretofore in my testimony.

I note that Mr. Cram has quoted some operation directions given by the Telefunken Co. concerning the method of operating the Telefunken wireless telegraph field wagon set supplied on Signal Corps Order No. 9648. I believe that I operated many times this same set, and that the operating instructions which were used by the Signal Corps men in charge of the set were identical with those given in the revised edition of Circular No. 1, Office of the Chief Signal Officer, 1914, revised under date of May, 1915, which are found on page 93. These instructions found in small type, I know, were copied directly from the Telefunken list of instructions which came with that set. At any rate, they were those under which the enlisted men operated and adjusted the apparatus. They follow:

"Transmitter and receiver: The connections of both are clearly shown in the drawing and require no further description.

"To adjust the transmitter for any wave length within the range of the set proceed as follows, assuming that the desired wave length is 1,000 meters:

"1. If it is intended to send at full power, adjust the voltage of the generator by means of the slide rheostat (at the left) to about 85 volts.

"2. If it is intended to send at less than full power, short circuit one or more of the gaps by means of the clips provided and at the same time reduce the generator voltage about 10 per cent per gap short circuited.

"3. Set the primary variometer (at the left) at the wave length desired, viz. 1,000.

"4. Put the aerial-coil plug (at the right) in hole No. 1, marked 680/1050. This adds sufficient inductance to the aerial to bring the final adjustment within range of the aerial variometer.

"5. Make the final adjustment with the aerial variometer (also on the right and on one side of the aerial coils) by turning it slowly up from zero until the ammeter in the aerial ground circuit indicates a maximum.

[fol. 1648] "6. The transmitter is now adjusted for the most efficient production and radiation of the wave length selected when used with the aerial and counterpoise supplied with the set."

* * * * * * *

There is a difference between these directions and the directions for operating quoted by Mr. Cram on page 1463 of the record, wherein paragraph 2 reads:

"This will result in a certain definite wave length or period in the primary circuit, and all that remains to be done is to put the aerial circuit in resonance with the primary circuit by means of the aerial variometer in (V)."

This paragraph does not appear in the list of instructions of the set actually used by troops wherein no instructions are given to put the aerial circuit in resonance with the primary circuit. I should like to point out that these instructions in Circular No. 1, referred to above, including all the data in small type on pages 92 and 93 of this circular were seen by me about the year 1910, and that after a number of tests in the field I wrote the additional instructions contained on pages 94, 95, 96, 97, 98, 99, 100, 101, and 102 of the circular mentioned, and they were sent to the Signal Office and published with the data which had been submitted by the Telefunken Co. These instructions give the idea which I have put forth in my direct testimony that there is some variation between the frequency of the primary circuit and that of the secondary, or at least that they permit of that interpretation.

As I have stated elsewhere in the testimony, that while as a matter of procedure one might set up a certain frequency in the primary circuit which is approximately that to which the set is to be tuned, that circuit if entirely un-

coupled from the secondary would not have the same frequency as if it were tightly coupled to the antenna circuit because of the relation of that circuit to it and the effect on the constants of the primary circuit. Hence, it can only be said that these instructions or other instructions which state that the operator shall first tune the primary circuit to the wave length which he desires to emit can only result in an approximate adjustment of frequency if we are to consider the effect of a coupled secondary circuit which must be adjusted thereafter, and which will therefore at such time react upon the frequency of the primary circuit. The operators are further instructed to make the final adjustment of the antenna circuit by the variation of the variometer in that circuit.

As I have pointed out, the instructions on page 83 of circular No. 1 omit any reference to the fact that, as stated by some, the secondary circuit should be placed in resonance with the primary, and justly so, for such is not the case. The secondary is adjusted to be in the neighborhood of the frequency of the primary and when a close coupling is obtained, such as is necessary to secure the proper transfer of energy from the primary circuit to the secondary circuit in such a way that there will be an abrupt cut-off of the oscillations in the primary circuit at the time when the oscillations in the secondary circuit have reached their maximum value, it will be found that there may be a still further difference in the frequency of the primary and the secondary circuits, even with reference to the frequency [fol. 1649] first established in the primary circuit of the transmitter, as a basis from which to start the adjustment. The adjustment of the gap then follows to get the clear note, and the ammeter in the antenna circuit should give a maximum reading when the adjustments have been properly made, but the fact that the ammeter does give a maximum reading does not at all mean that the products of the inductance and capacity in the primary circuit are exactly equal to the product of the inductance and capacity in the secondary circuit. In other words, as I have before stated, the operations in adjusting the apparatus are such that as a result there is not a resonant transfer of energy from the primary to the secondary circuits in the case of the quenched spark apparatus. The secondary circuit has a frequency somewhere in the vicinity of the frequency of the primary circuit.

By comparison the Marconi open-gap transmitter is quite a different arrangement. In the case of this apparatus it is absolutely essential that the frequencies of the primary and secondary circuits shall be carefully and separately adjusted to the same frequency and then coupled with the least practicable degree of coupling so that there may be constantly a resonant transfer of energy from the primary to the secondary.

Mr. Cram's statements concerning the adjustment of quenched-gap transmitters are, in my opinion, not in conformity with Signal Corps practice. In this connection I shall further refer to the fact that Mr. Cram himself wrote the instructions for the pack sets which are contained in circular No. 1 of the office of the chief signal officer, above referred to, and which are found on pages 107, 108, 109, and 110 of that document. From that it is seen that Mr. Cram had the erroneous idea that resonance must be obtained between the primary and secondary circuits of the quenched-gap transmitter, and he was further of the erroneous opinion that the ammeter indication of a maximum meant that such resonance had been obtained. I believe that a number of engineers will not support that view.

13. Question. What is the essential difference in operation between a tightly coupled quenched-gap set in which the charging and antenna circuits are tuned in the vicinity of each other, as contrasted with a plain gap loosely coupled set in which the two circuits are tuned in the vicinity of each other?

* * * * * * *

Answer. From my experience with both types of apparatus I should say that if the plain gap loosely coupled set were to have its two circuits tuned in the vicinity of each other, instead of being exactly in resonance with each other, that the result would be very inefficient transmission and the radiation of two humps or two frequencies would undoubtedly result; whereas, when, as I have stated, a quenched-gap set has its two circuits tightly coupled, the greatest efficiency is obtained and the radiation of a single wave length results when the coupling is exactly right.

(Defendant's counsel offers in evidence a copy of page 93 of the circular No. 1 from the office of the chief signal officer for 1915, referred to by the witness in answer to question 12. This publication is the same publication as

the publication Radio Telegraphy, heretofore referred to [fol. 1650] by claimant's witnesses Waterman and Cram. The Exhibit is marked "Defendant's Exhibit No. U-6.")

* * * * * * *

14. Question. Please refer to Mr. Hogan's answer to question 31 and say if you agree with this testimony.

Answer. This matter has been more or less covered in my comments upon some of the statements made by Mr. Weagant. I do not agree with Mr. Hogan that "This adjustment is most likely, if not certain, to bring the two circuits practically exactly in tune with each other so as to give the best resonant transfer of energy from the closed primary circuit to the open antenna circuit."

As to the suggestion about the measurement of the frequencies of the two circuits after the antenna and ground have been disconnected, I should like to state that I admit Mr. Hogan's objection to my method, but that I further suggest that it is perfectly feasible to measure one or the other circuits and find its frequency after both circuits have been adjusted and the correct degree of coupling attained, and I am quite certain that if this be done the differences in frequency which I maintain exist will be found to exist.

15. Question. Please refer to Mr. Hogan's testimony in answer to question 23 and to Mr. Weagant's testimony in answer to question 213 at printed page 1650, and say if you agree with the contentions of these witnesses to the effect that Stone did not describe a system having a lateral circuit and an antenna circuit tuned to the same frequency.

Answer. A perusal of Mr. Hogan's testimony seems to bring out the idea that the antenna system in no case was tuned to the frequency of the associated closed circuit. He further states:

"He (Stone) even gave the option of making the closed-circuit frequency the same as the fundamental of the aerial *wire* used, but he nowhere suggested that it could be the same as the natural or tuned frequency of the aerial system or circuit, *including both vertical wire and transformer coil.*"

I am satisfied that Mr. Hogan is not correct in this particular. Stone provided a transformer coil in the antenna circuit and the ground connection, and it is inconceivable

since the ground was connected, that he should mean to infer that the closed circuit might be tuned to the frequency of the vertical wire only, since this would be utterly senseless and of no use from a practical standpoint. It goes without saying that the coil, the vertical wire, and the ground must all have been connected together in practice. From this we see that Stone did not always intend to use "forced oscillations." He foresaw the actual tuning to resonance of the two related circuits, or rather provided for such tuning. I therefore must disagree with the contentions of the witnesses to the effect that Stone did not describe a system having a lateral circuit and an antenna circuit tuned to the same frequency.

* * * * * * *

[fol. 1651] Cross-examination.

By Mr. Vaill:

17. Cross-question. Are you still a commissioned officer in the United States Army?

Answer. Yes, sir.

18. Cross-question. What is your present rank?

Answer. Lieutenant colonel, Signal Corps.

* * * * * * *

[fol. 1652] 19. Cross-question. In answer to question 5 of your present deposition you referred to "a trick circuit not at all proposed by Fleming, Marconi, or anybody else but Mr. Weagant." Did you intend to mean or refer by use of the word "trick" that it was a deceptive or misleading circuit?

Answer. To the extent that it purported to show that it was an inherent characteristic of a 2-electrode tube in normal operation in a normal circuit, I do; and furthermore, I believe that Mr. Weagant was perfectly honest and perfectly within his rights in attempting to take out patents designed for the purpose which I have stated in the last sentence of the paragraph to which you refer.

* * * * * * *

20. Cross-question. By your last answer do you mean to infer that testimony of this type relating to the oscillation of a hard 2-electrode tube as testified to by Mr. Weagant and other witnesses for claimant amounted to an attempt to deceive the court?

Answer. To the extent that it was offered as evidence in rebuttal of the Government's claim, that a 2-electrode tube did not inherently possess the properties of amplification and oscillation, I do.

21. Cross-question. You agree, do you not, that in causing a high vacuum or hard 3-electrode vacuum tube to oscillate and amplify, that such a 3-electrode tube requires "The addition of other apparatus outside of the tube and affecting the tube or its action."

Answer. I do.

22. Cross-question. And in that respect, as stated in the last question, the action produced by these outside apparatus when amplifying or oscillating is substantially the same in the 2-electrode as well as the 3-electrode hard or high-vacuum tubes?

Answer. I do not. You are talking about two different sets of outside apparatus and they must not be confused.

23. Cross-question. Have you examined prior to your direct examination claimant's Exhibit No. 225, opposite page 1527 of the printed record, and introduced during Mr. Weagant's deposition?

Answer. I have.

24. Cross-question. In view of your reference to the Fleming patent on your direct examination, I assume that you examined that patent prior to your direct examination. Will you now kindly compare the circuit of Figure 1 of the Fleming patent 803684 and Exhibit 225, above referred to, and state wherein you consider that Mr. Weagant used connections or arrangements of apparatus which were not in substantial accord with the diagram of Figure 1 of the Fleming patent.

Answer. Exhibit 225 has a plate battery in circuit not shown in Figure 1 of Fleming. It has telephone receivers [fol. 1653] instead of the indicator *l* of Figure 1 of Fleming. The diagram of the tube is entirely dissimilar to the extent that the plate does not surround the filament, as in Fleming. This I consider as vital, and in the experiment which Mr. Weagant performed, and to which his Exhibit 225 purports to pertain, I find that he stated in the text that he has used a coupling between the condenser R and the plate of the tube *a* which does not show in the Exhibit 225.

25. Cross-question. You mean to infer from your last answer that a 3-electrode high-vacuum tube could be made to

oscillate without the use of a plate battery which you object to Mr. Weagant's having used for that purpose?

Answer. I did not mean to infer from my last answer that a 3-electrode high-vacuum tube could be made to oscillate without the use of a plate battery, nor did I make any objection in that answer to Mr. Weagant's having used such a battery for this purpose. You asked me to point out if there was any substantial difference between the Fleming diagram and Exhibit 225, and I believe I have pointed out substantial differences and also the fact that Exhibit 225 is not in accordance with the text concerning Mr. Weagant's demonstration.

26. Cross-question. Do you consider the use of a plate battery by Mr. Weagant, as indicated in claimant's Exhibit No. 225, one of the elements constituting what you have termed a "trick" circuit?

Answer. The addition of such a battery to the Fleming circuit was undoubtedly made for the purpose of assisting in the production of oscillation; the real trick, however, consisted in coupling back the condenser R to the tube in such a way, as Mr. Weagant himself has pointed out, as to control electrostatically the electron flow from filament to plate.

27. Cross-question. Then you admit that Mr. Weagant did not attempt to in any way conceal the fact that one end of the condenser was so placed as to effect the electron stream in the circuit of claimant's Exhibit No. 225, and therefore you used the word "trick" as being an expert manipulation without any idea of concealing its real action, did you not?

Answer. I think that Mr. Weagant's statements made it perfectly clear to any engineer just how he was bringing about the result which he obtained, but I do think that, since the court is ordinarily not composed of an engineer or engineers, the slight difference which amounted to the trick might not readily be understood by the court in its consideration of the testimony placed before it in written form.

* * * * * * *

28. Cross-question. Do you still deny that a hard or high vacuum 2-element vacuum tube having the filament and [fol. 1654] plate arranged as indicated in claimant's Ex-

hibit No. 225, when used with the addition of other apparatus outside of the tube and affecting the tube or its action, is inherently capable of oscillating or amplifying?

Answer. If arranged as shown in the diagram, claimant's Exhibit No. 225, with the inductances well removed from the 2-electrode tube, as shown in the diagram, and with the condenser marked R so removed, as shown in the diagram, I maintain that even the vacuum tube introduced by Mr. Weagant can not be made to amplify or oscillate. Furthermore, that a 2-electrode tube, even such as he used, which was specially constructed for the purpose of permitting an outside action upon it, is not inherently capable of amplifying or oscillating in normal 2-electrode circuits.

* * * * * * *

29. Cross-question. Following the thought expressed in your previous answer, do you deny that if what you have referred to in your direct examination as "the addition of other apparatus outside of the tube and affecting the tube or its action" includes the necessary correct position of what you have termed "the inductances"; do you still deny that such a tube, as indicated in claimant's Exhibit No. 225 by letters a, b, and c, is inherently capable of oscillating by virtue of the control of electron stream between the filament and plate?

Answer. I never denied that such a tube was capable of oscillating by virtue of the control of the electron stream between the filament and plate exercised by the application of electrostatic control applied from without the tube. The quality of oscillation or amplification is inherent in the circuit and not in the tube, in my opinion.

30. Cross-question. Do you mean to imply by the last answer that an oscillating circuit which would ordinarily be used with a three element or two element high vacuum tube would oscillate with the tube removed?

Answer. If supplied with energy from an outside source, it would.

31. Cross-question. Will you kindly take a piece of paper and draw a circuit such as you may have had in mind in making your last answer.

Answer. This di-gram which I have made and marked "Mauborgne sketch on cross-examination" illustrates a simple circuit from which the vacuum tube has been removed as shown by the dotted lines. The leads former.y

connected to the grid and plate terminals of the tube marked "G" and "P," respectively, have been connected by a wire. With the tube removed the circuit becomes a simple oscillatory circuit in which oscillations can be produced by the application of a high frequency from an outside source. In other words, the circuit reduces itself to a simple circuit having inductance and capacity in which oscillations will be set up and the presence of which can be determined by suitable means well known to the art. I did not intend to give the idea that the circuit of itself would be capable of the production of oscillations without the use of a vacuum tube and suitable means for coupling the plate and grid circuits.

[fol. 1655] 32. Cross-question. Have you ever tested a circuit like that illustrated in full lines in your sketch just made to ascertain whether it would oscillate and amplify like a vacuum tube in the circuits usually used with such tubes?

Answer. No; I have not, nor do I maintain that such a circuit will amplify or oscillate like a vacuum tube circuit using a vacuum tube and provided with proper arrangements for producing oscillation.

* * * * * * *

The Notary. Do you know any other matter or thing relative to the claim in question? If so, please state it.

Answer. I do not.

Mr. Vail. Counsel for claimant offers in evidence the sketch made by the witness as claimant's Exhibit No. 318, "Mauborgne sketch on cross-examination," and request that it be so marked by the notary.

Deposition of John M. Miller (recalled), for Defendants, Taken at New York City on the 2d to 11th days of October, A. D. 1928

Direct examination.

By Clifton V. Edwards, Esq.:

John M. Miller, being recalled as a witness on behalf of the defendant, testifies in answer to interrogatories as follows:

1. Question. Are you the same John M. Miller who has heretofore testified in this case?

Answer. I am.

2. Question. Have you read that part of the testimony of Roy A. Weagant, a witness for the claimant, in which he describes certain experiments which he considers to demonstrate that a 2-electrode vacuum tube is inherently capable of producing oscillations?

Answer. I have.

3. Question. Please consider the testimony of Mr. Weagant and say if you agree that Mr. Weagant demonstrated the capability of the 2-electrode vacuum tube to generate oscillations, and explain your reasons.

Answer. In my previous deposition in answer to question 4 (printed record, p. 758) I denied the statement that the 2-element valve, such as that of Edison or Fleming, possesses inherently the same capacity for generating radio waves as is possessed by the 3-element De Forest valve. I discussed the experiments of Mr. Weagant and Mr. Waterman before Judge Mayer, which were intended to prove that the 2-element device did have the capability. I pointed out that this demonstration was a "trick" demonstration—that with a critical amount of gas in the tube and with critical operating conditions the tube could be made to generate oscillations in a worthless manner.

By reason of this critical amount of residual gas and operating conditions the tube could be given a "falling characteristic," like that of the arc, and in a circuit similar to that of the arc could be made to generate unstable and varying oscillations. In doing this the mode of operation is totally different from the operation of the 3-electrode device of De Forest.

[fol. 1656] I also pointed out that Mr. Waterman's testimony relative to this demonstration was not in accordance with the facts. Mr. Waterman stated that his operation "does not in any way resemble an arc." As a matter of fact the fundamental mode of operation was entirely similar to the operation of an arc, and the circuits employed were similar to those employed with the arc. These facts relative to the demonstration before Judge Mayer are not contradicted in the rebuttal testimony of Mr. Weagant and Mr. Waterman. In fact the plea of ignorance is now entered and Mr. Weagant states in answer to question 255 (printed record, p. 1714):

"At the time that the demonstration was made to Judge Mayer, while we had succeeded in making the 2-electrode

tube oscillate, we did not know the mechanism which was responsible for the action."

And later.

"The fact that the 2-electrode tube possessed the falling characteristic, which is also possessed by the arc, was quite unknown to any of the witnesses for the plaintiff at that time."

It is evident, therefore, that Judge Mayer was not informed as to the facts concerning this demonstration, but was in reality misinformed by Mr. Waterman with respect to a very vital fact.

In the present case we have another "trick" demonstration by Mr. Weagant, purporting to show that a high-vacuum 2-electrode tube is capable of generating oscillations and of operation as an amplifier. These demonstrations are based upon nothing more than a quibble on words, and it is hard to believe that they would be advanced seriously. They appear to me to be more satisfactory as demonstrations that the 2-element device is incapable of such operation, for, in order to make these demonstrations, Mr. Weagant has added to the 2-electrode device a third element which is identical in functioning and made of operation to the control member or grid of the De Forest device. The addition of this control member, even though it is less effective and partially disguised, makes a 3-element device out of the 2-electrode device and permits the 3-element operation.

In simple terms, what Mr. Weagant has done in these demonstrations is to employ circuits customarily employed with the 3-electrode device. He has positioned the portions of these circuits, which are normally connected to the grid of the 3-electrode device, very close to the walls of a specially constructed 2-electrode tube. These portions of the circuit, which are normally connected to the grid of the 3-electrode tube, have the same electrical condition or potential as the grid. Since these portions of the circuit have the same electrical condition or potential that the grid would have the electrostatic field from these portions would be similar to that of the grid, and if they are positioned near the electron stream from plate to filament within the tube this electrostatic field will operate in a manner similar to that of the grid, but less effective.

For example, we could start with a usual 3-electrode tube circuit including a 3-electrode tube and could continuously shorten the lead connecting the grid of the tube to the circuit. In doing this the portions of the circuit connected to the grid, and which have the same electrical conditions as the grid, would be brought closer and closer to the tube. [fol. 1657] The electrostatic field from these portions of the circuit would be additive to the electrostatic field and cooperate with the electrostatic field of the grid. If the tube is specially constructed so that it is possible to bring these portions of the circuit very close to the electron stream within the tube it is possible that this additive and cooperating field can be made sufficient to operate by itself, in which case the connection to the grid could be cut out the grid itself dispensed with. However, although the functioning is not as effective, it still remains the same in character, and we still have a 3-element device operating in a 3-element circuit. The device which was demonstrated was not a 2-electrode device, but a 2-electrode device to which a controlling element had been added, making it a 3-element device entirely similar to the De Forest 3-element device. In fact the De Forest 3-element device with the grid might just as well have been employed in these demonstrations in so far as these demonstrations prove anything.

That such an additional control element was added to the 2-electrode tube is shown by Mr. Weagant's testimony in explanation of these demonstrations. For example, in connection with the oscillation demonstration (claimant's Exhibit No. 225) in answer to question 5 (printed record, p. 1528):

"One side of condenser R, namely, the side which is connected to inductance *m*, is close to the glass of the 2-electrode tube at a point midway between the 2-electrode. This physical association of the apparatus results in the electric field which exists on condenser R effecting the electron stream which passes between the hot electrode *a* and the cold plate *c*, giving thereby an electrostatic control of the electron flow. As the result of this control, this arrangement is able to oscillate, since small electromotive forces on this condenser can release relatively greater amounts of energy flowing between the plate *c* and the filament *a*."

Thus Mr. Weagant is employing in this demonstration an electrostatic control element in addition to the 2-electrode tube, and it is this electrostatic control element which De Forest first introduced into the 2-electrode tube of Edison. In passing, I would like to draw attention to Mr. Weagant's use of the term "control" in the above answer. The way he has used it here is in accordance with the usage of the art. It is the way Doctor Van der Bijl used it in the quotations that I have taken from him, and it is the way that I have used it in my deposition. I shall have occasion later to point out that in other places both Mr. Weagant and Mr. Waterman have misused this term "control," and have used it in a manner not in accordance with the art.

Proceeding now to the amplifier demonstration (claimant's Exhibit No. 227), Mr. Weagant states in explanation of this demonstration at printed record, page 1530:

"In the arrangement of the amplifier tubes in the apparatus which I have just demonstrated the 2-element vacuum tube is arranged so that the glass of the tube is close to one end of the winding of the input circuit. This is illustrated in the diagram which I have produced, where it will be noted that one end of coils L_3, L_5, and L_7 are close to the 2-electrode tube. This end of the coil is what is commonly referred to as the high potential end, and it is the electric [fol. 1658] field from this coil which influences the electron stream in the tube, with the result that the 2-electrode tube functions as an amplifier."

Here again it will be seen that Mr. Weagant has added an additional element to the 2-electrode device which he now calls an "influence" instead of "control," as in the preceding answer. As he states, it is this added "influence" or "control" which makes possible the functioning of the device as an amplifier. The device again is nothing other than the 3-element De Forest device.

Again, in connection with claimant's Exhibit No. 301 (printed record, opposite p. 1770), which is cited by Mr. Weagant as showing the possibility of using a 2-electrode tube in a regenerative circuit, his explanation shows that he has added a third element in this case likewise. Thus, in his explanation, Mr. Weagant states near the bottom of page 1770.

"In the actual arrangement of the apparatus the upper end of the coil j^2 is placed as closely as possible to the space between the cold plate P and the filament F. Under these circumstances, the electrostatic field existing at the terminus of this coil influences the electron stream in the valve, and this, in conjunction with the feed back action between the coils j^3 and j^2, results in the production of sustained oscillations."

The circuit which Mr. Weagant is employing is identical with the 3-electrode tube circuit of plaintiff's Exhibit No. 114, excepting that Mr. Weagant has located the upper end of the coil j^2 which is normally connected to the grid of the 3-electrode tube close to the glass wall of the tube and hence close to the electron stream between the plate and the filament. This portion of the circuit has the same electrical condition as the grid and, as I have explained above, the electrostatic field from this portion of the circuit can function as a sort of inferior grid or control element. Thus, again, Mr. Weagant is utilizing nothing different from a 3-element device in a 3-element circuit. As he states, it is the added "influence" or better "control" which makes the production of sustained oscillations possible. The justification for these experiments seems to lie in a contention by Mr. Weagant and Mr. Waterman that there is a "control" and a "controlling action" in the 2-electrode tube. This is not at all correct. I have pointed out that Mr. Weagant, perhaps unwittingly, used the term "control" in its correct technical meaning and in accordance with the art of explaining his oscillation demonstration. Mr. Weagant and Mr. Waterman have used the same term "control" in connection with the 2-electrode tube and its operation where it does not apply. In the sense in which this term is used in connection with a 2-electrode tube it is broad enough to cover practically any case of the flow of electrical current in the electrical art. Such a loose use of a technical term, which has a definite meaning in the art, leads to confusion and can not be tolerated in a discussion of matters of invention where it is necessary to distinguish between different devices and different modes of functioning. The way Mr. Weagant and Mr. Waterman have used the term "control" in connection with the 2-electrode tube would enable this same term to be employed in connection with almost any electrical process. As a matter of fact, the

term "control" has a very definite meaning in the art, and the meaning corresponds with Mr. Weagant's use of the [fol. 1659] term in connection with his explanation of the oscillation demonstration.' The loose use of the term which follows can only be for the purpose of introducing confusion into the discussion of two devices which are different and have different modes of functioning.

The question naturally arises that if there is a "control" or "controlling action" in the 2-electrode device, why did Mr. Weagant have to add a control identical with that of the De Forest grid in order to make his demonstrations of oscillation generation and amplification? The answer is: There is no control in the 2-electrode device, and it was De Forest who first introduced a control and controlling action into that device. The Weagant demonstrations are, therefore, nothing other than demonstrations that the De Forest 3-element vacuum tube is capable of oscillation generation and amplification.

It is of interest to point out the things which Mr. Weagant did in making his demonstrations which have no foundation in the disclosure of the Fleming patent. First, Mr. Weagant used a special form of 2-electrode tube, differing widely from that disclosed in the drawings of the Fleming patent. Despite the fact that Mr. Weagant has taken out a number of patents (cross-question 26, printed record, p. 1539) concerned with an "outside control element or electrode," which is a modification of the De Forest device, and in fact similar to the device employed in his demonstrations, Mr. Weagant specified a variety of constructions (cross-questions 104, 105, printed record, p. 1537) in order to obtain one suitable for his purpose. In his answer to question 9 (printed record, 1529) Mr. Weagant describes the special form of 2-electrode tube which he used in these demonstrations, and it is interesting to compare this tube with the tube shown in Figure 1 of the Fleming disclosure. Thus the tube used by Mr. Weagant was long and narrow, approximately 1 inch in diameter and 4 inches long. The tube of Figure 1 of the Fleming disclosure in contrast is a short, thick tube. In the Weagant tube the plate element was a circular disk of metal slightly smaller in diameter than the inside of the glass tube, and spaced at a distance along the axis of the tube from the filament. In the Fleming disclosure the plate element was a cylinder of metal

completely surrounding the filament and spaced a considerable distance from the glass walls of the tube. Thus it is evident that the special tube employed by Mr. Weagant is so constructed as to make it easy to get an "outside control element or electrode" close to the space between the filament and plate inside of the tube which is carrying the electron stream. The tube shown in the Fleming patent is totally unsuited for this purpose. First, even if the external control element is located close to the glass wall of the tube, it would not be near the space between filament and plate which is carrying the electron stream, and furthermore the cylindrical metal plate surrounding the filament which Fleming employed would operate as a "shield" to prevent the electrostatic field reaching the space between the filament and the plate. Second, Mr. Weagant located particular portions of the apparatus close to the glass walls of the tube in order to obtain his result. Other portions of the apparatus would be neutral or have no effect, and in the case of oscillation generation might even act to oppose the generation of oscillation. There is, of course, nothing in the Fleming disclosure telling one to locate any portion [fol. 1660] of the apparatus close to the glass wall of the tube, and of course there is nothing instructing one to locate particular portions of the apparatus close to the tube. Third, the battery P of claimant's Exhibit No. 225, or the battery marked "180" at the right of Exhibit 227, is not shown in the Fleming disclosure, and in fact, as I will show, the use of such a battery in the Fleming circuit would be detrimental to the operation of the device, as Fleming described it. These batteries are the "B" battery of the De Forest 3-element device and are totally different in magnitude and in purpose from the so-called "biasing" battery which had previously been employed in some cases with detectors. The voltage of these batteries was very high (180 volts, cross-question 74, printed record, p. 1535-1536). This is even higher than the usual voltage of 115 volts, which is employed by power companies for power, lighting, and heating purposes. This voltage is the characteristic "B" battery voltage of De Forest, which is essential with an amplifying device. The so-called "biasing" voltage employed with detectors was normally of a few volts or a few tenths of a volt. As I have pointed out in my direct examination, in answer to question 18 (printed record, p. 791), the use of such a "biasing" voltage with

a detector may serve to make the detector more sensitive, but its use is optional and not necessarily required. The use of the De Forest "B" battery with the 3-element amplifier is indispensable.

These two uses of batteries for different purposes are well illustrated in Mr. Weagant's amplification demonstration, of which the circuit diagram is claimant's Exhibit No. 227. The battery marked "180" at the right of the figure is the "B" battery, and it is to be noted that this battery is connected through the coils L_2, L_4, L_6 to the plate elements of the vacuum tubes V_1, V_2, and V_3. These are the tubes which Mr. Weagant states are operating as amplifiers, and they are also provided with external control elements, viz., the coils L_1, L_3, and L_5 of which the high potential terminals are located close to the glass walls of the tube. In other words, these amplifying tubes have the De Forest "B" battery and have the third control element of the De Forest 3-element device. The tube V_4 is stated by Mr. Weagant to be the detector. The "B" battery is not connected to the plate of this tube and neither is this tube provided with a control element. The plate element of this tube V_4 is connected through coil L_7 and the indicating devices T_1 and T_2 back to one terminal of the filament, a circuit very similar to that of the Fleming disclosure. Instead, however, of the return of this circuit being connected to the negative terminal of the filament, as in the Fleming disclosure, the return in this case is to the positive end of the filament. This amounts to including in this circuit a small biasing voltage amounting to the drop-in voltage in the filament of the tube, or about 4 volts. This connection probably improves the operation of the tube V_4 as a detector over the connection where the return is made to the negative end of the filament without a biasing voltage.

[fol. 1661] As is shown by Mr. Waterman's drawing No. 6, plaintiff's Exhibit No. 323, which I will discuss later, the inclusion of a battery such as this 180-volt "B" battery in the circuit of the tube V_4 or in the circuit of the Fleming disclosure would be ruinous to the desired operation of the device as a detector. Thus Mr. Weagant's own diagram of his amplifier demonstration is a nice illustration of the difference in magnitude and functioning of the "B" battery as employed with an amplifier and a biasing battery as employed with a detector.

Fourth, in connection with Mr. Weagant's oscillation demonstration, there is also another particular feature which does not originate in the Fleming disclosure. In order for oscillations to be obtained with the circuit of claimant's Exhibit No. 225 and in accordance with Mr. Weagant's explanation, it is necessary that there be definite connections between the coil system *m, k*, and the rest of the system and a definite sense of winding of the coils *m, k*. Thus if the connections to either coil *m* or coil *k* were reversed, or if the sense of winding of either of these coils were reversed, the action would be such as to oppose the generation of oscillations instead of favoring them. In order that oscillations can be generated it is necessary that the connections and sense of winding of the coils *m, k* be such that during an oscillation the upper part of coil *m* is positive in polarity at the same instant that the upper end of coil *k* is negative in polarity. This is a fact well known in connection with 3-element vacuum tube operation, and in fact this demonstration is nothing but a demonstration of a 3-element device. I find no instructions in the Fleming disclosure specific as to the connections or sense of winding of the similarly lettered coil system in that disclosure. In fact there is no reason why there should be such instructions in this disclosure, for it has no effect upon the operation of the Fleming device as a detector.

4. Question. Do you agree with the testimony of Mr. Weagant (R. 1705) to the effect that the grid added no new mode of operation to the 2-electrode tube?

Answer. I do not agree with Mr. Weagant's statement to the effect that the grid added no new mode of operation to the 2-electrode tube, and excepting a possible quibble on words the statement is contrary to facts. The term "grid" is symbolical of a control element which De Forest was the first to employ with a 2-electrode device. By a control element I mean an element which by the effect of its electrostatic field and without requiring or consuming appreciable energy, is capable of controlling the electron stream between the plate and filament elements of the tube. Without this control element the 2-electrode device is only capable of operating as a rectifier. With this control element the 3-element device is capable of operation as an amplifier. Thus the 3-element device has an entirely new mode of operation from that of a 2-electrode tube.

The term "grid" carries with it more significance than merely a structure. In De Forest's earliest patent, showing the control element (Patent No. 841,387, claimant's Exhibit [fol. 1662] No. 270) this control element takes the form of a plate (D' of fig. 2). A little later (patent No. 879,532, defendant's Exhibit K) De Forest showed the control element as a grid-like structure interposed between the filament and the plate. In Mr. Weagant's patents, mentioned on page 1539 of the record, and considerably later in date, the control element takes the form of a plate surrounding the glass wall of the tube. In Mr. Weagant's demonstrations the control element is likewise located outside of the tube and takes the form of the metal of a condenser or the end of a coil. Of all of these forms of control element, the grid control element is most effective, and the term "grid" is therefore synonymous with a control element.

All of these forms of control element operate the same, the only difference being in the degree of effectiveness. De Forest was the first to have introduced the control element and to have obtained the new mode of operation resulting therefrom, and excepting for a quibble on words, Mr. Weagant's statement is contrary to the facts.

5. Question. Will you refer to the testimony of Mr. Weagant, at record 1706, relative to the 3-electrode tube having a falling characteristic, and say to what extent you agree with Mr. Weagant?

Answer. Mr. Weagant was asked to state whether and under what conditions a high-vacuum 2-electrode tube involves an action having a falling characteristic. I have shown that the characteristics of a high-vacuum 2-electrode tube are always rising characteristics, and hence the answer is that a high-vacuum 2-electrode tube never involves an action having a falling characteristic. Mr. Weagant, in the first part of his answer, states that he has demonstrated that a high-vacuum 2-electrode tube will generate oscillations, and in the remainder of his answer deals with 3-electrode tubes. As I have pointed out, Mr. Weagant's demonstrations did not employ a high-vacuum 2-electrode tube, but did employ a 2-electrode tube with an added control element or 3-element device, so that Mr. Weagant's answer deals throughout with 3-element devices and operation. Apparently his answer is intended to show that

3-element devices can show a falling characteristic and operate by reason of a falling characteristic.

The De Forest device in this issue does not have a falling characteristic, and does not operate by reason of a falling characteristic, but operates with a rising characteristic by reason of the controlling action of the third element. Any device, whether it has two electrodes or three, if it operates by reason of a falling characteristic, is a different device from the DeForest device employed by defendant.

Mr. Weagant refers to a 3-element device of a patent to White, No. 1393594, and to another 3-element device, called the "dynatron," described in an article by Doctor Hull, of the General Electric Co. Mr. Weagant states that both of these devices are 3-element devices which show falling characteristics and operate by reason of these falling characteristics.

It is a fact that the devices shown in these references do operate by reason of a falling charactertistic, and it is also a fact that they are fundamentally different in operation [fol. 1663] from the De Forest device in this issue—a fact which Mr. Weagant's references themselves are sufficient to show.

First, let us consider the White patent. Mr. Weagant did not quote enough from Mr. White's disclosure. Thus, starting at line 16, page 1, Mr. White states:

> "Electron discharge devices of the type comprising an evacuated container inclosing a filamentary cathode, a plate-shaped anode, and a grid, interposed between cathode and anode, are well known. Various connections have also been employed with such devices to cause them to operate to produce high-frequency oscillations. In all of the connections which have previously been used, however, so far as I am aware, the same fundamental principle has been employed; that is, a portion of the amplified energy in the plate circuit of the device has been fed back to the grid circuit by some form of coupling so as to make the system self-exciting."

This prior use to which Mr. White refers in this paragraph is the use of the DeForest device as an oscillation generator as employed by defendant and as I have explained the operation in my direct examination. Mr. White then proceeds to explain his invention, and continues:

"In carrying my invention into effect, however, I employ a materially different principle. I have found that under certain conditions with devices of this type the current in the grid circuit may have a dropping characteristic; that is, as the voltage impressed upon the grid increases the current in the grid circuit will decrease."

Thus Mr. White himself points out that by employing a 3-electrode device which operates by reason of a "dropping characteristic' he is employing a "materially different principle" from that of defendant's De Forest device. As a matter of fact this "materially different principle" of operation results from the ionization of residual gas in the vacuum tube, which leads to this "dropping characteristic," and is quite similar to the "gas" demonstration before Judge Mayer, which was likewise an operation which depended upon ionization of residual gas and a falling characteristic. Mr. Weagant states that the tube employed by Mr. White had a very good vacuum, and he finds in an obscure corner of the patent that Mr. White employed a plate voltage of 375 volts, which indicated that the vacuum was good. However, Mr. Weagant passed over Mr. White's statement on page 2, line 7, as follows:

"This characteristic of the current curve is probably due to the effect of slight traces of residual gas in the device and the consequent positive ionization. The greater the negative grid voltage the lower the plate current, and therefore the lower the amount of positive ionization in the device due to residual gas therein."

Thus Mr. White points out that the different principle which he is employing to generate oscillations depends upon gas ionization just the same as the gas demonstration before Judge Mayer depended upon gas ionization. This gas characteristic, which Mr. White employed, is quite well known and well understood, and anyone at all conversant with vacuum tube devices should be familiar with this whether Mr. White pointed it out or not. In fact this very characteristic which Mr. White employs for oscillation generation is the basis of a widely used test to determine when [fol. 1664] the vacuum in a 3-electrode tube is defective. Thus this very characteristic has been employed by the United States Signal Corps and by the United States Navy as an acceptance test for 3-electrode vacuum tubes, and

these tubes are accepted on the basis that this gas ionization characteristic shall be absent to an extent that it will be negligible in the operation of the tubes under normal operating conditions. Thus Mr. Weagant's own reference shows that the White 3-electrode device, in so far as it depends upon this dropping characteristic, is a totally different device from the De Forest device in this issue.

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Mr. Weagant then refers to the "dynatron" of Doctor Hull as being another 3-electrode device having a falling characteristic. This device is described in a paper by Doctor Hull in the Proceedings of the Institute of Radio Engineers, which is claimant's Exhibit No. 262. The "dynatron" of Doctor Hull is a 3-electrode device which has a falling characteristic and operates by reason of this falling characteristic. The characteristic in this device does not depend upon gas action, but upon a phenomena called "secondary emission." Since the device is a high-vacuum device it is thoroughly dependable in operation and probably would have wide use excepting for the prior De Forest device over which it has no advantage and some practical disadvantages. Its mode of operation is entirely different from that of the De Forest device and depends, as I have said, upon what is called "secondary emission" of electrons. An electron moving at a very high velocity and striking a metal plate can dislodge one or more electrons from the metal plate, and this is called "secondary emission."

In the "dynatron" this effect leads to a falling characteristic like that of the arc, but not due to the ionization of a gas, as in the arc. By reason of this falling characteristic, the "dynatron" can operate as an amplifier, and in a circuit resembling that of the arc can operate as an oscillation generator.

The device has a filament which emits what we will call "primary electrons," a gridlike structure maintained at a high positive voltage, and which operates to give these "primary electrons," a gridlike structure maintained at a a plate maintained at a lower positive voltage which is bombarded by the high-speed primary electrons and emits secondary electrons as a result of the bombardment. The gridlike electrode also operates as a collector for these secondary electrons emitted by the plate. The net result of this action is to give the plate circuit of the device a fall-

ing characteristic, and excepting for the action of the gridlike element in giving the plate circuit this falling characteristic the gridlike structure does not enter further into the operation of the device. In so far as connections are made between the outside circuits for utilizing the device these connections are made solely between the plate electrode and the filament electrode, and therefore in its utilization it is practically a 2-electrode device possessing the falling characteristic. The circuit employed with the device is likewise similar to that employed with the arc or other 2-element device possessing a falling characteristic. [fol. 1665] The grid electrode of the De Forest device is never employed with a steady high positive voltage in practice; usually it is made negative instead. It is evident that the "dynatron" is an entirely different device, that it operates in a totally different manner and on a totally different principle.

While Mr. Weagant refers to Doctor Hall's paper on the "dynatron," claimant's Exhibit No. 262, he neglects to quote a part of this paper which is very important, for Doctor Hull very definitely points out that the 2-electrode rectifier, the De Forest 3-electrode device, and the "dynatron" are three devices fundamentally different in principle and operation. In this paper Doctor Hull makes use of the General Electric Co. trade names "Kenotron" rectifier for the high-vacuum Edison device, "Pliotron" for the high-vacuum De Forest device, and "Dynatron" for his own device. On page 5 of this reference Doctor Hull states:

"In construction the dynatron resembles the kenotron rectifier and the pliotron. In principle and operation, however, the three are fundamentally different. Each utilizes a single important principle of vacuum condition. The kenotron rectifier utilizes the unidirectional property of the current between a hot and cold electrode in vacuum. The pliotron utilizes the space charge property of this current, which allows the current to be controlled by the electrostatic effect of a grid. The dynatron utilizes the secondary emission of electrons by a plate upon which the primary electrons fall."

Thus the dynatron, though a high-vacuum device, is similar to the "gas" tube demonstrated before Judge

Mayer in that both have falling characteristics, and by reason of the falling characteristics can operate as amplifiers or oscillation generators. The high-vacuum 2-electrode tube is only a rectifier. The De Forest device is fundamentally different in principle and operation from all of these. It will be noted that while Mr. Weagant and Mr. Waterman have misused the term "control" time after time in discussing the 2-electrode tube, Doctor Hull does not use this term in connection with the 2-electrode tube or the dynatron, but in accordance with my use and that of the art reserves it for the functioning of the grid electrode of the De Forest device.

I have previously quoted in several places from Doctor Van der Bijl's book on vacuum tubes, which quotations show that he also uses this term "control" as a distinguishing term for the operation of the grid electrode in the De Forest device. Thus it appears that Mr. Weagant and Mr. Waterman differ from the art both in respect to the use of the technical terms employed in describing the operation of these devices and also with respect to the fundamental principles which underlie the operation of these various devices.

This matter of distinguishing between a device operating by reason of a falling characteristic and the De Forest device, which operates by reason of a controlling grid, is a proposition which, as I pointed out in my direct examination at question 5 (printed record, p. 763) came up before Judge Mayer at a later date in the case of Hewitt *v.* American Telephone & Telegraph Co. In this later case Judge Mayer was fully informed as to the fact that the Hewitt device operated by reason of a falling characteristic, and he recognized the fundamental difference in principle of operation of such a device and of the De Forest device.

[fol. 1666] The remainder of this answer of Mr. Weagant deals with the matter of "negative resistance reaction" and "alternating current negative characteristics." These terms, which I will show, are of no significance in differentiating as to the fundamental principle of operation of these various devices. Thus (printed record, p. 1701), Mr. Weagant states that we must consider both the device and its circuits, and that when we do this the various systems can show what he calls "negative resistance reaction" or "alternating current negative characteristics," as proven by his experiments on the circuit of Weagant sketch No. 4.

or the patent No. 1334165 to Pupin and Armstrong. Just what his point is here is not very clear. These terms mean nothing more than that a device with its circuits which shows these effects is capable of approaching the oscillating condition or of operation as an oscillator. They do not distinguish at all with respect to the fundamental principle of operation which makes this possible. The high vacuum 2-electrode device which can not either approach the oscillating condition or function as an oscillation generator is likewise incapable of showing "negative resistance reaction" or "alternating current negative characteristics." The "gassy" 2-electrode device, as demonstrated before Judge Mayer and which operates by reason of a falling characteristic, can show this "negative resistance reaction" and "alternating current negative characteristics." The 3-electrode device of De Forest, which operates by reason of the added control element, can also show this "negative resistance reaction," or "alternating current negative characteristics." As I have stated above, these terms mean nothing more than that the particular device with its circuits can approach the oscillating condition or operate as an oscillation generator. It is conceivable that we might have a dozen such devices, such as the arc, gassy 2-electrode tube, dynatron and De Forest 3-element device, all of which are capable of approaching the oscillating condition but do so with wide differences in the fundamental mode of operation. Therefore these terms employed by Mr. Weagant have no significance in differentiating between devices which are totally different in operation.

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6. Question. In Mr. Waterman's answer to question 14 (printed record, p. 1919) he criticizes your explanation of the fundamentals on which the operation of these vacuum devices rests and introduces Waterman's drawings 5 and 6 to illustrate correct comparisions of the 2 and 3 electrode devices. Do you agree with this testimony?

Answer. Mr. Waterman prefaces his answer to this question by stating that the testimony of Doctor Millikan and myself was given in 1923, the Fleming patent, applied for in 1905, and the De Forest grid patent in 1907, and that we were entirely unable to get back to the fundamental basis on which these structures rest. Mr. Waterman states that he will endeavor to get back to the fundamentals in-

volved which constituted the great disclosure of Fleming on which the whole radio art of vacuum tube devices rests. I have discussed these fundamental matters in my answer to question 28 (printed record, p. 823). I showed there [fol. 1667] that the 2-electrode device goes back to Edison in 1883, who discovered the property of unilateral conduction possessed by that device. I also pointed out that prior to the Fleming disclosure, the 2-electrode device was a well-known rectifier of alternating currents to Howell, Wehnelt, and Fleming himself. I also pointed out that prior to the Fleming disclosure, and through the work of J. J. Thomson, O. W. Richardson, and others, the fundamental features underlying the operation of this device were well understood. So that the knowledge necessary for Doctor Millikan and myself to discuss the operation of the 2-electrode device of the Fleming disclosure was all available prior to the date of that disclosure. It is true that subsequent to 1907 much knowledge has been added with respect to the 3-element device of De Forest. Such knowledge, however, merely insures that Doctor Millikan and myself could speak as to what the operation of the De Forest device must have been in 1907 with decided confidence.

Now let us look at the uses and development of the two devices, the 2-element and the 3-element, in the electrical art, and see if the whole radio art of vacuum tubes rests upon the "great discovery of Fleming." As I have pointed out, the 2-electrode device was a well-known rectifier of alternating current prior to the Fleming disclosure. Two electrode tubes have been continuously employed as rectifiers of low-frequency alternating currents, and at the present date are widely used as such in the electrical art and specifically in connection with radio signals. They are not used as radio detectors, but the primary use is to rectify low-frequency alternating currents to supply direct current power to radio receiving sets. The old 2-electrode tube is used to some extent but more frequently it is a type of double 2-electrode tube in one evacuated inclosure; that is, the single-plate electrode of the earlier structure has been divided into two parts, each of these parts operating as separate rectifiers in exactly the same manner as the single plate element of the old device. It is called a double wave or biphase rectifier, it serves to rectify both halves or alternations of the current wave,

and is similar in result to what Fleming obtained with two separate rectifying devices in his Figure 2 of the Fleming disclosure, or what Valbreuze similarly obtained by the use of two separate rectifying devices in his prior French patent No. 328687, which I discussed in my direct examination.

Thus the use of a 2-electrode Edison device as a rectifier of alternating currents, in particular of low frequency, which started prior to the Fleming disclosure, still continues up to the present day. What Fleming did in his disclosure was to substitute this well-known rectifier of alternating currents for another well-known rectifier of alternating currents in the circuit of the prior Valbreuze patent. The result is the same in both cases; that is, the device functions as a rectifier of high frequency alternating currents, or a detector. This, I pointed out in answer to this same question 28 (printed record, p. 826) and illustrated in Miller drawing M, opposite page 826. I also pointed out that this was an obvious step, that sufficient exact information was available prior to the Fleming disclosure to predict that the 2-electrode Edison device would operate just the same as a rectifier of alternating currents, whether the frequency be high or low. I have also pointed out that [fol. 1668] this step was also a failure as far as practical results are concerned, and that the use of the 2-electrode device as a rectifier of high-frequency alternating currents or detector made no impression upon the radio art. The art has used, and continues to use, the 2-element device as a low-frequency rectifier, whereas the Fleming use has proven to be an insignificant offshoot of this low-frequency use.

The De Forest 3-element device likewise originated and had its first wide use as an amplifier of low-frequency alternating currents. Thus the earlier De Forest patent, No. 841387 (claimant's Exhibit No. 270), to which Mr. Waterman has referred, was concerned with the amplification of low or audio frequency alternating currents. The first wide use and perhaps the most important use of the De Forest amplifier was in connection with ordinary telephony over wires. It is this device which made practical long-distance telephony possible, and even to-day it is a question as to whether this use is not the one of greatest utility to mankind. The first use of the De Forest device by the United States Navy in connection with radio signaling was this same use of the device as an amplifier of

low-frequency alternating currents. At a later date other important uses of the De Forest device were discovered, all of which, however, are fundamentally based upon this amplifying property which originated and first developed at low frequencies. These important properties were regenerative amplification, oscillation generation, and radio-frequency amplification, and it is by reason of all of these properties, all based on the amplification property, that the 3-element De Forest device has found wide use in connection with radio signaling. In combination with one of these amplifying properties, the De Forest device has also been used as a detector of radio waves. The detecting function, however, has been of little importance and is not responsible at all for the wide use of this device in radio. As I pointed out in my direct examination, in any of the uses of the De Forest device as a combined detector and amplifier, the well-known crystal detector could be substituted for the detecting function without any loss in results, in some cases with even better results, and in no case with more loss than a mild inconvenience. Thus it is evident that the Fleming disclosure amounted to nothing more than an unimportant offshoot of the prior Edison device, and it is not in accordance with fact to state that the whole radio art of vacuum-tube devices depends upon this disclosure of Fleming which made no impression upon the art, in particular when this radio art of vacuum tubes is largely comprised of the 3-element De Forest device which had its origin as a low-frequency amplifying device and which stands upon its own legs, whether it is used at high or low frequency.

On page 1923 Mr. Waterman refers to my drawing, "Miller drawing B," opposite printed record, page 7[illegible], and which is a comparison of the operation of a 2-electrode rectifier and a 3-electrode amplifier, and he states that this drawing does not illustrate these two devices under similar conditions and therefore is a misleading diagram. Mr. Waterman then produces two diagrams, "Waterman draw- [fol. 1669] ing No. 5" (claimant's Exhibit No. 322), and "Waterman drawing No. 6" (claimant's Exhibit No. 323) in which he proposes to compare these devices in the way that it should be done. The 2-electrode rectifier and the 3-element amplifier are two devices which function in a totally different manner and operate under different operating conditions. When it is attempted to operate either

one of the devices under the operating conditions of the other device the characteristic operation is destroyed. In Waterman drawing No. 5 Mr. Waterman is attempting to operate the 3-element De Forest device under conditions similar to those of the 2-element rectifier. In doing this he destre s the fundamental function of the 3-element device which is that of amplification. In this Waterman drawing No. 5 Figure 1 is the same as Figure 1 of my "Miller drawing B," and correctly represents the functioning of the 2-electrode vacuum tube as a rectifier which is its only useful function. In Figure 2 of this drawing Mr. Waterman attempts to employ the 3-electrode device under similar conditions. In doing this he has left out any connection to the plate element, so that the device has no plate circuit, and he has likewise left out the B battery which is a characteristic of the 3-electrode operation. In Figure 3 of this drawing in another attempt to employ the 3-electrode tube under conditions similar to the 2-electrode tube there is again the absence of the B battery, and furthermore the grid of the 3-electrode device is connected to nothing, and hence left out in the cold. Mr. Waterman states that the results indicated by the diagrams below, the individual figures are identical. This is a fact. The 2-electrode device of Figure 1 is a rectifier and a rectifier alone and the result of its operatien as shown below the figure is that of rectification. The identical results obtained in Figures 2 and 3 show that when it is attempted to employ the 3-element amplifying device of De Forest under conditions similar to those of the 2-electrode device, it loses its characteristic amplifying property and degenerates into nothing more than a rectifier. Defendant has not employed the 3-element device in circuits like these of Figures 2 and 3. In fact, it is obvious that the 3-element device can not function as a 3-element device under these conditions. In Figure 2 there is no plate circuit and no B battery, hence there is no plate current for the grid electrode to control. In Figure 3 there is no B battery and no plate current for the grid to control and there is no grid circuit by which a controlling voltage can be applied to the grid electrode. As circuit diagrams of the 3-element device these diagrams are truly absurd.

In Waterman drawing No. 6 the attempt is made to operate the 2-electrode device with a B battery and to get an operation similar to that of the 3-electrode device. This

is even more absurd though perhaps not as obvious. The addition of a B battery to the 2-electrode device not only does not give it any of the amplifying property of the 3-electrode device, but actually takes away from the 2-electrode tube, its only useful mode of functioning; that is, rectification. Figure 2 of this Waterman drawing No. 6 is taken from "Miller drawing B," and represents in simple form the functioning of the 3-element De Forest device as an amplifier. The wavy curve below this figure represents the alternating component of current in the plate or output circuit and which flows through the indicator I. In wave form this is a reproduction of the alternating volt- [fol. 1670] age applied to the grid or input circuit, the source of which is represented on the left of the figure by an alternator. What this figure does not show by itself, but which I have explained in connection with this same figure at the bottom of page 767 of the record, is the fact that the power represented by this alternating current which flows through the indicating device is many times, perhaps thousands of times, as great as the power which must be supplied by the alternator or alternating voltage applied to the grid or input circuit. This input alternating voltage may, for example, be the weak telephone signal at the end of a long telephone line, while the output device or indicator may be a telephone receiver and the power represented by the current flowing through this receiver will be many times the original signal and hence correspondingly louder. The two fundamental requirements of an amplifying device are first, the wave form of the alternating current in the output of the device should be a duplicate of the alternating voltage applied to the input; second, the power represented by this output current should exceed the power which is required to be supplied in the input of the device. In Mr. Waterman's Figure 1 of this drawing No. 6 where the attempt is made to operate a 2-electrode device with a B battery, it is true that a similar wavy current curve is obtained. However, the alternating current which this wavy form of curve represents flows in a single circuit in which the alternator, the indicator I, and the vacuum tube are in series. The alternating current which flows in this series circuit is produced by the alternator itself and all of the power represented by this current is supplied by the alternator. Since this is the case we are not dealing with amplification. In fact,

the same identical result would be obtained if the vacuum tube in this figure were replaced by an ordinary resistance of suitable magnitude, such as a wire coil. Conditions are the same or worse in Figure 3 of this Waterman drawing No. 6 which again purports to show the use of a 2-electrode tube with a B battery. In this case the alternator supplies alternating current through the condenser and then through the indicator I and the vacuum tube in parallel. The alternator must supply all of the power both for the current which flows through the vacuum tube and the current which flows through the indicator. Only the latter is useful, hence the operation would, if anything, be improved if the vacuum tube were entirely omitted. Since the alternator again supplies at least all of the useful current flowing through the indicator, we are again not concerned with amplification. Therefore, the use of the B battery with the 2-electrode device does not make an amplifying device out of it. What, however, is still more serious is the fact that by trying to utilize a B battery with the 2-electrode device, Mr. Waterman has destroyed its only useful function, that of a rectifier. In all of the figures the dash line in the lower portion of the figure represents the steady current which is drawn from the B battery. Superimposed upon this is the wavy curve form which represents the action of the alternator. It will be noted that in all three of the figures this wavy curve is symmetrical with respect to the dash line, or, in other words, a rising portion of the curve above the dash line is immediately followed by a portion similar in shape below the dash line. The portion above the line leads to an increase in current, but it is immediately followed by a corresponding portion below the dash line [fol. 1671] which cancels and annuls it so that the average current is unchanged by the action of the alternator and is just the same as the steady current drawn from the B battery. The function of a rectifier is to convert from alternating current into direct or continuous current. Since the direct current is unchanged in these figures by the action of the alternating voltage there is no conversion from alternating current to direct current and hence no rectification. Thus, in Figures 1 and 3 the 2-electrode tube not only does not function as an amplifier by reason of the addition of a B battery, but actually loses the capability of operating as a rectifier. In Figure 2 the 3-electrode vacuum tube with a B battery is exhibiting its character-

istic operation as an amplifier and likewise is not operating as a rectifier. Thus, Figures 1 and 3 of this Waterman drawing No. 6 are worthless and represent no useful function. Neither these figures nor Figures 2 and 3 of his drawing No. 5 show anything of value in this issue. Earlier in this answer, on page 1921, Mr. Waterman states "De Forest improved the Fleming 2-element vacuum tube by putting into it the third or grid element. That it was an improvement no one would deny, but he did not thereby take away from the device any of its former properties." The proper statement would be that De Forest converted the Edison 2-element rectifier into a device capable of an entirely new operation, amplification, by inserting in the Edison device a third or control element by using with it a B battery and by employing it in new circuits. In the earlier De Forest patent, as I have pointed out, the device was used as an amplifier for speech or telephony. In order that the device shall operate to reproduce speech distinctly and faithfully or to operate as a "distortionless amplifier" it is essential that rectification be absent to an extent as to be negligible. In Figure 2 of Waterman's drawing No. 6, which is also my drawing and which represents the operation of the 3-element device as an amplifier, I have just pointed out that rectification is absent. Thus the second part of Mr. Waterman's statement is not in accordance with the facts, for the essential step which De Forest took in converting the 2-electrode rectifier into a 3-electrode amplifier was to take away the property of rectification. Later, on page 1924, Mr. Waterman states after discussing the introduction of the grid into the tube by De Forest, "But in adding this device, De Forest did not abandon anything which Fleming had, nor anything which affected the fundamental mode of operation, by electrostatically controlling the emitted electrons. On the contrary, what he did was to perfect and extend this control." What I have just pointed out is the fact that the fundamental thing which De Forest did was to abandon the rectification of the 2 element device and obtain a new device, an amplifying device. With respect to abandoning the electrostatic control of the emitted electrons, it was impossible for De Forest to abandon something not possessed by the Edison or Fleming device. In this answer Mr. Waterman not only confuses the two totally different functionings of rectification and amplification, but he also is confused in his

use of the term "control" to describe these two different operations, and he seems to consider amplification to be nothing different from improved rectification. In my direct examination in answer to question 6, printed record, page 764, I explained very carefully the difference between the functioning of an amplifier and a rectifier, and in particu- [fol. 1672] lar on page 766 pointed out "the function of an amplifier is to keep the character of the alternating quantities unchanged but to increase their power. This is quite the opposite from the function of a rectifier which is required to change the character, viz., convert from alternating current to direct current and in which the power is all supplied by the source." To summarize, the 2-electrode tube is a rectifier and nothing but a rectifier. The 3-element De Forest device is an amplifier, and it is this property of amplification which is entirely responsible for its wide use in the electrical art. It is obvious, therefore, why, in my direct examination in connection with Miller drawing B, I passed directly from the description of the 2-electrode device as a rectifier to the 3-element device of De Forest as an amplifier without interposing worthless diagrams such as those introduced by Mr. Waterman in his drawings Nos. 5 and 6.

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Cross-examination.

By Mr. Vaill.

7. Cross-question. In your answer to question 3 in which you made some rather severe criticisms of Mr. Weagant's demonstrations relating to the capability of the 2-electrode tube to oscillate, I would ask you to compare Figure 1 of the Fleming patent No. 803684 in issue (plaintiff's Exhibit No. 22) with claimant's Exhibit No. 225 (opposite page 1527 of the printed record) and to state as a matter of fact whether the arrangement illustrated in claimant's Exhibit No. 225 includes all of the circuit arrangements of Figure 1 of the Fleming patent except that the variable condenser R has been added in the antenna circuit for the purpose of tuning, a battery P has been added to the plate circuit through the coil *k*, and a variable resistance R_1 has been added for the purpose of adjusting the amount of the filament current.

Answer. With these exceptions stated in the question, and with some more exceptions, there is a resemblance between the circuit diagram of Exhibit 225 and Figure 1 of the Fleming disclosure. One exception additional to those cited in the question is the construction of the vacuum tube shown in Exhibit 225 as compared with the construction shown in Figure 1 of the Fleming disclosure. In my answer to question 3 I called particular attention to the difference in construction in the two tubes illustrated in these respective diagrams, and pointed out that the tube employed by Mr. Weagant in his demonstration was one particularly suited for the application of an additional control element, whereas the tube disclosed in Figure 1 of the Fleming patent is entirely unsuited to this purpose. I note another apparent difference in the two diagrams, that is, that while in the coil system *k. m* of the Fleming disclosure, the sense [fol. 1673] of winding of the two coils as indicated by the spirals employed to designate these coils seems to be the same, whereas in the circuit diagram of Exhibit 225 the spirals representing the coils *m. k* indicate that the two coils have opposite senses of winding. This difference in claimant's Exhibit No. 225 may be accidental or intentional—at any rate, as I have pointed out in this same question 3, it was necessary for Mr. Weagant to have a definite sense of winding in this coil system *m. k* in connection with proper connections between these coils and the other elements of the circuit in order to make his demonstration, whereas in so far as the purpose of the Fleming disclosure is concerned, the matter of this sense of winding or of connections is of no importance.

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8. Cross-question. Was your use of the word "trick" in your answer to question 3 intended to imply an attempt to mislead the court as to actually what took place during Mr. Weagant's demonstrations on account of any failure of Mr. Weagant to disclose during the demonstrations anything which should have been disclosed, or are you using the term simply as indicating that an expert radio engineer arranged the circuits in an efficient way to produce the desired results?

Answer. My use of the word "trick" in answer to question 3 was not intended to imply an attempt to mislead the court or any failure of Mr. Weagant to disclose anything

which should have been disclosed. In fact it will be noted that in discussing these demonstrations I employed and quoted Mr. Weagant's explanations throughout. On the other hand, I did not use the term simply as indicating that an expert radio engineer arranged the circuits in an efficient way to produce the desired results corresponding with the latter part of the question.

9. Cross-question. If you did not use the term as stated in the last sentence of your previous answer, what did you intend to imply by it?

Answer. I intended to imply by the use of the word "trick" the same meaning that would be implied in speaking of the "trick" of a magician. The trick of a magician comprises doing something that is well recognized as being something impossible to perform. The demonstration of Mr. Weagant purported to be the demonstration of the generation of oscillations by a high-vacuum 2-electrode tube. The high-vacuum 2-electrode tube does not possess this capability, which is a matter well known and well understood by those skilled in the art. Therefore Mr. Weagant's demonstration was an attempt to demonstrate something not possible of demonstration. Just as in the case of the magician's trick, there is an explanation as to how the trick was done. I pointed out that in the case of Mr. Weagant's demonstration, this demonstration was made possible by the addition of a third, or control, element. The facts upon which I base this statement are contained in Mr. Weagant's testimony, but he does not put these facts in the form in which I have put them and in which the fact that he was employing the 3-element device and not a 2-element device becomes completely evident.

[fol. 1674] 10. Cross-question. It is true, is it not, that in the work of a magician or a prestidigitator, the attempt is always to conceal from his audience the facts or means employed to produce a particular result with a certain set of materials which is well within the vision of the audience. In other words, the purpose of the work of a magician or prestidigitator is to mystify and to produce results apparently contrary to well-recognized physical laws. You did not use the word "trick" in this sense, did you?

Answer. The avowed intent of Mr. Weagant in this demonstration was to show that a high-vacuum 2-electrode vacuum tube was capable of operating as an oscillation generator. This is a result contrary to well-recognized physical

laws. In this respect there seems to me to be a resemblance between this demonstration and the trick of a magician. It is true that Mr. Weagant gave a technical explanation as to how his results were obtained, which made it very evident to me how the trick was performed. On the other hand, he does not give the very simple and nontechnical explanation of his demonstration which is possible; that is, that the device was not a high-vacuum 2-electrode device, but a device comprising a third element, or control element. Thus, as far as results which this demonstration purports to produce, the use of my term "trick" seems to be appropriate, and in so far as the explanation of the operation was not reduced to the simplest possible terms, there is a possibility that the way in which the trick was worked might not be entirely clear to everyone.

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11. Cross-question. Assuming that you have read the stipulation on page 1546 of the printed record regarding the amount of vacuum in the tubes used by Mr. Weagant in his demonstrations, I show you tube No. 665 mentioned in said stipulation and which was offered in evidence as a part of "Claimant's Exhibit No. 224." This is a 2-electrode vacuum tube having two conductors adjacent but not touching each other inclosed therein, one of said conductors adapted to be heated by a battery and the other conductor, the circular plate, being an opposed electrode, is it not?

Answer. This tube No. 665 appears to me to be a 2-electrode vacuum tube as described in the latter part of the question, excepting that I do not know what is meant by "opposed electrode."

12. Cross-question. You understand, do you not, that this tube was used in Mr. Weagant's demonstration in which oscillations were produced, in the identical physical form that it is now submitted to you for examination, referring particularly to the tube as shown in the photograph, claimant's Exhibit No. 228, opposite page 1528 of the printed record?

Answer. In so far as the tube per se is concerned, I have no reason to doubt that this physical form of tube was employed in Mr. Weagant's oscillation demonstration. It seems to correspond very closely with the description given by Mr. Weagant and discussed in my question 3.

13. Cross-question. Your previous answer, as I understand it, involves the fact that no third electrode was added to this tube during the demonstration by Mr. Weagant, as [fol. 1675] illustrated by the arrangement shown in claimant's Exhibit No. 228 when taken in connection with the diagram of claimant's Exhibit No. 225. Is my understanding correct?

Answer. If the term "electrode" is restricted to something sealed inside of the tube, I am willing to state that to the best of my understanding no such electrode was employed by Mr. Weagant during his demonstration. The term "electrode" probably should be restricted to this form, but is also frequently used for a piece of metal or something similar on the outside of the tube and which operates in connection with the electrical functioning of the tube. Mr. Weagant apparently does not draw any sharp distinction in this respect, and in his cross-examination states, on page 1540, that the matter depends largely on the significance and meaning given to the word "electrode." I know that in Mr. Weagant's patents which were under discussion at this time, he frequently speaks of an external control element or electrode. In the sense that there was no electrode sealed within the tube, I will agree that no third electrode was added to this tube during the demonstration by Mr. Weagant.

14. Cross-question. Referring again to claimant's Exhibit No. 22, Fleming patent No. 803684, particularly to Figure 1 thereof, I would ask you whether, according to your understanding of Mr. Weagant's demonstration, as illustrated in claimant's Exhibit No. 225 and claimant's Exhibit No. 228 relating to the oscillating properties of the 2-electrode tube, any physical element was added to the parts and constituting a third electrode over what is shown in Figure 1 of said Fleming patent?

Answer. In the broad sense in which Mr. Weagant has used the term "electrode" in his patents mentioned above, the condenser R of claimant's Exhibit No. 225 is a third or added electrode to the disclosure of Figure 1 of the Fleming patent. In the narrow sense of an electrode as being something sealed within the tube, such an electrode was not employed in Mr. Weagant's demonstrations.

15. Cross-question. Then, as I understand your last answer, you are in accord with my understanding that no third electrode forming a part of the tube structure was

added during the demonstrations which were made by Mr. Weagant and as illustrated in claimant's Exhibits Nos. 225 and 228?

Answer. Whether or not I am in accord with your understanding depends upon your understanding of the meaning of the term "electrode." The condenser R, as employed in a demonstration, of which Exhibit No. 225 is a circuit diagram, was placed very close to the glass wall of the vacuum tube, and was, as accords with Mr. Weagant's explanation of this demonstration, an essential factor in the operation of the tube. In this respect the condenser R is just as much an electrode added to the tube as the external electrode which is added in Mr. Weagant's patent disclosures above referred to.

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16. Cross-question. The condenser R referred to by you in your last answer and indicated in claimant's Exhibit No. 225 and claimant's Exhibit No. 228, which latter is a photograph and shows the condenser as a cylindrical black casing with a white dial and black knobs on the top thereof, is [fol. 1676] what is commonly known in the art as a variable condenser and has long been used in the radio art in circuits in which it has been desired to vary the capacitance of a circuit for tuning or other purposes affecting the electrical period of the circuit and never has formed a part of a vacuum tube, either a 2-electrode tube or 3-electrode tube. Is that correct?

Answer. Excepting for Mr. Weagant's demonstration with which we are here concerned, this statement is perhaps generally true. Such a condenser is generally used for tuning purposes alone. However, in accordance with Mr. Weagant's explanation of his demonstrations, this condenser R is here operating as a control element over the electron stream flowing within the vacuum tube by reason of the fact that the condenser is physically located very close to the walls of the tube so that its electrostatic field is capable of controlling the current flow within the tube. Thus functionally it is just as much a part of the tube as the grid electrode of the DeForest device. As I have pointed out, the action of this condenser, though less effective, is identical in functioning, both to the grid electrode of the DeForest device, or the external electrode of the Weagant patents.

Thus functionally it is just as much a part of the tube as either of the other two.

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17. Cross-question. Now, Doctor Miller, will you kindly confine yourself to the answers to my questions, as put, and not wander off into argumentative dissertations on your interpretation of the facts inquired about? I will ask you again whether the condenser R is known in the art as a variable condenser for varying the cap-itance in an electrical circuit which has been in common use for a long time and is one of the accepted elements usually employed in various rădio circuits for tuning them. Will you please answer my questions specially, yes or no?

Answer. I can not answer this question yes or no, by reason of the specific reference to the condenser R.

18. Cross-question. You know, of course, do you not, that what I refer to as a condenser R is the same condenser referred to in my previous question and which was used in Mr. Weagant's demonstrations of the oscillations occurring through the use of a 2-electrode vacuum tube, and as previously referred to as being indicated in claimant's Exhibits Nos. 225 and 228. Will you kindly answer the question specifically, if you did not before understand that condenser R was the one just indicated in those exhibits?

Answer. Since this question makes it clear that the condenser R of Mr. Weagant's demonstrations was specifically referred to in the preceding question, and not variable condensers in general, it is not possible for me to answer the preceding question, yes or no. As I have pointed out, this condenser R is used in a dual function.

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(Witness continues answer.) With the understanding that by condenser R nothing more is meant than variable condensers in general, my answer to question 17 is "Yes."

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[fol. 1677] 19. Cross-question. In your direct examination you referred to the demonstrations or tests made before Judge Mayer and characterized the demonstration made by Mr. Weagant in this case by similar derogatory terms. In view of this I will ask you to examine the sketch "Defendant's Exhibit A" printed opposite page 266 of the printed

record, and ask you if the vacuum tube shown in that sketch is not physically the substantial equivalent of the vacuum tube No. 665 of claimant's Exhibit 224, which I have already shown you?

Answer. The sketch "Defendant's Exhibit A" shows a vacuum tube quite similar in construction to the vacuum tube No. 665 of claimant's Exhibit No. 224, which has been shown to me. As to the term "physically the substantial equivalent" I am not sure as to what is implied, and to whether it could be shown by a sketch.

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20. Cross-question. Are you willing to say that the electrode tube illustrated in defendant's Exhibit A and the tube No. 665 of claimant's Exhibit No. 224 are the same type of 2-electrode vacuum tube. Please answer specifically, yes or no.

Answer. I can not answer this question, yes or no, by reason of the fact that there are two parts to the question which require different answers.

21. Cross-question. My question was whether you are willing to answer the question, or say under the wording of my question whether you are willing to state that the two tubes referred to are the same type of 2-electrode vacuum tube. Will you answer that question, yes or no?

Answer. I am willing to answer the question yes or no providing you point out specifically what you mean by "the same type of 2-electrode vacuum tube."

22. Cross-question. Although I can not see that my designation of the tubes requires any explanation, if you require it I will say that I intend to include under the term "type" a 2-electrode tube inclosed in a glass container in which there is evacuated space, one of the electrodes being capable of being heated, while the other electrode is separated from the first electrode in the evacuated space and capable of receiving the electrons emitted by the first or heated electrode. With this explanation in mind as to the use of the word "type," will you please state specifically by yes or no, whether the vacuum tube No. 665 of claimant's Exhibit No. 224 is the same type as the vacuum tube illustrated in defendant's Exhibit A?

Answer. Yes.

23. Cross-question. In your direct examination where you referred to the demonstrations made by Mr. Weagant

in which the 2-electrode tube used by him was caused to produce oscillations, you had a great deal to say about the part located outside of the tube itself. In view of this, I will ask you if it isn't a fact that in order to make a 3-electrode tube containing a high vacuum—that is, a tube having a so-called plate, grid, and a filament adapted to be heated—that the conditions which produce or enable the oscillations to be produced in the tube depend upon the [fol. 1678] arrangements of the wires of circuits exterior to the tube as much, if not more, than the fact that the tube contains three electrodes, such as in a De Forest audion?

Answer. Both the tube and the circuit are essential in the production of oscillations. Therefore I can not state that one is more important than the other.

24. Cross-question. The audion or 3-electrode vacuum tube will not generate oscillations, will it, except that by the addition of a special outside circuit and instrumentalities such as a circuit capable of oscillatory currents, means of applying a controlling voltage to the grid, and means for supplying power from the plate circuit of the tube to maintain the oscillations?

Answer. It will not.

25. Cross-question. In answer to question 6 on your direct examination you stated as follows:

"In order that the device shall operate to reproduce speech distinctly and faithfully, or to operate as a 'distortionless amplifier' it is essential that rectification be absent to an extent as to be negligible."

In making this statement I understand you were referring to the 3-electrode tube of "the earlier De Forest patent." With this understanding I will ask you if it is true that the 3-electrode De Forest tube has two cold electrodes and one hot electrode?

Answer. I see no connection between the two portions of this question, and hence must comment on both. The statement quoted in the first portion of the question applies to the 3-electrode tube as a speech amplifier in general, and hence includes the earlier De Forest patent. The last sentence of the question is generally true.

26. Cross-question. Still considering that the 3-electrode De Forest tube inquired about may be used either for a

speech amplifier or a radio-frequency amplifier, it is true, is it not, that if you canneet an electrical circuit to either of the cold electrodes and apply an alternating potential between either one of them and the hot electrode, the current will flow through each of these circuits in one direction but not in the other, whether the grid or the plate is the cold electrode selected?

Answer. This is not true.

27. Cross-question. In order to avoid inserting unnecessary conditions in your answer, which I think you contemplate doing, if the question is persisted upon, I may explain that the 3-electrode tube under consideration is in no other circuit than a circuit which may be connected between the hot filament and the grid, or the hot filament and the plate, and an alternating potential applied between those elements independently. Under these conditions I take it that you will assent to the proposition that in either case the current will flow but in one direction, whether it be the grid or the plate that is connected with the filament. Is that so? If you desire a diagrammatic representation of the two conditions under which the current will flow in one direction only, I will say that you may refer to Figures 2 and 3 of claimant's Exhibit No. 322, Waterman drawing No. 5, printed opposite page 1923, which you considered during your direct examination.

Answer. Yes, I will agree that if the attempt is made to operate a 3-electrode tube in circuits such as Figures 2 and [fol. 1679] 3 of Waterman drawing referred to, in either of the cases there shown the current will flow but in one direction. As I pointed out in my answer to question 6 in discussing this Waterman drawing, this attempted operation causes the 3-electrode tube to lose its function of amplification and degenerate into nothing but a rectifier. This does not change my answer to the preceding question which was concerned with the 3-electrode De Forest tube as an amplifier.

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28. Cross-question. Doctor Miller, will you please confine your answers to my questions without injecting other considerations? By so doing we shall save much time. When this action is taking place between the grid and the filament or the plate and the filament, as inquired about

in cross-question 27, it is commonly known in the art as rectification, is it not?

Answer. Yes.

29. Cross-question. And the vacuum tube under either of these conditions would then be rectifying, would it not?

Answer. Yes.

30. Cross-question. When the 3-electrode De Forest tube is employed as an amplifier, there is a circuit connection between the cold plate and the hot filament, is there not?

Answer. There is.

31. Cross-question. In such a De Forest tube so used the current through this circuit between the hot filament and the cold plate flows in one direction only, does it not?

Answer. No.

32. Cross-question. Do you mean that it flows in two directions?

Answer. When it is operating as an amplifier the current with which we are concerned in the amplifying process can flow in either direction in the plate circuit.

33. Cross-question. Assuming that you understand the part of the circuit within the tube is included in the plate circuit, do you mean to say seriously that there can be an electron flow in both directions within a 3-electrode De Forest tube when the tube is acting as an amplifier; of course I mean the electron flow between the plate and filament?

Answer. If you mean the steady flow of electrons which is produced by the B battery in the plate circuit of the De Forest amplifying device, it is a fact that this flow can not be reversed. However, what we are concerned with in the amplification process is an alternating current superimposed upon this steady direct current. It is well known that an alternating current flows in both directions in a circuit. Hence in so far as we are concerned with the De Forest device as an amplifier, we are concerned with a current which flows in either direction in the plate circuit of the device.

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34. Cross-question. Now, Doctor Miller, it is a fact, is it not, that when a 3-electrode tube is acting as an amplifier, there is absolutely no reverse flow of the current between the filament and the plate, although what you have termed "superimposed alternating current" may act upon the

potential produced by the B battery. In other words, when a 3-electrode De Forest tube acts as an amplifier [fol. 1680] there is always and only a flow in one direction, between the filament and the plate, although this may fluctuate according to the vibration of the voice or music. Please state whether or not this is correct, specifically by the answer yes or no.

Answer. I can not answer this question yes or no, by reason of the fact that it is necessary that my answer be qualified as in the preceding answer.

35. Cross-question. Will you please answer specifically by yes or no whether in a 3-electrode De Forest tube when it is acting as an amplifier, there is any actual flow of electrons between the filament and plate in any other than one direction?

Answer. I can not answer this question yes or no and without qualifying the answer. To do so would lead by implification to the existence in the plate circuit of the De Forest amplifying device of an operation which is not present in the plate circuit of that device.

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36. Cross-question. In view of your last answer and your opportunity to consider the last question since the termination of the last session of testimony, I will ask you to please answer specifically by yes or no whether in a 3-electrode De Forest tube when it is acting as an amplifier there is any actual flow of electrons between the filament and plate in any other than one direction? If your answer is "No," and you consider that such an answer by itself would lead by implication to the deduction that there may exist in the plate circuit of the De Forest amplifying tube something that is not present in the plate circuit of that device, you may for your own satisfaction qualify that answer.

Answer. My answer is "No" if the flow of electrons which is being considered is the total flow of electrons, or that corresponding to the steady direct current produced by the battery with an alternating flow superimposed upon this steady flow, which latter is the thing of interest when we are considering the device as an amplifying device. The whole pheaomenon is not one which depends upon the fact as to whether a vacuum tube is included in the circuit or not. It is merely a matter of the superposition of an

alternating-current flow upon a steady direct-current flow and does not differ from what would occur under these same conditions in any electrical circuit, whether it contains a vacuum tube or not. This assumes of course that the amplitude of the alternating-current component does not exceed the value of the steady current in the circuit. If this were to happen then there would be a difference in the case of the vacuum tube over that occurring in an ordinary [fol. 1681] electrical circuit. However, this merely means that the vacuum tube is being overloaded and the practice in the art would be to take a larger tube carrying a larger direct current so that this could not occur.

37. Cross-question. The alternating-current flow which you say is superimposed upon the steady flow produced by the so-called B battery, as a matter of fact never produces an actual reverse flow in the plate circuit; that is, in opposite direction to the flow produced by the B battery, does it? Will you please answer the question, yes or no? You may assume, of course, that I am referring to the normal operation of the tube in amplifying without what you have termed in your last answer as the vacuum tube "being overloaded."

Answer. If you mean the total current is never reversed, the answer is "No," under the conditions of the question.

38. Cross-question. I did not qualify my question by the use of the word "total" and I do not know how you use that term, but I will again ask you whether the alternating-current flow which you said is superimposed upon the steady flow produced by the B battery ever produces any actual reverse flow at all in the plate circuit; that is, in a direction opposite to the flow produced by the B battery. Will you please answer the question by yes or no?

Answer. Your question refers to two currents superimposed upon each other. The answer is "no" if you have in mind the sum or resultant of these two currents.

39. Cross-question. Then, as I understand your last answer, you would admit that if a current meter capable of showing the positive or negative flow of a current through it were placed in the plate circuit of a vacuum tube when acting normally as an amplifier, of course I refer to a De Forest 3-electrode tube, that meter would always show a current flowing in one direction only. Is that so?

Answer. That would depend upon the characteristics of the meter. If this were an ordinary direct-current meter

it would show merely the plate battery current. If the meter could respond rapidly enough to follow the alternating current fluctuations in the circuit, it would show superimposed upon this battery current an alternating current flowing first in one direction in the circuit and then in the other direction.

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40. Cross-question. Now, Doctor Miller, I will repeat the above question with the additional limitation or explanation that the meter referred to is a direct-current milliammeter capable of showing current flow in either direction above or below the zero reading when it is detached from any circuit. Such a meter when placed in a plate circuit as stated in the previous question would never show a current flowing in opposition to the current produced by the B battery when the tube is operating normally, would it? Please answer this questoin by "It would not," or "It would," whichever you consider to be correct.

Answer. I can not answer this question "It would not," or "It would," by reason of the fact that my answer depends upon the possible rapidity of response of the meter, as discussed in my previous answer.

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[fol. 1682] 41. Cross-question. Now, Doctor Miller, will you allow me to ask you the following question, and will you confine your answer either affirmatively or negatively, as the case may be? When a 3-electrode De Forest vacuum tube is being used as an amplifier and a milliammeter capable of indicating either a positive or a negative flow of current therethrough be inserted in the plate circuit when the tube is acting normally, would such a meter ever have its indicating pointer on the opposite side of the zero mark from that side on which it would be when indicating the normal flow through the plate circuit? Of coarse I am assuming that you understand that the tube is amplifying the received current which is of an alternating form and is impressed upon the plate current, or superimposed, as you have stated. Will you please answer this question by "It would," or "It would not"?

Answer. It would not.

42. Cross-question. Under the conditions stated in the last question the plate circuit would be a circuit containing a rectifier, would it not?

Answer: It would not.

43. Cross-question. Do you not consider that a device which having an alternating current applied to the input side of it resulting in a unidirectional flow in the output circuit, such as would be indicated by the ammeter under the conditions mentioned in cross-question 41, would constitute a rectifier?

Answer. Your question assumes that which is contrary to the facts. In the use of the De Forest device as an amplifier the application of an alternating current to the input side of the device does not result in a unidirectional flow in the output circuit, and an ammeter in the output circuit does not indicate such a result in this case. As I pointed out near the end of my ansnwer to question 6 in the surrebuttal testimony, the application of an alternating voltage to the input circuit of this amplifying device has no effect upon the steady or direct current flowing in the plate circuit of the device. There is, therefore, no conversion of alternating current into direct current, and hence no rectification, and the device does not constitute a rectifier.

44. Cross-question. When you say, as you did in your last answer, in referring to the De Forest tube as an amplifier, "the device does not result in a unidirectional flow in the output circuit, and an ammeter in the output circuit does not indicate such a result in this case," you mean, do you not, that the ammeter does not indicate that the current in the plate circuit consists of two components, one of which is a steady current due to the plate battery, and the other of which is a fluctuating or pulsating component, which varies in accordance with the alternating potentials impressed upon the tube? Am I correct in understanding this view of your statement?

Answer. No. The two components are a steady current due to the plate battery and an alternating component which corresponds with the alternating potentials impressed upon the tube. A direct-current meter in the plate circuit reads the same whether the alternating component is present or not; that is, it reads the same whether an alternating voltage [fol. 1683] is applied to the input of the device or not. There is, therefore, no change in the direct current, no conversion from alternating into direct current, and no rectification.

45. Cross-question. Do you consider that your last statement would be true for any rate of frequency impressed upon the input circuit of the tube; that is, whether of high frequency or low frequency?

Answer. There is no difference in so far as the operation of the vacuum tube is concerned. At very low frequencies the direct-current meter might not function in the way it is normally supposed to function; that is, to indicate the direct current, but may follow the alternating current variations to some extent.

46. Cross-question. Do you still consider that under the condition stated in your last answer—that is, when the direct-current meter is following "the alternating current variations to some extent"—the plate circuit would not be a circuit containing a rectifier?

Answer. Certainly. The tube is functioning in just the same way. It is merely a matter that the meter might not function properly as a direct-current meter.

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47. Cross-question. In order that there may be no uncertainty as to what you intended to say in your last answer, I will ask you, when the De Forest 3-electrode tube is employed as an amplifier and there is a circuit connection between the plate and the filament, the latter circuit is a circuit containing a rectifier, is it not?

Answer. It is not.

48. Cross-question. I believe you have already agreed that when the 3-electrode De Forest tube is employed as an amplifier there is a circuit connection between the plate and the filament. That is so, is it not?

Answer. Yes.

49. Cross-question. The current through this circuit flows in only one direction, does it not?

Answer. That dependends upon what current you are talking about. If you are talking about the steady battery current, the answer is "yes"; if you are talking about the alternating current, which results from the operation of the device as an amplifier, the answer is "no," for this latter current flows in both directions in the plate circuit.

50. Cross-question. Do you mean to say when you refer to the plate circuit in your last answer to that part of the circuit which exists between the plate and the filament?

Answer. I mean the complete plate circuit; that is, ex-

ternal to the tube between plate and filament of the tube, and internal of the tube between the same two electrodes. The same current flows at all parts of this circuit.

51. Cross-question. By your last answer do you mean that the current flows in both directions between the plate and the filament?

Answer. The alternating component of the current flows in both directions.

[fol. 1684] 52. Cross-question. By alternating current in your last answer do you not mean the up-and-down variation of the current in the plate circuit?

Answer. It is the superposition of this alternating-current component upon the direct-current component which produces the resultant or total current that fluctuates up and down in the plate circuit of the tube.

53. Cross-question. But that total current that fluctuates up and down in the plate circuit of the tube never in any case produces a flow in the tube in the opposite direction to the flow produced by the plate battery, does it?

Answer. No. I have stated before that the total plate current does not reverse. This is by reason of the fact that the amplitude of the alternating-current component is always less than that of the direct current.

54. Cross-question. When the DeForest 3-electrode device is employed as an amplifier the potential due to the incoming signal is applied between the grid and the filament, is it not?

Answer. In some cases it is—in other cases it is not.

55. Cross-question. In cases where the incoming signal is applied between the grid and the filament when the De Forest tube is being used as an amplifier and no biasing potential is applied to the grid, the potential due to the incoming signal causes the current to flow between the filament and grid in one direction but not in the other, does it not?

Answer. If such a current does flow it is harmful to the operation of the device as an amplifier, and it merely means that you are using an improper circuit for amplifying purposes.

56. Cross-question. I did not ask you anything about an improper circuit for amplifying purposes. Will you please confine your answer to the question asked, and if you did not know it, please assume that it is a proper circuit for amplifying purposes.

Answer. The answer is then that no appreciable current will flow between the grid and filament of the tube.

57. Cross-question. I understand of course that you are answering my previous question, as put, and that you have not injected other conditions, and that it assumes that there is no negative bias, as stated in that question. Is that correct?

Answer. Yes. I only assume, as stated in your previous question, that it is the proper circuit for amplifying purposes.

58. Cross-question. In order that there may be no misunderstanding about your last answer, will you please state specifically whether you did or did not assume that the amplifying tube was used under the conditions stated in cross-question 55, which provided that "no biasing potential is applied to the grid?"

Answer. I did assume that "no biasing potential is applied to the grid" and the circuit is a proper one for amplifying purposes, as stated in cross-question 56. I might add that these conditions can only be satisfied when the alternating voltage that is to be amplified is a very feeble one.

59. Cross-question. In order that there may be no misunderstanding as to the meaning of your answer to cross-question 55, I will ask it again as follows:

[fol. 1685] In cases where the incoming signal is applied between the grid and the filament when the De Forest tube is being used as an amplifier and no biasing potential is applied to the grid, the potential due to the incoming signal causes the current to flow between the filament and grid in one direction but not in the other, does it not? Please answer this question specifically, yes or no, without injecting conditions not inquired about.

Answer. Unless you have something unusual in mind, the answer is No; not to an appreciable extent.

60. Cross-question. What do you mean by the word "appreciable" in your last answer? Does it infer any flow at all, except in one direction?

Answer. As I have pointed out before, the current flow between the grid and filament is detrimental to the operation of the device as an amplifier, and it is usual to take steps such as the biasing battery, so as to absolutely prevent such a flow from taking place. It is possible that a

slight amount of such flow might be permissible if its effect upon the amplifying process is so slight as to be inappreciable. That is what I meant by the use of the word "appreciable"; that is, that such a current may flow to the extent that it is not appreciable in its effect upon the amplification. The only effect which such a current can have is one detrimental to the operation of the device as an amplifier.

61. Cross-question. Under the conditions stated in my cross-question 59, it is true, is it not, that the internal circuit between the grid and filament permits the flow in one direction only without appreciable flow in the other direction, and that the action between the grid and filament under those conditions is one of rectification, it is not?

Answer. It is true that under the assumed conditions and perhaps with a fairly strong signal that such rectification can occur in the input circuit of the tube. I must add, however, that under these conditions your circuit is not suitable for amplification purposes.

62. Cross-question. You state that "under these conditions your circuit is not suitable for amplification purposes." It is true, is it not, that even at the present day 3-electrode De Forest tubes are used for amplification purposes in many instances without a grid biasing battery or potential?

Answer. Yes, it is true that in many cases the De Forest tubes are used for amplification purposes without a grid biasing battery; usually in those cases where the alternating voltage which is being amplified is fairly feeble. Whenever the voltage which is to be amplified is rather large almost invariable the biasing battery is employed.

63. Cross-question. Under the conditions stated in the preceding answer in which the biasing battery is employed, there is then no current flowing in the grid circuit, is there?

Answer. There is no current of the kind which we have been considering if a suitable value of biasing potential is employed for the job on hand.

64. Cross-question. What is this "suitable value of biasing potential" applied to the grid under the conditions you have just stated?

[fol. 1686] Answer. Roughly, the magnitude of the biasing battery should be about the same in value as the amplitude of the maximum alternating voltage which is to be amplified.

65. Cross-question. Why is this "suitable value of biasing potential" applied to the grid under the conditions you have just stated?

Answer. To prevent an appreciable flow of current in the grid circuit which would be detrimental to the process of amplification under these conditions. I might add that the use of such a biasing potential is almost universal in the use of the De Forest device as an amplifier at the present day.

66. Cross-question. In what way would an appreciable flow of current in the grid circuit be detrimental to the process of amplification under these conditions?

Answer. In two ways. First, the actual amplification is reduced; second, in the case of audio or telephone frequency amplification there is distortion which operates to destroy the intelligibility of speech or the pleasing characteristics of music. In the case of tuned radio frequency amplifiers it also results in loss of selectivity, or in the facility of distinguishing between one signal and another.

67. Cross-question. This use of a biasing battery for the grid circuit of the 3-electrode De Forest tube is made possible by the rectifying capability of such a tube, is it not?

Answer. This is rather a peculiar statement, since the biasing battery is employed to prevent rectification. It is true that if there were no danger of rectification the biasing battery would probably not be employed, but I would not state that it is rectification that has made the biasing battery possible. I would rather state that it is the biasing battery which eliminates and prevents rectification.

68. Cross-question. It would not be possible, would it, to use a biasing battery in a 3-electrode De Forest amplifying tube if there were no rectification between the grid and the filament?

Answer. As I have stated previously, there is no rectification in the 3-electrode De Forest amplifying device, and it is the biasing battery in many cases which brings this about. It might be possible to use a biasing battery even if there were no possibility of rectification, but what the function of such a battery would be I do not know, for the function of the biasing battery is to prevent the possibility of rectification in the use of the 3-electrode De Forest device as an amplifier.

69. Cross-question. Then to state the fact in a slightly different way, it is true, is it not, that when a biasing

battery is used with a 3-electrode De Forest tube for amplification purposes there is no current flow at all between the grid and filament, although the grid itself may have a decided negative potential?

Answer. Yes; that is true.

70. Cross-question. During your direct examination I believe you mentioned the book written by Dr. H. J. Van der Bijl, which I believe was published by McGraw-Hill Book Co., (Inc.), I therefore desire to quote on the record a paragraph from page 149 of the first edition of that book (second impression) and ask you if you differ from the statements made in this paragraph, as follows:

"If, now an alternating E. M. F. be impressed on the grid circuit so that the grid becomes alternately positive [fol. 1687] and negative with respect to the filament or cathode, the resistance of the grid circuit GFE_g which is usually referred to as the input circuit, will, if the frequency is not too high, be practically infinite for the half cycle that the grid is negative and finite, but variable for the positive half cycle. If, on the other hand, the alternating E. M. F. be superimposed upon a constant negative grid potential, which is so chosen with respect to the value of the impressed alternating voltage that the grid always remains negative with respect to the filament, the resistance of the input circuit is infinite."

Answer. I agree with this statement of Dr. Van der Bijl generally, merely pointing out that in the first part of his statement the effect of which he is speaking would only be appreciable for alternating E. M. F.'s of relatively large magnitude.

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71. Cross-question. In view of your answer to cross-question 69 I will ask you to state your understanding of why it is that there is no current flow at all between the grid and filament when the grid itself has a decided negative potential, understanding, of course, that the 3-electrode De Forest tube referred to therein is being used as an amplifier?

Answer. This is a direct result of the property of unilateral conduction which was discovered by Edison. Thus Edison discovered that when we have a heated filament and a cold plate in an evacuated inclosure no current will

flow between the filament and the plate if the plate is negative with respect to the filament, but current will flow if the plate is positive with respect to the filament. It is the combination of these two effects which was employed by Howell, Wehnelt, and Fleming himself prior to the disclosure of the Fleming patent to make the device operate as a rectifier. In order for the device to operate as a rectifier the alternating voltage applied to it must on one swing cause the plate to become positive with respect to the filament and on the other swing to become negative, so that on one swing you get a current flow, but on the opposite swing there will be no current flow.

Now, if a steady biasing voltage is applied to the device so as to make the plate element always negative with respect to the filament element there will be no current flow between the two electrodes and hence no possibility of rectification, even though an alternating voltage be applied simultaneously with the biasing voltage. In other words, the presence of this steady biasing voltage removes one of the conditions of operation which would be essential for the device to operate as a rectifier.

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[fol. 1688] 72. Cross-question. It is true, is it not, that any 3-electrode tubes with a high vacuum, such as those used at the present day and known as "power tubes," may be used as detectors if desired?

Answer. They may be used as a combined detector and amplifier.

73. Cross-question. In your answer to question 3 of your present deposition (TW., p. 41) you stated, after referring to a voltage of 115 volts, as follows:

"This voltage is a characteristic 'B' battery voltage of De Forest, which is essential with an amplifying device."

It is a fact, is it not, that "B" battery voltages measuring all the way from 135 to 400 volts or more are now commonly employed in amplifying devices or 3-electrode tubes of the De Forest type?

Answer. Such voltages are employed to quite an extent.

74. Cross-question. If your knowledge extends so far, will you please state whether or not it is a fact that in using the early De Forest amplifying tube it was usual to use plate or "B" battery voltages ranging between 12

and 35 or 40 volts, depending upon the vacuum characteristics of such tubes?

Answer. From my knowledge of the use of the early De Forest amplifying tubes I would state that your upper limit of voltage is about right, but your lower limit is too low.

75. Cross-question. Do you mean to imply by your last answer that an early De Forest 3-electrode tube would not act as an amplifier if voltages of the order of 12 or 15 volts were applied to the plate electrode?

Answer. I do not. The operation would be inferior to that obtained with a higher voltage. Your question, I understood, was directed to the practice in the art.

76. Cross-question. My previous question simply asked you if an early 3-electrode De Forest tube would amplify if a "B" battery voltage of the order of 12 to 15 volts were applied to the plate electrode. I understand from your last answer that the answer is in effect in the alternative. Is my understanding correct?

Answer. Your understanding is correct.

77. Cross-question. According to your understanding, what change was made in the early De Forest type of 3-electrode tube which enabled higher voltage to be used, such as those of the order of 135, 180, or more? Of course I refer to the plate or "B" battery voltage.

Answer. The vacuum was improved.

78. Cross-question. What other changes have been made in the early De Forest tubes, according to your knowledge, which have improved their operation as amplifiers, oscillators, or detectors?

Answer. By your use of the term "detector," in answering your question, I must assume that you refer to the combined use of the De Forest type tubes as amplifiers and detectors. The improvements, rather than changes, which have led to improved amplification are primarily improvements in the material employed for the filament, in addition to the improvement in vacuum, mentioned above.

79. Cross-question. It is true, is it not, that in 3-electrode vacuum tubes in common use, or at least a large propor- [fol. 1689] tion of them, the plate electrode completely surrounds a filament, or hot electrode, as it did in the Fleming patent No. 803684, in suit?

Answer. In the Fleming patent the plate electrode is shown as a cylinder completely surrounding the filament;

probably the most general construction employed to-day is a sort of oval-shaped electrode surrounding the filament, though in many cases the plate electrode is split into two parts, one part located on either side of the filament, but the two being connected together and operating as a single plate electrode.

80. Cross-question. It is true, is it not, that 3-electrode tubes of modern construction are sold at the present day, and have been so sold for several years, that are intended to be used as detectors in receiving sets, and not as amplifiers?

Answer. Assuming that your question uses the term "detector" in the loose sense as referring to the combined use of the 3-electrode device as a detector and amplifier, I agree that tubes have been sold for this use exclusively.

81. Cross-question. In order to have specific examples of such 3-electrode tubes that have been sold as detectors within the last few years, of record in this case, in view of your direct examination which deals mainly with the amplification and oscillation of 3-electrode tubes I now show you two tubes contained within pasteboard cartons which I mark, respectively, "Radiotron, Model UX-200 Detector" and "Radiotron, Model UX-200-A Detector," and ask you if it is not correct, according to your knowledge, that such tubes as these contained in these cartons have not been available in the market for several years?

Answer. These tubes which are intended to be used as combined detectors and amplifiers, loosely termed "detectors," have been available in the market for several years.

Mr. Vaill: The tubes and the cartons with their contents are offered in evidence as claimant's cross Exhibits Nos. 350 and 351, respectively, and it is requested that they be so marked by the notary.

82. Cross-question. In a receiving circuit containing a 2-electrode vacuum tube such as the circuit illustrated in Figure 1 of the Fleming patent in suit No. 803684 (plaintiff's Exhibit No. 22), it is true, is it not, that the incoming signal, when impressed upon the cold electrode *c*, causes a variation in the electron stream flowing between the hot filament *d* and said cold electrode so that the signal is made intelligible in the receiver or indicator 1?

Answer. No, I would not express it that way. I would rather state it that when the signal is impressed in one direction upon the electrode *c* it may produce a variation which is different from that produced when the signal is impressed in the other direction, that is responsible for the fact that the signal is intelligible in the receiver or indicator 1.

83 Cross-question. Regardless of how you prefer to express it, am I to understand from your previous answer [fol. 1690] that you consider that my previous question, cross-question 82, is an incorrect statement?

Answer. Yes; the way I regard it, it is an incorrect statement.

84. Cross-question. In what respect do you regard it incorrect?

Answer. It is not the variation in the electron stream produced by the signal that makes the signal intelligible in the receiver or indicator, but it is the difference in variations in the electron stream for voltage variations in opposite directions that produces the signal which is intelligible in the receiver or indicator. The first of these I would consider to be a radio frequency variation in the electron stream which is incapable of affecting the receiver or indicator 1.

85. Cross-question. That is, I understand your last answer, you assent to the proposition that in the case referred to as illustrated in Figure 1 of the Fleming patent in suit, the action of the incoming signal, when applied to the cold electrode *c*, and also, of course, to the filament or hot electrode, as illustrated in said figure, does produce a variation in the electron stream between those electrodes?

Answer. Yes; I have stated this before.

86. Cross-question. It is also true, is it not, that when a 3-electrode De Forest tube is used in a detector circuit in the usual way, which I assume you will understand, the incoming signal, when applied to the cold grid electrode, and also, of course, the filament or hot electrode, produces a variation in the electron stream between those electrodes?

Answer. In one of the uses of the 3-electrode De Forest tube as a combined detector and amplifier, one of the things that happens is that the incoming signal does produce a variation in the electron stream between the grid and filament electrodes.

87. Cross-question. This variation last referred to, under the conditions previously stated, also produces a variation in the electron stream or flow between the filament or hot electrode and the plate or the other cold electrode, does it not?

Answer. Yes, this is another action which takes place simultaneously with the one discussed above. This latter action is due to the controlling action of the grid electrode upon the electron stream between the plate and filament electrodes or is an amplifying action, and this is the reason why I emphasize that the device in this use is operating both as a detector and an amplifier.

88. Cross-question. You will agree to the proposition, will you not, that there are a good many features of operation in common between the 2-electrode tube and the 3-electrode tube?

Answer. No, I would state that broadly and fundamentally the two devices are totally different in operation.

89. Cross-question. Both the 2-electrode tube and the 3-electrode tube involve or require an evacuated vessel, do they not?

Answer. They do.

90. Cross-question. Both the 2-electrode tube and the 3-electrode tube inclose and require one heated electrode or filament and at least one cold electrode, do they not?

Answer. They do.

91. Cross-question. In both the 2-electrode tube and the 3-electrode tube an electron stream or flow is produced between the heated electrode or filament and at least one cold electrode, is it not?

[fol. 1691] Answer. This question is so indefinite that I cannot answer it.

92. Cross-question. In any use to which the 2-electrode tube or the 3-electrode tube may be put, an electron stream or flow is produced between the heated electrode and one cold electrode, is it not?

Answer. This question is very indefinite; it does not state the agency by which the electron flow is supposed to be caused. As I understand it, the answer is no.

93. Cross-question. Do you mean to say that either a 2-electrode vacuum tube or a 3-electrode vacuum tube such as those of the Fleming and De Forest patents are capable of the use to which they are intended without the presence

of an electron stream between a heated electrode or filament and a cold electrode?

Answer. I do not.

94. Cross-question. Then as I understand you, either a 2-electrode vacuum tube or a 3-electrode vacuum tube such as those of the Fleming and De Forest patents are incapable of use for which they were intended without the presence of an electron stream between the heated electrode or filament and a cold electrode? Is that correct?

Answer. This is true of these devices in general, including the devices of the Fleming and De Forest patents.

95. Cross-question. The basis of the operation of both the 2-electrode and the 3-electrode tube depends upon the electron flow through a high vacuum, does it not?

Answer. They depend upon different characteristics of the electron flow through a high vacuum.

96. Cross-question. That is, I take it from your last answer you admit or agree that both the 2-electrode and the 3-electrode tube depend upon the electron flow through a high vacuum, although you consider that the flow may be of different characteristics, is that so?

Answer. I agree that both devices depend upon the electron flow. I would not say that the flow has different characteristics in the two cases, but rather that the operating conditions of the two devices are so different that two totally different characteristics of the electron flow are employed in the respective devices.

97. Cross-question. You are the John M. Miller who testified for the defendant in the case of Marconi Wireless Telegraph Co. of America *v.* De Forest Radio Telephone & Telegraph Co. in the southern district of New York during the accounting proceedings in that case, are you not?

Answer. I am.

98. Cross-question. You testified in that case, did you not, as follows (T. W. Rec., p. 444):

"The basic operation of both the 2 and 3 electrode tubes depends upon the electron flow through a high vacuum.

Answer. I believe that I did.

99. Cross-question. You also testified in that case during the accounting proceedings as follows, did you not? (T. W. Rec., p. 475):

"Q. And the operation of both the 2-electrode tubes and the 3-electrode tubes depends upon the impressing in some way of a receive*d* of incoming energy on the electron flow: that is to say, employing the incoming energy to modify the electron flow?—A. Yes."

Answer. I believe that I did.

[fol. 1692] 100. Cross-question. It is true, is it not, that the function of the 3-electrode tube, like the so-called De Forest audion, when used as a detector, is to convert the received signals of radio-frequency into an audio-frequency response that can be perceived, and in this operation it is similar in result to the crystal detector or coherer—that is, it performs a similar function of making the radio-frequency oscillations or signals observed either by the ear or by the recording or indicating device employed?

Answer. One of the functions of the 3-electrode tube or so-called De Forest audion when it is being used as a detector is to convert the received signals of radio-frequency into the form of response that would be audible if of sufficient intensity. In other words, it doesn't matter whether the signal is strong enough to be heard or not; if the device is producing this conversion it is operating as a detector. In this operation it is similar in result to the crystal detector. The coherer is a device with which I have had little or no familiarity.

101. Cross-question. On direct examination in answer to question 5 you quoted from the patent to White, No. 1393594, as regards the characteristic of the current curve shown in that patent as probably being due to the effect of slight traces of residual gas in the device and the consequent positive ionization. As concerns this statement, it is true, is it not, that practically all of the high-vacuum 3-electrode tubes of the present day contain some residual gas, such as referred to by Mr. White in his patent?

Answer. It is true that such a thing as a perfect vacuum is unknown. Therefore there is some residual gas present in all high-vacuum 3-electrode tubes. However, the practice in general is to evacuate 3-electrode tubes in general so that the gas plays no appreciable part in the operation of the tube under normal operating conditions. For one type of tube this may require the higher vacuum than for another, depending upon the operating conditions. In fact, as I stated in this discussion, the very absence of this char-

acteristic which White employed for oscillation generation is a very general test employed to show that the various devices have a suitable vacuum.

102. Cross-question. You do not find in the White patent, do you, any statement that he used anything in the way of a vacuum tube in producing his results described therein, other than a good 3-electrode vacuum tube of usual construction and degree of vacuum employed in radio circuits at the time he obtained his patent or filed his application?

Answer. Mr. White states that the operation which he was describing depended upon ionization of the residual gas. He does not state whether or not he considers the tube to be a good tube or not with respect to vacuum, but in general the criterion, as far as I know, has always been that when gas ionization enters appreciably into the operation of the tube it is not considered to be a high vacuum tube.

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103. Cross-question. Will you please answer my previous question specifically in the affirmative or negative.

Answer. I do. Mr. White points out that the operation which he obtained was due to the ionization of residual gas in the device.

[fol. 1693] 104. Cross-question. You have indicated in one of your previous answers that practically all tubes having a high vacuum have some residual gas. What reason, if any, have you for believing, if you do so believe, that there was any more residual gas in the tube used by White than is contained in the 3-electrode high vacuum tube in general use at the time of the White patent or used at the present day?

Answer. Prior to 1918, and through Mr. White himself, I had received and operated vacuum tubes manufactured by the General Electric Co., by whom Mr. White was employed, and I know that these tubes had vacua far superior to the tube disclosed in this White patent. I also know, by having made numerous measurements upon present-day high vacuum 3-electrode tubes utilizing this same White characteristic as a test, that present-day tubes intended to be operated at a voltage as high as that employed by Mr. White have a vacuum far superior to the vacuum in the tube of the White patent.

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105. Cross-question. From the last sentence of your answer I take it that you agree that the present-day tubes of high vacuum utilizing this same White characteristic can be operated as White described, in his patent No. 1393594?

Answer. Present-day tubes are not utilized in this way, and I do not agree that they can be utilized this way as long as the operating conditions for which the tubes are designed are adhered to. This is my belief by reason of the fact that, at least, as far as the Government is concerned, vacuum tubes are purchased on the basis that this White characteristic shall be present to a negligible degree. Any tube which under standard operating conditions would operate by reason of this White effect I would say is not a high vacuum tube.

106. Cross-question. Then, as I understand your position, it is that a modern, present-day high vacuum tube having the usual amount of residual gas present in such tubes will not operate as White described in his patent. Am I correct?

Answer. If it did it would not, in my mind, be a high vacuum tube.

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107. Cross-question. Will you please answer my question specifically, affirmatively or negatively?

Answer. If it is a high vacuum tube, as I regard it, the answer is no.

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Deposition of Edward H. Loftin (recalled), for defendants, taken at New York City on the 26th and 27th days of November, A. D. 1928, December 21, 27, 1928, and January 3 to February 4 to 11, 1929.

Direct examination.

By Clifton V. Edwards, Esq.:

[fol. 1694] Edward H. Loftin, being recalled as a witness on behalf of the defendant, testifies in answer to interrogatories as follows:

1. Question. Are you the same Edward H. Loftin who has heretofore testified in this case?

Answer. I am.

2. Question. Have you read that part of the testimony of Roy A. Weagant, a witness for the claimant, in which he describes certain experments which he considers to demonstrate that a 2-electrode vacuum tube is inherently capable of producing oscillations and amplifying?

Answer. I have, and I witnessed the experiments.

3. Question. Please consider the testimony of Mr. Weagant and experiments witnessed by you, and say if you agree that Mr. Weagant demonstrated the capability of the 2-electrode vacuum tube to generate oscillations and to amplify and explain your reasons.

Answer. I do not agree that Mr. Weagant demonstrated the capability of the 2-electrode vacuum tube to generate oscillations and to amplify. To make my reasons for my opinion clear some explanation of the mode of operation of vacuum tubes and the experiments of Mr. Weagant is necessary. In question 4 to Mr. Weagant (printed record, 1527) he was asked to demonstrate the capability of *the* hard or high vacuum 2-element tube to oscillate and receive broadcast signals. He did not demonstrate *the* 2-element tube, but *a* particular and selected construction of such a tube, and the result of his careful selection of the construction of *a* particular tube I will bring out in detail later.

To understand the Weagant experiments a few of the fundamentals of the operation of vacuum tubes is helpful. A vacuum tube ordinarily comprises a glass vessel inclosing a highly evacuated space, the evacuation being carried to the extent that gas atoms are so few in the space that their presence is in effect negligible. There is included in the evacuated space a filament or other form of electrode which, when heated, is capable of emitting electrons in profuse number into the space surrounding the filament, these electrons originating in the atoms comprising the material of which the filament or emitting substance is composed. The filament is generally heated by the heat generated by an electric current passed from the outside of the tube through that part of the filament inclosed by the tube, and this heat sets the electrons of the atoms in the filament material into such activity that some of the electrons are accelerated into such velocity of motion that they break through the surface resistance of the atoms and fly out into space surrounding the filament, thus creating a cloud or field of electrons about the filament.

These electrons are extremely minute, there being many of them in each atom of matter, and are said in electrical terms to represent the smallest division of electrical energy. When an electron parts from its mother atom, as it does in flying out of the filament of the vacuum tube, there is immediately set up by the mother atom an influence, potential or force, tending to cause the departed electron to return. The electrical man refers to the electron as a negative element of electricity, and looks upon the atom from which one or more of electrons have departed as being a positive element of electricity because of the absence of one or more [fol. 1695] negative elements. It is thus seen that in the case of a heated electron emitting filament in a vacuum tube there is a continuous flying out into space of electrons accelerated into action by heat, and the maintenance of a force between the stationary atoms of the substance of which the filament is composed and the field of electrons thereabout, the field of electrons being native, electrically speaking, and the filament being positive. The result is that some of the electrons in the field are traveling away from the filament while others are traveling toward the filament.

Electrical current, whether it flows in wires or conductors, or through space, as in a vacuum tube, is merely the movement in these media of electrons, so that in the vacuum tube having a heated electron emitting filament there occurs simultaneously flow of electrical current away from and toward the filament.

The 2-electrode vacuum tube has, in addition to the electron emitting filament, a second electrode physically spaced from the filament, this second electrode being generally cylindrical or otherwise shaped to completely or substantially surround the emitting filament, and thus capable of acting, when called upon to do so, upon the greater part of the total electron emission from the filament.

The description so far is all that is needed to completely describe *the* 2-electrode vacuum tube. What this device is capable of doing depends entirely upon circuits and other associations externally thereof, and various adjustments of these circuits and associations.

If a circuit is connected externally between the electron emitting filament and the second electrode, which I will hereafter term the plate, to conform to the usual designation of the art, a variety of effects can be produced from

the electron field surrounding the filament, depending upon character and kind of electrical forces introduced on the plate through the aid of electrical potentials applied to the external circuit. For example, if the plate be given a fixed, steady, positive potential, as by connecting a battery in the external circuit, this positive plate potential will compete with the positive character of the filament for control of the movements of the space electrons, with the result that electrons will be drawn to the plate across the intervening space in number in proportion to the relative strengths of the positive influences existing on the plate and filament, and the electrons drawn to the plate will then return to the mother atoms in the filament by way of the external circuit, and this movement of electrons in the external circuit constitutes a current flow that is evident with the aid of any of the instruments devised for detecting current flow, such as ammeters, galvanometers, and the like. Obviously, the greater the potential applied to the plate or, in other words, the greater the potential of the battery in the external circuit, the more will be the number of electrons which move across the tube space to the plate, and correspondingly the greater the current in the external circuit.

It is thus seen that by adding to *the* 2-electrode vacuum tube *per se* an external circuit, and including in this external circuit a source of potential, the device becomes a different element in that there now does not exist merely a field of electrons surrounding the filament and flying out from and back to the filament, but now numbers of these electrons are engaged in constituting a constant stream of electrons, or current flow, to the plate, to ultimately pass [fol. 1696] through the external circuit to form a current flow in a wire or conductor where instruments and devices can be inserted for practical utilization of current flow effects.

Another fundamental effect that should be in mind is that the number of electrons diverted from the filament region by an electrical potential applied to the plate is not directly proportional to the potential applied to the plate except over but a portion of the range of potentials employed in practice. At the beginning of applying a plate potential it is obvious that the opposing potential of the filament predominates, so that the plate receives only a few electrons. As the plate potential is increased the predominancy of

effect gradually transfers from filament to plate, so that the rate of increase of electron flow to the plate increases out of proportion to increase of plate potential until a plate potential is reached where the predominancy is substantially all on the side of the plate potential when the rate of increase of electron flow to the plate is substantially proportional to the increase in plate potential, this until an approach is made to the point where the plate potential begins to take all of the electrons that are being omitted by the filament under the condition of heating thereof, it being obvious that if there are no more electrons available further increases of plate potential will not increase electron flow to the plate. These effects are usually illustrated in a curve known as a plate current characteristic curve, which is substantially straight in the middle and curved at both the lower and upper ends. In practice the curved portions have definite uses, and the straight portion has a definite use. It is thus obvious that in operating a vacuum tube for some purposes choice must be made as to the potential applied to the plate in addition to actually applying the potential to start an electron stream across the space within the tube.

In addition to the application of a steady, fixed potential to the plate as above described, uses are made of tubes involving the application of potentials which are alternately positive and negative, as by applying an alternating current potential effect to the external circuit connecting the plate and filament, and some of the fundamental effects resulting from the application of alternating current potential should be kept in mind. Considering that the cycle of events in an alternating current starts with zero or no potential, gradually builds up to a maximum positive potential, positively declines back to zero potential, negatively builds up to maximum negative potential, and finally negatively declines to zero potential, and so on cycle after cycle, it is obvious that as the plate first becomes positive electrons will be drawn from the field around the filament toward the plate, some of them passing through the external circuit. As the positive potential declines the force on these electrons en route to the plate will gradually be released, so that the movement of electrons toward the plate will recede. As the plate becomes negative it will actually repel electrons and force them away, and at the same time

the positive filament will aid this action, so that there will, in fact, be a reverse flow or movement of electrons within the tube space. It is obvious, however, that since a part of the electrons passed into the plate during the positive half of the cycle, there can not be as much backward movement of electrons during the negative half of the cycle as there [fol. 1697] was forward movement during the positive half of the cycle. The effect of this is that more current flows in one direction during the positive half of the cycle than there is current flow in the reverse direction during the negative half of the cycle, resulting in an unbalance or distortion commonly referred to as "rectification." It is pointed out, however, that this rectification is not complete to the extent that there is no current flow in the reverse direction, the effect being merely a partial one. This distortion or partial rectification effect is useful in what is known as "detecting" the very high frequency current effect used in radio signaling, as well as having other practical values.

It is also possible to simultaneously apply a steady, fixed potential to the plate and an alternating current potential, with the result that there will be a main body of electrons continuously flowing from the electron field about the filament to the plate which is modified or varied in accordance with the superimposed alternating current potential on the steady potential. When this is done the degree of distortion or partial rectification of the alternating current potential effect depends greatly upon the value of the initial steady potential, and at that portion of the plate current characteristic curve arrived at by fixing the steady potential where the curve is substantially straight distortion or partial rectification is substantially eliminated, so that a 2-electrode tube so connected and so energized with a fixed direct current potential is substantially useless in the matter of giving distortion or partial rectification to aid in detecting the alternating currents of high frequency common to radio practice, and for other purposes where partial rectification is needed.

There are, therefore, two conditions under which the 2-electrode vacuum tube may be operated. (1) With the alternating current applied to the plate without the presence of a steady direct current potential on the plate, in which condition there is partial rectification sufficient to be of practical utility; (2) with alternating current applied

to the plate in the presence of a steady, direct current potential applied to the plate, in which condition there may or may not be sufficient partial rectification to be of practical utility, depending upon the degree of direct current potential on the plate.

In either mode of operation there can be no amplification. Amplification requires that the result exceed in magnitude the cause. In other words, to have amplification out of a system involving a 2-electrode tube the current that flows in the external circuit as a result of applying an alternating current potential to the plate must exceed the applied alternating current needed to develop the actuating potential. Since the 2-electrode tube and circuits does not differ from any other physical machine in that losses are introduced in bringing about operation, it is obvious that some of the energy of the applied operating alternating current is lost, and the result or effect is less than the cause, but the result may include the partial rectification so necessary for some purposes.

As to the production of oscillation, it is an axiom in electrical practice that oscillations can not be produced without amplification, and since it is impossible for the 2-elec- [fol. 1698] trode tube to amplify because of losses which can not be recovered, it is likewise impossible for such a tube to produce oscillation.

In order for a tube to be able to amplify and, with the aid of amplification, to be able to oscillate, there must be means provided for controlling the action of a large amount of energy by the expenditure of a small amount of energy, which effect is no different in principle from that employed in steam engines where a small amount of the steam energy is employed to operate the valve mechanism which controls the large amount of steam acting on the piston to develop large energy for work purposes. In the tube if a large amount of direct current potential is applied to the plate to maintain a large stream of electrons continuously flowing from the electron field about the filament to the plate, there is thus available a large amount of energy from which amplification can be produced if the energy can be controlled by the expenditure of a small amount of controlling energy. I have previously pointed out how the electrons in the tube respond to potentials developed at the filament by the electron emission action and at the plate by poten-

tial from an external source. Obviously, the electrons can be influenced by other potentials, and if the influence of other potentials can be made greater with small expenditure of energy, then the influence had by the potential on the plate for a given amount of potential, then amplification can be produced at the expense of the energy-delivering device employed to apply the potential to the plate, as for example, the usual so-called plate circuit or "B" battery. It is also physically obvious that where an attempt is made to control movement the more intimate the relation between the thing controlled and the controller, the more effective is the resulting control. De Forest, often mentioned in this case, produced the 3-electrode tube as a device permitting of most effective control of the electron stream or flow to the plate in a vacuum tube by most correctly inserting the third or control electrode directly in the path of flow of electrons from the region of the filament to the plate, and it turns out that by expending a very small amount of electrical energy in developing a potential on this De Forest control electrode a large effect can be had on the flow of electrons to the plate, so much so that a high order of amplification is obtainable with the De Forest 3-electrode tube. Obviously, the amount of control effect and resulting amplification is reduced by altering the degree of controlling relation between the electron stream and the controlling potential. This third and controlling element of De Forest has been popularly termed the "grid," this because he early suggested a grid construction for the third electrode because of its insertion directly in the electron stream, and not wanting to physically obstruct the flow of electrons, but merely to electrically control their movements with the aid of potential. Obviously, if the grid or its equivalent is gradually removed from the main electron stream position of De Forest, the control effect will gradually decrease, thereby reducing amplifying ability of the tube. Mr. Weagant produced for the experiments I witnessed tubes so constructed in physical distribution that a grid effect located just outside of the wall of the glass container would still have enough control effect on the electron stream between the filament region and the plate in the tube to produce a fair order of amplification, but not that order of amplification securable with the correctly constructed De [fol. 1699] Forest 3-electrode tube, and by reason of this

special construction was enabled, by bringing elements of the circuits he used close to the wall of the tube immediately adjacent the electron stream, to bring about noticeable amplification and even oscillation with the aid of the De Forest effect.

In order to bring to bear the De Forest control effect Mr. Weagant departed from the usal tube construction of a globular glass container of proportionately large diameter inclosing a heated filament almost completely surrounded by a cylindrical or like shaped plate, to a highly elongated cylindrical tube of small diameter, and so located the filament in one end of the tube and the plate in the other end of the tube that the main electron stream would be rather long and completely exposed and near to the walls of the tube. In his testimony Mr. Weagant states that the tubes are approximately 4 inches long, 1 inch in diameter, and that the filament and plate separation of the tubes used for amplifying and oscillation was about three-quarters of an inch, though the separation appeared to be somewhat greater to me. He further testified, and I so observed, that the plate comprised a circular plate of diameter almost as large as the internal diameter of the tube, and that the filament comprised a circular arrangement of wire, the circle being of large diameter, thus providing for a large part of the electron stream being closely associated with the internal walls of the tube. It is thus easily seen that an element brought close to the outside of the wall of the tube could, when energized with a controlling potential, secure a fair amount of the De Forest controlling effect.

That Mr. Weagant used these tubes ingeniously constructed to aid him in securing the De Forest effect in ways to produce this effect is evidenced by the apparatus employed and described by him, one test including the apparatus of claimant's Exhibit 224 and a diagram covering the connections thereof, claimant's Exhibit 225, the second test including the apparatus of claimant's Exhibit 226, covered by a circuit diagram, claimant's Exhibit 227.

In the case of the test of the apparatus diagramed in claimant's Exhibit 225 (opposite p. 1527) the condenser R of the antenna circuit was located immediately at that part of the wall of the tube *a* surrounding the space due to the wide separation of filament *b* and plate *c*, so that varying potentials occurring on the condenser R were intimately effective on the electron stream just inside the walls of

the tube. In this way, and with a special tube, Mr. Weagant was enabled to produce amplification with the apparatus demonstrated. Further, by so associating the coupling coil *m* in the antenna with the coupling coil *k* in the external circuit of the tube that there existed a feed back of energy in the right phase, the amplification produced by him was enabled to produce oscillations by the well-known feed back or regenerative principle now commonly used in connection with 3-electrode vacuum tubes. If Mr. Weagant had reversed the polarity of the coupling between coils *m* and *k* he would have destroyed the amplification producing effect and would have been unable to produce oscillation, thus showing that the effect he did obtain depends not only upon the tube, its particular construction, the production of an electron stream, the control of the stream, but upon the mode of connection of circuits as well.

[fol. 1700] Mr. Weagant's diagram of Exhibit 225 on its face does not disclose the true condition of his test, his failure to show the condenser R close to that part of the tube adjacent the electron stream, which could easily have been done, being an unpardonable omission from the point of view of a technical man. For example, I am certain from my experience in filing patent applications in the Patent Office that had Mr. Weagant submitted the arrangement of the apparatus tested by him to the Patent Office in the form of a patent application accompanied by the drawing of claimants' Exhibit 225 he would have been required to modify the drawing to show the condenser R located adjacent the proper part of the tube, even though he had fully set forth the location in his written description.

* * * * * * *

To make my point clear I have made a diagram of the apparatus of claimant's Exhibit No. 224 in a way I consider would clearly illustrate to the experienced man of the art the connections in fact of the Weagant test, which diagram I mark "Loftin Diagram of Apparatus of Claimant's Exhibit 224." My diagram readily suggests a special construction of the tube and the location of the condenser R in intimate relation with the electron stream in the tube, and the feed back effect between the external circuit of the tube and the antenna circuit in which the controlling condenser R is located, thus immediately suggest-

ing to one familiar with vacuum tubes and associated circuits the kind of operation to be expected.

In answer to question 4 (pp 1527-1528) Mr. Weagant pointed out that he caused the apparatus of claimant's Exhibit 224 to produce oscillations by changing the coupling between coils *m* and *k*, illustrated in claimant's Exhibit 225, thus conclusively proving that he did not depend upon any capability of the 2-electrode tube *per se* to generate oscillations, but upon a feed-back connection between the external circuit of the tube and the antenna circuit in order that amplified energy could be fed back to amplify the controlling potential on the condenser R located adjacent the electron stream in the tube.

In view of the above it is my opinion that Mr. Weagant, in producing oscillations with the apparatus of claimant's Exhibit 224, did not demonstrate the capability of the 2-electrode tube *per se* to amplify and oscillate—that the result was obtained by a multiplicity of modifications and associations with the tube as follows:

(1) A very special construction of tube.

(2) Injecting a direct current potential (battery P, claimant's Exhibit 225) in the external circuit of the tube to change the electron condition within the tube from that of a field about the filament to a continuous stream of electrons from the region of the filament of the plate.

(3) Placing the condenser R in intimate relation with the electron stream in the tube to control the electron movement thereof.

(4) Coupling the external circuit of the tube with the antenna circuit with proper polarity to create potentials of correct polarity on controlling condenser R to give feed back of right phase to give feed-back amplification up to oscillation production.

[fol. 1701] In his test of apparatus of claimant's Exhibit 226, including four tubes connected in cascade, Mr. Weagant did secure noticeable amplification, but again, it was not by reason of any capability of the 2-electrode tube to amplify.

In this test he again used tubes of the very special construction I have referred to. As before, he employed a battery in the external circuit to create a continuous electron stream within the tube. He made the slight change of submitting a coil of wire located in intimate relation with

the electron stream in the tube for control of the electron movement instead of the condenser in the preceding test, which substitution makes no difference in the resulting electrical influencing effect. In this test Mr. Weagant availed himself not only of the amplification due to the De Forest effect of bringing to bear a controlling influence on the electron stream, but of further amplification due to feed back by reason of the particular type of circuits used, illustrated in the diagram of claimant's Exhibit 227 (opposite p. 1529). This circuit, as illustrated, is one commonly termed "tuned radio frequency," the circuit in general use for broadcast radio receivers. The great practical difficulty with these broadcast receivers is to so construct them that there will not be enough feed-back amplification to cause oscillation. This feed-back amplification in tuned radio frequency is caused by the internal capacity of the tube between the plate and grid electrode where the ordinary De Forest type of tube is used, it being well known that the amplified energy in the output circuit flows back to the controlling grid electrode by way of this internal capacity to reinforce the influencing potential on the grid electrode. The absence of a grid electrode in the tubes used by Mr. Weagant in his test does not obviate this capacity effect that permits feed back. For example, looking at tube V_1 in claimant's Exhibit 227 it is seen that the upper or plate electrode is diagrammatically illustrated to have rather large area, and that the coil L_3 is placed close thereto. The surface area of the wire of coil L_3 forms a capacity-creating area with the area of the plate electrode fully commensurate with the capacity existing between the plate and grid of an ordinary De Forest 3-electrode tube, so that there is provided in the system of plaintiff's Exhibit 227 the same feed-back effect for increased amplification as exists in the ordinary tuned radio frequency receiver.

In answer to cross-question 112 and cross-question 114 (p. 1538) Mr. Weagant testified that the spacing between the detector tube V_4 in claimant's Exhibit 227 was but one-quarter of an inch, and that the lesser spacing operated better as a detector, and that the greater spacing operated better as an amplifier. It is my opinion that had Mr. Weagant tried to use tubes of the lesser spacing given to the detector tube in the amplifier position that he would have been unable to show any noticeable degree of amplification.

With the plate so close to the field of electrons about the filament, the influence of the plate potential on the electrons would have so far predominated over the influencing potential coming from the coil outside the tube that the amount of controlling influence would have been of no practical effect, which brings out the great importance of the third electrode or controlling effect in the matter of securing amplification.

[fol. 1702] I further point out that Mr. Weagant applied 180 volts direct-current potential to the plates of the amplifier tubes, an extremely high potential for vacuum-tube operation in connection with radio-frequency amplification, and only used about 6 volts on the plate of the detector tube, all evident from the data given on the diagram of claimant's Exhibit 227. This practical operation emphasizes what I have previously said as to the necessity for choosing proper potentials for the plate electrodes of tubes operating for different purposes. The high plate potential on the amplifier tube permitted of operating these tubes on the strait portion of the characteristic curve, where there was little or no practical partial rectification, and so operated, these tubes would be substantially useless as detectors. With the low voltage of 6 on the detector tube, the operation occurred at a curved portion of the characteristic curve, thus bringing in partial rectification, making detection practical. So operated, the arrangement would be practically useless as an amplifier even had Mr. Weagant used a tube of great spacing between the plate and filament and had brought to bear the controlling influence of the coil on the space between these elements.

In view of the above it is my opinion that Mr. Weagant did not produce amplification in his test of claimant's Exhibit No. 226 by reason of any capability of the 2-electrode tube *per se* to amplify, but did do so with the aid of specially constructed tubes, the development of a controllable electron stream therein, the location of apparatus to introduce the De Forest controlling effect on the electron stream, and by the use of circuits making feed-back amplification possible.

* * * * * * *

4. Question. Do you find disclosed in the Fleming patent in suit any description or suggestion of the apparatus as

demonstrated by Mr. Weagant? Please state your reasons for your answer.

Answer. No. First of all the Fleming patent 803684 in suit shows a 2-electrode tube so connected with circuits that no provision is made for creating a continuous electron stream from the region about the filament to the plate. Referring to Figure 1 of the Fleming patent, or example, it is seen that the wire *j* connects the plate *c*, a cylinder surrounding the filament *b*, to the wire *e* which passes into the tube to the filament *b*, and that there is not included in this external connection any source of potential, such as a battery, for applying a positive potential to the plate absolutely necessary to the creation of an electron stream. Furthermore, this wire *j* in its connection to heating current wire *e*, is shown connected to the negative side of the heating current battery *h*, as a result of which the plate *c* is negative with respect to the average potential of the filament *b* by one-half of the potential of heating current battery *h*, which negative potential of the plate results in repelling electrons from the plate rather than creating a continuously moving stream of electrons to the plate.

[fol. 1703] If Mr. Weagant had connected the tube or tubes in his two tests in the manner hown in the Fleming patent, it would have availed him nothing to introduce the De Forest effect by bringing a condenser or coil of an external circuit close to the tube, as, since there would have been no electron stream to control, the controlling influence of the coil or condenser would not have been effective. The continuously moving electron stream and the energy it represents is an absolute essential to the amplification and oscillation obtained by Mr. Weagant, and the Fleming patent is devoid of this essential feature.

Again referring to Figure 1 of the Fleming patent, it will be noted that the glass vessel *a* of the tube is of relatively large diameter in proportion to its length; that the plate *c* is shown widely spaced from the wall of the tube; and of extreme importance, the cylindrical plate *c* almost completely houses the region about the filament *b* which would be occupied by emitted electrons, thus shielding the electrons from any possibility of useful external influencing. Thus, if Mr. Weagant had constructed a 2-electrode tube in anywise resembling that illustrated in the Fleming patent, and had been entitled to produce an electron stream

therein by some definite disclosure in the Fleming patent, he still would not have been able to introduce with such tube the control effect of De Forest which he successfully did with the tubes of his special construction.

Again, reference to Figure 1 of Fleming shows that he did not include any condenser in the antenna circuit *n* such as the condenser R included in Mr. Weagant's apparatus of the first test as shown in the diagram, claimant's Exhibit 225, clearly showing that Fleming in nowise contemplated any such procedure as that resorted to by Mr. Weagant in using the potential developed on condenser R as a controlling influence.

Referring to the diagram plaintiff's Exhibit 227, illustrating the circuit arrangements in Mr. Weagant's second test, it will be noted that the coils L_1, L_3, and L_5, used for influencing the electron streams in tubes V_1, V_2, and V_3, respectively, are connected in special or auxiliary circuits instead of in the circuit connecting the plate to the filament. No such special connections are shown or hinted at in the Fleming patent. In Figure 1 of Fleming there is shown a coil *k* connected in the circuit between the plate and filament. If Mr. Weagant had connected his coils in the same way and placed them close to the tube, even though the tube was of a special construction used by him and included an electron stream, no result such as that obtained by him by the introduction of the De Forest effect would have been obtained.

In my opinion Mr. Weagant went far beyond the disclosure in the Fleming patent in bringing about the results demonstrated by him, availing himself of knowledge and practice of various kinds which became available to the art many years after the filing of the application for the Fleming patent; that the Fleming patent is wholly incapable of producing, and fails to provide information for producing the amplifying and oscillation production results of the tests of Mr. Weagant.

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[fol. 1704] By Mr. Edwards. The sketch made by the witness in answer to question 3 is offered in evidence as defendant's Exhibit V-6.

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[fol. 1705] 9. Question. In his answer to question 74, printed page 1802, the witness Aull refers to experiments

by him in which he alleges to have used a "2-element electron tube" for producing oscillation. Do you agree that the experiment described by him depended upon a 2-element tube?

Answer. No. The answer of Mr. Aull makes it clear that he merely duplicated the work of the witness Weagant, and I have fully shown in the opening of my present testimony that these tests of the witness Weagant were not tests with a 2-electrode tube but in fact depended entirely upon 3-electrode tube action. The witness Aull states in the answer to question 74 that the result was obtained by placing the tuning inductance near the tube he used, and this, of course, was in order to control the electron flow in the tube just as was done by the witness Weagant.

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[fol. 1706] 11. Question. In his answer to question 8, page 1312, printed record, the witness Sarnoff for claimant refers to "a Marconi multiple tuner," and further refers to the arrangement as the "Franklin" tuner, and illustrates this device in Figure 4 of claimant's Exhibit No. 151, opposite printed page 1310. Further in his answers to questions 11 and 12, printed page 1313, the witness Sarnoff refers to this device as Marconi design or origin. At printed page 1358 claimant's witness Taylor refers to the same arrangement, and in his diagram of claimant's Exhibit No. 163 (opposite page 1359) entitles this device as "Marconi multiple tuner." Is it your understanding that the circuit arrangement referred to by these witnesses and illustrated in their diagrams originated with Marconi or personnel of the Marconi Co.?

Answer. No. It is not entitled to the designation "Franklin," an engineer of the Marconi Co., given it in one instance by the witness Sarnoff, or the designation "Marconi" given it by both the witnesses Sarnoff and Taylor. The circuit arrangement in question is most correctly popularly known in this country as the "Stone weeding out circuit," and so far as I am aware was first suggested for radio uses by John Stone Stone, and was shown by him in many forms of use in his United States patent 714756, defendant's Exhibit P-3, a tuning patent filed prior to the priority date of the Marconi tuning patent in suit. This weeding out or intermediate circuit arrangement is shown by Stone in his early patent in a number

of forms in Figures 7, 8, 9, 13, 14, 15, 16, and 17, and is clearly described by Stone as having the same purpose for which it was later used by the Marconi Co. and others, namely, securing a high order of selectivity through cascading or multiplying a number of tunable circuits.

12. Question. Claimant's witnesses Brennan, Weaver, Reoch, Hartley, Isbell, Rabbitts and Reinhard have given testimony as to their early uses of Fleming valves as detectors at wireless receiving stations and alleging comparisions with crystal detectors. State whether or not you have read the testimony of these witnesses, and if you have what have you to say as to conditions under which these comparisons were made.

Answer. I have read the testimony of all these witnesses, and it all relates to experiences had by them 10 or more years ago, and it all clearly shows that the experiences had by them these many years back were not had with any intention of going into the matter of the relative merits of Fleming valves and crystals, but rather that they, as radio operators, used in the ordinary course of their operating duties such apparatus as was supplied them. No one of the witnesses offers any testimony showing that at the time of the experiences he had any particular interest in the subject about which he was called to testify many years after. The collective testimony of them brings out that the practical use of the Fleming valve as a detector in Marconi installiations did not endure for very long.

In general this group of radio operator witnesses was asked to compare the sensitivity of the Fleming valve as a detector with the crystal detector. In general they claimed [fol. 1707] experience with carborundum as a crystal detector and claimed greater sensitivity for the Fleming valve over carborundum detection. It is pointed out that carborundum is generally recognized to be one of the least sensitive of the crystal elements which have been proposed for radio detection purposes, and the testimony in this case leaves it clear that sensitive crystals, such as galena, silicon, pyrites, and combinations of them, are superior to Fleming valves as detectors.

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13. Question. In answer to question 8, page 1473, claimant's witness Lehr testifies as to radio sets installed in

United States naval ships received from the British Government. What have you to say as to this testimony?

Answer. In my previous testimony I testified that subsequent to 1910 I had not encountered in the Navy use of the open-type gap radio transmitter except on United States naval vessels assigned to duty with the British Grand Fleet during the war, and that I had visited these United States naval ships in British waters and had seen on them special radio transmitters of this type secured from the British Government. I visited these ships in January or February, 1918. I note in the testimony of the witness Clark for claimant in answer to question 58, page 1501, he states that early in 1918 he was detailed by the Bureau of Engineering, Navy Department, to visit a British naval ship in Canada, and during this visit acquired the details of the type of open-gap radio transmitter I mentioned, and it was not until subsequent to his visit that effort at production of these transmitters in the United States was commenced. In view of Clark's testimony and the dates involved, it is obviously conclusive that the transmitters I saw on the United States naval ships with the British Grand Fleet were secured from the British Government as I was so told at the time of my visit.

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14. Question. Please refer to the testimony of claimant's witnesses Weagant, in answer to question 206, page 1634, and Hogan, in answer to question 22, page 1835, and say if you are in accord with their reasons why the secondary circuit in Marconi patent 627650 is not tunable as you pointed out in your previous testimony.

Answer. I am not in accord with the reasons given by claimant's witnesses Weagant and Hogan in their attempted endeavor to set up conditions around which to argue that the secondary circuit in Marconi patent 627650 is not tunable as intended by Marconi. The Marconi patent constitutes a clean-cut disclosure of adjusting the time periods of the antenna circuit and the secondary circuit at a receiving station to accord with the wave length of received oscillations, the language of patent 627650 being so clear as to features to be carried out to obtain the result as to leave no shadow of doubt as to the completeness of the disclosure. Because of the most definite instructions of Marconi in 627650 to tune both circuits the

witnesses Weagant and Hogan resort to contentions which, [fol. 1708] if true, would wholly vitiate the testimony of practically all of claimant's witnesses that disclosures in Marconi patent 763772 are disclosures for tuning the antenna circuit and secondary circuit in a receiving system.

The witness Weagant first points out that the coil j' of "Loftin sketch M" is not shown variable in the Marconi patent 627650, but he makes no reference to the fact that this coil is clearly described as variable, and variable for tuning, in the specification of 627650, page 1, lines 31-37 as follows:

"It is desirable that the induction coil should be in tune or syntony with the electrical oscillation transmitted, the most appropriate number of turns and most appropriate thickness of wire varying with the length of wave of the oscillation transmitted."

Thus, the instructions for making the coil j' variable, and variable for tuning, could not be more complete, and the comment of the witness Weagant is obviously a pure quibble, and also unquestionably unfair as I included in the subject matter of the legend accompanying my sketch M the informatory notice that my sketch was based on "Figure 1 and description" of patent 627650, and I did this to foreclose against a quibble such as that indulged in by the witness Weagant.

The witness Weagant next quotes the clear instructions in patent 627650 for varying the capacity of the condenser in the secondary circuit as the wave length of the incoming signal oscillation is varied. He does not deny that these are definite instructions intended for tuning the secondary circuit as in Marconi patent 763772 in suit, but tries to take refuge in a contention outside of the disclosure of patent 627650 as follows:

"Inspection of the diagram shows that condenser K is in series with induction j^2 and detector T and, due to the perfectly well-known fact that the resistance of detector T is relatively high, a condenser connected in the position shown at K is totally incapable of tuning the receiving circuit and it seems to me incredible that Mr. Loftin should not be aware of this fact."

I can not overemphasize the wildness and desperate character of this refuge of the witness Weagant of the location of the detector T and its relatively high resistance as a basis for contending that the tuning prescribed and provided for by Marconi in patent 627650 is not obtainable in view of the fact that the contention runs counter to the testimony of practically every one of claimant's witnesses in this case.

The doctrine that tuning exists in spite of the presence of the high resistance of a detector in a circuit begins with the opening testimony of claimant's own expert, Waterman, in this case in connection with Waterman's description of Marconi reissue patent in suit. On page 162 Waterman states:

"The idea of tuning as set forth in this reissue patent was therefore the idea of constructing the receiving station so that it should be identical with the transmitting station, to the end of localizing at the receiver the messages from this particular transmitter."

It is clear in this case that the receiving circuit of the Marconi reisue patent comprises simply a wire having a coherer (the high-resistance detector objected to by Weagant in connection with Marconi patent 627650), in series therewith, and Waterman does not hesitate to ascribe [fol. 1709] tunability to this receiving circuit of this first Marconi patent in spite of the presence of the high resistance of the detector. Again in his opening testimony in connection with Marconi patent 763772 Waterman shows in plaintiff's Exhibit No. 99, opposite page 157, four separate receiving systems, and ascribes to all of them the property of tunability of the secondary circuit. In the case of three of these circuits, namely, Figure G, Figure I, and Figure J, the detector is located in the secondary circuit in the same way as in Figure 1 of Marconi 627650; that is, in series relation with an inductance coil and a condenser. Waterman testifies that such a circuit will tune and, in fact, tuning of such a circuit is the very purpose and basis of this testimony. Weagant now testifies that such a circuit can not tune, and therefore Marconi's instructions in 627650 to do so are without effect.

Claimant's witness Sarnoff showed in his diagrams, claimant's Exhibit No. 151, opposite page 1310, radio receiving systems of Figures 2 and 2*a* in which a high-resistance detector is in series with the capacity and inductance elements of a secondary circuit, and Sarnoff testified to the effect that he used the apparatus in tuned condition.

Claimant's witness Taylor, who has testified that he was a construction engineer for the Marconi Co. in the very earliest days of the Marconi Co.'s radio operations, shows in the diagram of claimant's Exhibit No. 158, opposite page 1348, a radio receiving system having a secondary circuit in which the high-resistance detector is in series with the inductance and capacity elements forming the circuit, and Taylor testified that such a secondary circuit was used in a tuned condition. Taylor gave the same line of testimony in connection with the receiving system of claimant's Exhibit No. 160, opposite page 1351, and the receiving system of claimant's Exhibit No. 162, opposite page 1358, in both which cases a high-resistance detector is in series with the circuit.

The witness Weagant next suggests, page 1624, that Marconi 627650 can be made tunably operative by modification as follows:

"In order for condenser K to tune with inductance j^2, that terminal of the condenser K which goes to the detector T, should go to one terminal of inductance j^2 through a connecting path which does not contain a high resistance."

In other words, Weagant now contends that of the four receiving systems illustrated by plaintiff's expert Waterman in plaintiff's Exhibit No. 99, opposite page 157, the one of Figure II is the only one having tunability in the secondary circuit, this in spite of Waterman's testimony that all of them permit of the tuning claimed for The later Marconi patent 763772 in suit.

In this connection the testimony of claimant's witness Taylor as to the early practice of the Marconi Co. is significant and clearly refutes the erroneous testimony now presented by Weagant. On page 1360 Talyor was questioned and answered as follows:

"Question. Do you recall whether or not at any time, while you were in the employ of the American Marconi

Co., any means was provided for getting sharp tuning in the detector circuit of the receiver?

"Answer. Towards the end of the use of the coherer detector for commercial purposes, we used to put a variable [fol. 1710] condenser across the coherer itself, but that was only done occasionalt , it was not general practice."

The arrangement for obtaining the "sharp tuning" specified in the question is the arrangement of Figure 11 of Waterman in plaintiff's Exhibit 99, and Taylor makes it clear that Marconi practice of the early days was satisfied with the less sharp tuning of Marconi patent 627650, and only resorted to the modification now suggested by Weagant occasionally, but Taylor's testimony contends that this less sharp tuning now condemned by Weagant was tuning nevertheless.

Claimant's witness Cram, in his testimony at pages 1460 and 1461, contends that certain receivers of defendant, such as the E_4 telefunken type of plaintiff's Exhibit 79, opposite page 180, are tuned in the matter of the secondary circuit, this in spite of the fact that the E_4 and other receivers of this kind have high resistance detectors in series with the secondary circuit, and thus are subject to the high resistance features which the witness Weagant relies upon for contending lack of tunability in Marconi patent 627650. In my previous testimony I have stated that receivers of this type are not tuned, this not because of the series relation of the detector but because of the way in which they are built and used. The witness Cram and other witnesses of claimant in testifying as to these receivers of the E_4 kind have not contended any variability of the condenser j^3 shown in the E_4 secondary in plaintiff's Exhibit 79, as is provided in Marconi patent 627650. Cram and these other witnesses point out that the inductance coil j^2 is provided with a series of taps by which the inductance can be varied in large steps rather than continuously, and point to five to six of these large steps of inductance selection in some of these receivers. Obviously, the secondary circuit has one natural period for each one of these steps of inductance selection in these receivers. For example, for a practical receiver the natural period may be at 450 meters, 650 meters, 900 meters, 1,250 meters, 1,800 meters, 2,500 meters, and 4,000 meters

as the inductance tap is changed from one position to another. With the inductance tap such that the secondary circuit has a natural period of 450 meters, the operator may adjust the antenna circuit to each and every wave length from 300 meters up to 600 meters without the two circuits being at any time in tune except when the operator may by chance have occasion to tune the primary or antenna circuit to 450 meters. In other words, the operator may employ several hundred adjustments of his antenna circuit for the operation of the receiver with the one setting of the adjustment tap on the inductance of the secondary circuit without ever having the two circuits in tune. There is no doubt, of course, that the reception is best at that point where the adjustment of the tune of the antenna circuit may happen to accord with the natural period of the secondary circuit, but because of the high resistance of the detector T the resonance point is not particularly prominent, so that the operator would not note any pronounced increase in signal strength at this point as compared to a sharply tuned secondary circuit in which the detector is connected across the tuning condenser, but the [fol. 1711] resonance point does exist in spite of Weagant's contention to the contrary. If defendant had made the condenser j^1 in the secondary circuit of the E_4 kind of receiver variable as instructed in Marconi patent 627650 so that the tuning of the secondary circuit could be varied as the wave length of the incoming signals and the tuning of the primary circuit were varied, then this not sharply defined resonance point could have been moved from one part of the band of wave lengths to all others in consonance with the varying of the wave length of the incoming signal and the tuning of the antenna circuit to maintain continuous tuning throughout the band, and under such circumstances the witness Cram could have correctly testified that the E_4 and other like receivers of defendant were tunable for the purpose of Marconi patent 763772 in suit, but in the manner provided for in the earlier Marconi patent 627650. This reference to the Cram testimony thus brings out that Cram contends for tuning within the terms of Marconi patent 763772 in apparatus with provisions for tuning less than those in the earlier Marconi patent 627650 at the same time that Weagant contends that no tuning at all can be accomplished because of the high resistance of the detector element.

Claimant's witness Clark, pages 1503 and 1504, testifies in the same manner as claimant's witness Cram as to defendant's receivers having a secondary circuit of the kind typified in E_4 telefunken type of receiver, thus adding another one to the list of claimant's witnesses opposed to the erroneous contention now made by the witness Weagant.

The confusion and contradiction that claimant has injected into the matter of resonance and tuning circuits one to another through claimant's multitude of witnesses in an effort to save the Marconi patent 763772 from the tuning of the prior art and yet bring defendant's apparatus within the patent in suit is particularly typified in some of the testimony of the witness Clark. For example, on page 1515 in answer to cross-question 119 Clark stated that two circuits, one of which is tuned to a wave length of 900 meters and the other to a wave length of 895 meters, are not in "exact resonance." In his answer to cross-question 120 he stated that in the case of two circuits, one tuned to 900 meters and the other to 850 meters, he would "consider this difference in wave length to be beyond that tolerance allowable for the word 'resonance' in operation." Again, in his answer to cross-question 122 he stated that in the case of one circuit tuned to 200 meters and the other to 250 meters he would say that they are not in resonance.

But in his testimony included from question 63 to question 68, pages 1503 and 1504, Clark contended that receivers having secondary circuits of the type of the E_4 receiver are tuned. In his answer to question 68 he tabulated the wave lengths' ranges covered by the different taps or plug positions for the inductance in the secondary circuit. For plug position 1, he gives a wave length range of 200-500 meters. Obviously, if such a secondary circuit will effectively cover from 200 to 500 meters, it would have a natural period or tune in the neighborhood of 325 to 350 meters, so that from this data Clark in the position of contending that when the incoming signal is 200 meters and the antenna tuned to 200 meters, the fixed secondary circuit [fol. 1712] with its natural period of from 325 to 350 meters is in tune with, or resonant to, the signals and antenna circuit of 200 meters, a difference of from 75 to 100 meters beyond what he said would not be resonance in his answer to cross-question 122. Like discrepancies in the case of his answers to cross-question 119 and cross-question 120 can be pointed out in the case of the plug positions for the

longer wave lengths given by Clark in his answer to question 68.

Returning to the testimony of the witness Weagant as to the secondary circuit of Marconi patent 627650, page 1634, after denying the capability of the condenser K to perform the tuning function allocated to it by Marconi, Weagant tries to give to this condenser another function to justify its presence within the secondary circuit, and accordingly states:

> "The action referred to in the patent and quoted by Mr. Loftin is merely the ordinary action of what is known as the 'stopping condenser.' The purpose of condenser K is to stop the flow of the battery current from battery B, through the inductance j^2, which would short-circuit the battery, prevent potential being applied to detector T. Oscillating current does not flow through condenser K, but the rectified half cycle of the oscillating current, which are all in one direction."

It is difficult for me to reconcile the violent transgressions of technical and physical facts contained within this quotation with my familiarity with the witness Weagant's knowledge of the physical actions inherently involved. Weagant ascribes to condenser K the function of preventing the flow of current from the battery B through inductance coil j_2. With the position of condenser K shown it does so act, but no definite value of capacity of this condenser is required to bring into play the stopping function, so that it is immaterial whether the capacity of the condenser is extremely small or extremely large, and a condenser having all the requirements for tuning the secondary circuit prescribed by Marconi would adequately provide for the function of a stopping condenser and, at the same time, the variation of the capacity for the tuning function disclosed by Marconi would have no effect whatsoever upon the stopping function. Furthermore, the current from the battery B is simply a steady, one-direction current, so that any suggestion that the provision by Marconi for varying the capacity of this condenser as the wave length is varied has anything to do with a direct current from a battery is an untenable physical proposition. The quotation further contends that oscillating current does not flow through the condenser K, this in spite of the fact that the detector T de-

pends for its operation and function upon oscillating current and this detector is located on the opposite side of the condenser from the source of the oscillating current, namely, the antenna circuit. If, as contended by Weagant, the condenser K prevents oscillating current from reaching the detector T the system is obviously inoperative, but Weagant does not contend so far as this. Weagant further says that only rectified half cycles flow through the condenser K, and that these all flow in one direction. Marconi shows the detector as a coherer, the operation of which is not the partial rectification of crystal and other rectifying detectors, but one of the effect of high frequency currents on the filings within the tube to alter the resistance [fol. 1713] of the filings' path to the flow of direct current from the battery. Obviously, if the high frequency currents could not reach the filings' path within the tube the coherer would fail of response, and once more the system would be inoperative, which is not contended by Weagant. If the detector is made of the partial rectifying type, such as a crystal, it is still necessary for the high frequency currents to reach the detector, and this can only be done by flow through the condenser K directly in the path, as it is only through the high frequency currents reaching the rectifying device that partial rectification can be brought about. Furthermore, in the case of a rectifying device there are not as a result rectified half cycles, but merely a distortion or partial rectification, so that there is still high frequency current merely of greater amplitude during one half of the cycle than during the other half of the cycle.

The witness Hogan, in his answer to question 22, page 1535, also contends that the secondary circuit of Marconi patent 627650 is not tunable because of the high resistance of the detector and accounts for the presence of the condenser K on the same basis and the witness Weagant. Since the testimony of Hogan adds nothing to that of Weagant the proceding comments as to the testimony of Weagant apply as well to the testimony of Hogan.

*   *   *   *   *   *   *

15. Question. Explain the relation, if any, between the resistance in a circuit and the tuning of the circuit.

Answer. Resistance in a circuit has no effect on the tunability of the circuit in so far as any practical consideration is involved, so that tuning or adjustment of the natural

period of a circuit is accomplished by selection of inductance and capacity values in the circuit. In other words, having a circuit with a given relation or product of inductance and capacity the resistance of this circuit can be changed from very low values to very high values without changing the tuning or natural period of the circuit. The effect of changing resistance in a circuit is to change the responsiveness of the circuit to electrical effects, or in other words the amplitude or degree of current flow in the circuit in response to the impressed electrical effects. The effect of resistance in a circuit upon the conduct of the circuit in response to impressed electrical effects is readily understood with the aid of diagrams generally employed by electrical workers, and I have made a sketch entitled "Loftin Sketch R, Effect of Resistance on Response of Circuit" to aid me in explaining this matter.

In Figure 1 of the sketch I show a circuit 1 having an inductance coil L and a condenser C, shown to be variable for tuning or changing the natural period of the circuit. In series with this circuit I show a variable resistance R, [fol. 1714] the slider connection to the resistance indicating that all of the resistance of the element can be removed in effect from circuit 1 or any portion of it included as desired. I show a detector D, such as a crystal detector, and telephone T associated with circuit 1 as means for indicating the degree of response of circuit 1 to electrical effects. As a means for impressing electrical effects or oscillations on circuit 1, I show an antenna circuit A including a coil coupled to inductance coil L in circuit 1, and my succeeding comments contemplate a sufficiently loose coupling between the antenna coil and the coil in circuit 1 to avoid a distorted or double-hump energy transfer from the antenna to circuit 1 in order that resonance or tuning results may be actually had.

It is assumed that the antenna circuit A is excited with high-frequency currents of many frequencies as is the case with an ordinary radio receiving antenna because of the many radio transmitting stations scattered about generating electrical effects of different frequencies.

The curves in Figure 2 graphically show the degree of response or current magnitude in circuit 1 under conditions of different values of resistances inserted in this circuit with the aid of the slider on resistance R. Ordinates I in Figure 2 represent current intensity or response in

circuit 1 and abscissae or horizontal dimensions F represent frequency of either the currents impressed upon the circuit 1 or natural period of adjustment of circuit 1.

Assuming circuit 1 to be adjusted to have a natural period represented by frequency f_1 in Figure 2, as by adjusting the variable condenser C to arrive at that definite product of capacity and inductance to give this circuit the time period corresponding to the chosen frequency f_1, the effect of varying the resistance R while leaving the condenser C untouched is shown by the four response curves of Figure 2 marked "Very Low Resistance," "Low Resistance," "High Resistance," and "Very High Resistance." If the slider is moved all the way to the left of resistance R to entirely eliminate this resistance from circuit 1, there will still remain some effective resistance in the circuit by reason of the resistance of the wire from which coil L is made and the wires used for connecting the several elements into a circuit, but by proper construction this inherent resistance can be made very low, and such a resistance condition of circuit 1 will result in a response curve somewhat as illustrated by the "Very Low Resistance" curve of Figure 2. It is seen that this curve is very peaked or what is termed "sharp" in electrical practice, indicating a very marked and proportionately large response of circuit 1 to impressed alternating currents of the frequency f_1 to which circuit 1 is tuned. It will be noted that for any other currents in antenna A of higher or lower frequency than f_1 to which circuit 1 is tuned—that is, to the left and right of f_1 in Figure 2—the low-resistance curve shows that the response of current intensity of circuit 1 falls off very rapidly in proportion to the maximum current had by signals of frequency f_1. For example, I have shown a vertical dotted line erected at frequency f_2 in Figure 2, and by noting where this vertical line cuts the low-resistance curve a measure of the response of circuit 2 to a signal of frequency f_2 in antenna A when circuit 1 is tuned to frequency f_1 is had.

[fol. 1715] Now if the slider on resistance R is moved to the right to increase the resistance in series with circuit 1, but at the same time leaving condenser C untouched in order to maintain the same product of inductance and capacity in circuit 1, a family of response curves can be had as shown by the several curves marked "Low Resistance,"

"High Resistance," and "Very High Resistance," as the resistance of R is increased, but I call particular attention to the fact that this changing of resistance while leaving the condenser C untouched does not cause the points of maximum response of the curves to depart from the vertical dotted line representing frequency f_1 to which circuit 1 is tuned by reason of a proper selection of the product of inductance and capacity, and this is true for a change of resistance from the small value of a fractional part of an ohm to thousands of ohms. In other words, the changing of resistance in a circuit does not have any effect on the tuning of the circuit as determined by a selected product of inductance and capacity.

It will be noted, however, that varying the resistance does affect the response or magnitude of current flow in the circuit, the higher the resistance the less the response, but no matter how high the resistance the response at resonance is always maximum, or exceeds the response at other frequencies, so that even though the over-all response is reduced, and may be greatly reduced in the case of a high resistance circuit, there always exists a tuned or resonant response, though it may not be sharply defined.

For the reason that high-resistance circuits, though responding poorly throughout a wide range of frequencies as compared to low-resistance circuits, do not show a marked difference between the maximum response at resonance and lesser responses at widely different frequencies, as well shown in the "Very High Resistance" curve of Figure 2, such circuits are referred to in practice as "broadly tuned."

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In Figures 3 and 4 the effect of resistance in circuit 1 on the amplitude of current flow is shown. Figure 3 is intended to show the relative amplitudes of a current of frequency f_1 impressed upon circuit 1 when it is tuned to frequency f_1. For example, the alternating current on the line *a* of Figure 3 shows the amplitude of the current in circuit 1 when the resistance is very low, corresponding to "very low resistance" response curve in Figure 2. The alternating currents on lines *b*, *c*, and *d* of Figure 3 indicate the current amplitudes corresponding to the other three response curves in Figure 2. In Figure 4 the currents on lines a′, b′, c′, and d′ indicate the lesser amplitudes

of the currents when the incoming signal frequency is at f_2, shown by the vertical dotted line at f_2, Figure 2, when the circuit 1 remains tuned to frequency f_1, the amplitudes in Figure 4 corresponding to the resistance conditions covered by the response curves in Figure 2.

As a practical example of these effects in radio practice, the "very low resistance" response curve of Figure 2 may represent the result obtained when the secondary circuit of a receiver has the detector removed from the tunable circuit, as is shown in the detector location in Figure 1 of my [fol. 1716] sketch, and the circuit, not including deliberately any unnecessary resistance, such a circuit being commonly known as a "low loss" circuit; this as compared to a circuit like that of Figure 1 of Marconi patent 627650, discussed in my previous answer, in which the detector is included within the secondary circuit, thereby introducing a resistance in series just as is accomplished by the resistance R in Figure 1 of my sketch. In his case the response curve will become depressed very much in comparison with the curve resulting from the other location of the detector, and thus may take the form of the "high resistance" or "very high resistance" curves of Figure 2 of my sketch. Obviously, however, the tuning in the latter form of detector connection still exists, the only difference being that the response is not as efficient for any frequency as it is in the first case, and is not as sharp or well defined at resonance as in the first case.

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16. Question. In answer to question 152, pages 1551 and 1552, claimant's witness Weagant, refers to Dolbear patent 350299 and Edison patent 465971. Do you agree with his testimony to the effect that these patents do not utilize Hertzian waves, and therefore have no relation to the Marconi reissue patent in suit?

Answer. I do not agree with Mr. Weagant that there is a difference between the electrical effects at a distance resulting from the systems of Dolbear patent 350299 and Edison patent 465971 and the electrical effects at a distance of Marconi reissue patent 11913. Hertz experimented with alternating currents flowing in electrical conductors, and found that such current flow resulted in electrical effects which were manifested through space and could be detected as at a distance. After the work of Hertz had been pub-

lished it became popular to refer to the mode of propagation of the electrical effect through space as "Hertzian waves." Although the investigations of Hertz were conducted some 40 years ago, and the effects noted by Hertz continuously and extensively studied and used since that time, there has not as yet been proposed by anyone a theory of the manner of the propagation of the energy or effects through space that has been universally accepted. All agree, and practice compels agreement, that when alternating currents are caused to flow in an elevated wire or antenna that there results electrical effects which spread out in all directions with gradually diminishing energy, and that with proper apparatus these effects can be detected and utilized. Hertz did not connect his conductors to the ground. Marconi in reissue patent 11913 did connect his antenna or conductor to ground. The same is true in the Dolbear and Edison patents, both earlier than the Marconi reissue patent. Dolbear, Edison, and Marconi all caused alternating currents to flow in the elevated antennæ provided by them at the transmitting station, and all of them set up elevated wires at distant receiving stations for detecting the electrical effects resulting from these alternating currents flowing in the elevated wires at the transmitting stations. The work of Edison and Dolbear was done prior to the time of adoption of the popular name of "Hertzian waves" for the electrical effects operating through space, so of course their [fol. 1717] patents make no reference to "Hertzian waves," but both patents bring out clearly that the electrical effects did cover the distance between the transmitting and receiving stations. The Marconi reissue patent, coming some 10 years after the work of Hertz, refers to the effect as "Hertzian waves," but makes no attempt to explain any mode of propagation through space of energy by means of "Hertzian waves," and draws no distinction over the electrical effects at a distance of Dolbear and Edison. Weagant refers to the effect in the Edison arrangement as "electrostatic induction" and states that it is not a system of "Hertzian wave telegraphy." He makes no attempt to show any difference, or explain a difference, and in fact it is impossible to adhere to the fundamental physical facts, and do so. In both cases alternating currents are caused to flow in an elevated conductor, and the electrical effects resulting therefrom are collected and detected at a distance. In the early days of radio practice many of the electrical

engineers who took up radio work showed an inclination to develop mysterious aspects for radio as compared to general electrical practice, and to claim for the high-frequency currents of radio practice different conduct from the lower frequency currents of general practice. Some progress was made toward creating new and mysterious names for features of high-frequency practice, and for some time it was thought that reference to "Hertzian waves" drew an unquestionable distinction over the fundamental principles of electrical effects. Now that radio is becoming more and more common, it is happily being studied and considered in the light of the fundamental physical facts covering all electrical effects, and these fictitious differences are disappearing. The fact is that the electrical effects in the Dolbear and Edison systems are precisely those of Hertz and the Marconi reissue patent following Hertz.

17. Question. Claimant's witness Weagant, page 1558, refers to an alleged difference between the Tesla apparatus of the Martin book and the Marconi reissue patent, and in this connection claimant's witness Hogan, page 1834, alleges that there is no foundation in Tesla's work for the system of your Sketch A opposite page 957. What is your opinion as to these allegations?

Answer. On page 1558 the witness Weagant states that in Marconi reissue patent 11913 it was intended to generate oscillations of high frequency—that is, to cause alternating currents of high frequency to flow in the elevated wire connected to ground or antenna circuit—and thus emit "Hertzian waves" from the antenna. He does not deny that Tesla in the Martin book, defendant's Exhibit B-4, provides for an elevated wire connected to ground associated with means for causing alternating currents to flow in the wire, but takes the position that because Tesla did not give any definite mode of transmission of the effects over a distance he did not include any "thought or suggestion of the use or generation of Hertzian waves." In other words, according to Weagant, it was not alone sufficient for one to create a system for producing adequate electrical effects at a distance in the early days, but it was also necessary to in effect trade-mark the system with the then popular term "Hertzian waves." The contention of Weagant as to the Tesla work in the Martin book is substantially that [fol. 1718] of his contention as to Dolbear and Edison referred to in my answer to the last question.

On page 1834 the witness Hogan states that the organization of the Tesla transmitter shown in my Sketch A opposite page 957 can not be found in any of the Tesla publications. He states that that part of the system of my sketch which is based upon Figure 165 of the Martin book "illustrates several circuits for converting electric current." His description of the apparatus is thus far from being complete. Figure 165 is entitled to much more than the brief qualification "converting electric current." The figure shows means of the most modern kind for converting low-frequency alternating currents into high-frequency alternating currents for just such a purpose as creating so-called "Hertzian waves" for radio transmission. In fact, the figure is so complete in all of its details that it merely requires the substitution of the modern quenched gaps in lieu of the open gaps *dd* in order to have the generator of the quenched-gap system as it is still being used to-day.

In Figure 185 of the Martin book there is shown a typically modern transmitting antenna system, there being an elevated wire having an extended portion at the top to give additional capacity as is used in all modern transmitters, and the wire is adequately grounded, as by connecting to water mains buried in the ground, a usual practice where such means are available. Tesla well explained that such an elevated antenna system excited with alternating currents would transmit electrical effects over distance which could be detected as signals for communication. It is noted that Mr. Hogan qualifies the figure as having "nothing whatever to do with radio telegraphic transmission." The Tesla figures do not in fact include telegraph keys for carrying out what is generally known as "telegraphic" communication by use of the so-called Morse telegraphic code. Of course, telegraphic communication is communication of but one kind, and the absence of a telegraph key is not basis for disqualifying an otherwise effective communication system.

Hogan also states, page 1834, that the primary and secondary coils P and S_1 may have been taken by me from one of the Tesla patents. That they are not shown in association with the other apparatus of "Loftin Sketch A." Figure 165 of the Martin book shows that I did not have to refer to Tesla patents as an origin for the primary and secondary coils P and S_1. The Martin book clearly de-

scribes the second system of Figure 165, as well as others of the figure, as a source of high-frequency alternating currents, and includes a transformer having a primary winding P' and a secondary winding S_1 for connecting this source to wherever use may be required of it, just as any properly designed electrical apparatus is provided with connections for making use of it. In his description of Figure 185, the antenna system, Tesla directs the use of a source of high-frequency oscillations inserted in the antenna to get the results described by him, and it requires no particular knowledge to use any one of the sources fully set forth in Figure 165, this including the primary and secondary coils therein provided.

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[fol. 1719] Mr. Edwards. Defendant's counsel offers in evidence a sketch made in answer to question 14, and the same is marked "Defendant's Exhibit W-6."

18. Question. Having in view your explanation of the effect of resistance given in answer to question 15, please again refer to Marconi patent 627650 and explain what, if any, effect the position of condenser *k'* with respect to the detector T has upon the tuning of the lateral circuit.

Answer. Marconi patent 627650 states, page 1, lines 38 to 43, that the condenser *k'* should be varied in order to obtain "best effects" if the length of the wave is varied. In speaking of a receiving system "best effects" means simply loudest signals, or in other words, Marconi directed varying the capacity of condenser *k'* until loudest signal was had. Referring to Figure 2 of my sketch R one, in following out the instructions of this Marconi patent, would vary the condenser *k'* when the wave length received corresponded to the frequency *f'* until a maximum or peak was had in response for the signal as shown, for example, by the "high resistance" curve in Figure 2 of my sketch. Because of the high resistance of the detector in series with the tuning condenser *k'* the response would not be as peaked or sharply defined as in the case of the "very low resistance" curve in Figure 2 of my sketch, but the "best effect" would be obtained under the limitation imposed by the high resistance of the detector. At this adjustment for "best effect" the secondary circuit would be in tune with the primary or antenna circuit and with frequency of the incoming signals.

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19. Question. Claimant's witness, Weagant, page 1558 and following, and Hogan, page 1832 and following, allege that Tesla patents 645576 and 649621 do not provide for tuning the antenna circuit to accord with the closed or lateral circuit. Will you consider the reasons given by these witnesses for their contention and state if you agree or not?

* * * * * * *

Answer. On page 1833 the witness Hogan states that Tesla in patent 645576, defendant's Exhibit X-2, does not describe tuning his "entire secondary circuit with the elevated conductor attached (not merely the secondary coil alone)." The left-hand figure of the drawing of this Tesla [fol. 1720] patent, which shows a transmitting station, shows a coil A having one terminal connected to ground and the other terminal connected to an elevated wire B, thus constituting the usual complete secondary circuit of a radio transmitting station whether of the present day or of the early day of the Tesla patent. There is no possible way in which one element in an electrical circuit can be tuned by itself to the exclusion of other elements connected thereto, and the contention of Hogan that Tesla tuned merely the coil A without bringing into the tuning effect the whole antenna circuit is as absurd from an electrical point of view as would be a contention that a man jumps some such distance as 20 feet with his legs only and that other elements of his body do not participate in the action. It is true that a coil by itself without any connections has a definite natural period, and is therefore capable of being in tune with electrical currents of the same period, but Tesla in paten 654576 did not show an isolated coil, but a complete antenna system of a transmitting station with the coil included as merely one element thereof, and the tuning specified in Tesla patent could only be brought about through the tuning of the whole circuit.

Again on page 1833 the witness Hogan states as to this Tesla patent the following:

"Tesla's explicit directions are to adjust a transformer secondary coil to produce a maximum voltage at its central terminal, and then to apply that exceedingly high voltage to the elevated conductor through the wire B (of his patent 645576)."

This is a misstatement of the tuning instruction in the Tesla patent, his actual instructions being found on page 3, lines 78 to 85, as follows:

"By such an adjustment or proportioning of the length of wire in the secondary coil or coils the points of highest potential are made to coincide with the elevated terminals DD', and it should be understood that whatever length be given to the wires this condition should be complied with in order to attain the best result."

Thus Hogan erroneously says that Tesla instructed to adjust to bring the highest potentials at the center of coil A of the left-hand figure, whereas Tesla in fact instructed adjusting to bring the highest potential in the terminal D at the top of the antenna, and examination of the figure shows that the full length of wire B separates the point actually stated by Tesla and the point erroneously stated by Hogan, so that Tesla did in fact provide for including the elevated wire and the other elements of the antenna circuit in addition to the coil in his instructions for tuning.

There is no contention made by Hogan that Tesla did not provide for generating or creating high-frequency alternating currents in the primary circuit for transfer to the antenna circuit in the transmitting system of the left-hand figure of the drawing, and in fact Hogan admits that all that was necessary for the system of Tesla in patent 645576 to be the system of Marconi in patent 763772 was the tuning of the antenna circuit. On page 1833 Hogans states:

"To make the Marconi invention Tesla would have to tune his entire secondary circuit * * *."

Since the very language of the Tesla patent provides for an adjustment of the antenna circuit to bring the point [fol. 1721] of highest potential at the top of the antenna, a true condition of tuning, instead of at the terminal in the middle of coil A as erroneously pointed out by Hogan, Tesla did provide for tuning the entire secondary or antenna circuit, and by so doing satisfied the requirements specified by Hogan as necessary to make up the alleged invention of Marconi patent 763772.

At the bottom of page 1832 Hogan admits that Tesla patent 645576 contemplated the transmission of intelligible messages, but states that this is not "wireless telegraphy"

as Marconi knew it, adding that it is not "Hertzian" or "radio telegraphy" which contention I can not understand unless a limitation is intended by reference to "telegraphy," as Tesla did not specifically refer to the use of a telegrapher's key for the transmission of the "intelligible messages" mentioned by him. Of course the limitation of the system to a telegraph one is not pertinent, as radio systems are most extensively used without employing telegraph apparatus as a means for forming the intelligible messages transmitted. For example, practically all of radio broadcast transmitters now used do not involve telegraph apparatus. There seems to be no dispute that the Tesla transmitter, having tuned antenna or not, provided for exciting the antenna with alternating currents, and there can be no question that the flow of these currents in the antenna circuit results in the production of electrical effect detectable at a distance, and it makes not the slightest difference whether or not the mode of transmission of these effects is given the name "Hertzian" or any other popular designation, the results are inevitably the same.

I do not find that the witness Weagant made reference to the matter of tuning as between the antenna circuit and the closed or lateral circuit in connection with Tesla patents 645576 and 649621 on page 1558 or the pages immediately following, but do find that he testified as to this matter on pages 1643 to 1649 and, like Hogan, contends that the tuning provided by Tesla refers, in the case of the antenna circuit, to tuning of the coil A by itself, basing his contention on some data of empirical character given by Tesla in patent 645576 for construction of the coil A. For example, at the bottom of page 1644, Weagant quotes from the Tesla patent to the effect that the wire forming the secondary coil in the transformer used by him should be "approximately" one quarter of the wave length of the electrical disturbance in the circuit, followed by fundamental reasoning on the part of Tesla as to why he suggested this approximation, and his reasoning is no more than that of every practical engineer in arriving from a first consideration point of view of a place from which to start in producing an electrical organization.

At the top of page 1645 Weagant quotes that part of the Tesla specification in patent 645756 as to adjustment to bring the highest potential at the top of the antenna which I quoted in referring to the testimony of Hogan in con-

nection with these Tesla patents, and although the quotation is specific for the adjustment of the potential condition at the top of the antenna, and thus far removed from the coil near the base of the antenna, Weagant discusses this quotation as limited to an adjustment affecting the coil only. Weagant states:

[fol. 1722] "Tesla, however, adjusts the natural period of his secondary coil alone and then directs the attachment to the high potential point of an antenna of any length."

That Tesla had the whole antenna circuit in mind rather than the mere coil as contended by Weagant is positive from Tesla's final instructions as to what would determine the best adjustment as follows, lines 80 to 85, page 3:

"and it should be understood that whatever length be given to the wires this condition (highest potential at the top of the antenna) should be complied with in order to attain the best results." (Parentheses mine.)

That is, Tesla laid down an empirical rule for constructing coils for use in the antenna circuit, but did not depart from the subject until he had left a clear instruction and caution that no matter what happened to be the length of the wire an actual electrical condition definite of tuning was to be arrived at in determination of final adjustment for "best results," and Tesla makes it clear in his specification that "best results" refers to tuning or syntony as between the primary and secondary circuits.

Weagant, like Hogan, contends that because Tesla did not claim to use "Hertzian waves" his system as a whole differs from the systems of Marconi who wrote the words "Hertzian waves" into his patent specification, and, like Hogan, does not deny that Tesla provided for causing alternating currents to flow in an antenna circuit without pointing to any circumstances in the Tesla systems which would prevent them from producing electrical effects detectable at a distance in the same way as the Marconi systems.

20. Question. In answer to question 208, page 1636, the witness Weagant criticizes your "Sketch L" of Figure 4 of Lodge patent 609154 because you omitted the spark gap in the antenna circuit. What are the instructions of the Lodge patent in this respect?

Answer. In his answer to question 208, page 1636, Weagant states that I, in drawing the circuit for my "Sketch L," omitted the spark gap in the antenna circuit shown in Figure 4 of Lodge. My Sketch L makes specific reference to "Figure 4 and description," and I so labeled my sketch in order to make it clear that some of the features of it depended upon descriptive matter not definitely covered in Figure 4 itself. In Figure 4 of the drawing Lodge shows the coil h^4 in the antenna circuit interrupted by a spark gap formed by the spark balls h^2, h^3. As to this gap Lodge states, page 2, lines 35 to 39:

"The best width of this gap depends upon circumstances, and it may be closed altogether without stopping the action."

My sketch shows an arrangement in which the gap between the two parts of the coil h^4 is closed as provided for in this part of the disclosure of Lodge.

21. Question. In the same answer the witness Weagant states that if Lodge Figure 4 is constructed in accordance with the instructions in the patent it is inoperative, and if [fol. 1723] the coil k is omitted, the system will give rise to the emission of a complex wave train. What can you say as to these contentions?

Answer. Commencing at the bottom of page 1637 Weagant refers to apparatus that was set up and operated during the Kilbourne & Clark case for a demonstration of Figure 4 of Lodge, and objects to this apparatus on the ground that for the coil k used in the demonstration there was included an iron core, stating that this construction is nowhere disclosed in the patent and quite contrary to the usual practice when coils are used for the purpose of carrying high-frequency currents. Lodge does not state that the coil k is intended to carry high-frequency currents, the purpose being stated by Lodge, page 2, lines 49 to 53, as follows:

"The outer coats of the jars must not be insulated from each other, and I usually join them by an induction coil of fairly thin wire k, so as to permit thorough charging."

Lodge charged the condensers with very low-frequency currents, and obviously the coil k should have such high inductance as to prevent in effect a short circuit between the two condensers j at the low frequency of the charging currents. In other words, the function of this coil k is related

to low-frequency currents rather than high-frequency currents as contended by Weagant. To make a coil with sufficient inductance to provide the required high impedance for low frequency currents requires a good many turns of fine wire. With our present knowledge of the effect of increasing inductance of a coil by using magnetic cores, such as iron, the same impedance can be obtained with the use of a much smaller number of turns of wire on an iron core than without such core, and it would be foolish to now construct a coil to produce the function of coil *k* in Figure 4 of the Lodge patent without an iron core.

The contention of Weagant is that the coil *k* should be built of a small number of turns in order to have small inductance in order to permit the flow of high-frequency currents quite readily, this in spite of the absence of any instructions in the Lodge patent covering a function in connection with high-frequency currents. If the coil *k* were constructed of a sufficiently small number of turns to be suitable for high-frequency current action it would amount to a short circuit between condensers *j* and prevent the "thorough charging" of these condensers by the low-frequency charging current contemplated in the Lodge patent, and the system would then be inoperative. The fact that such a construction of the coil *k* would result in inoperativeness makes it clear that Lodge did not contemplate the high frequency coil of the Weagant, and that the iron core of the coil used in the Kilbourne & Clark demonstration correctly followed the instructions of Lodge to provide a coil that would permit of "thorough charging" of the condensers *j*.

It is true that with the coil *k* omitted, as well as with the coil *k* of proper construction in play, the system of Figure 4 will give rise to the emission of a complex wave train, but this fact does not prove anything derogatory to the Lodge system. Figure 4 of Lodge provides for two circuits, an antenna circuit and a lateral circuit, having a coupling there between through which energy must be transferred. In my answer to question 21 in my prior testimony, [fol. 1724] between pages 1035 to 1041, I point out that wherever energy is transferred from one circuit to another there results a complex wave train by the interaction of the circuits, and that a multiplicity of frequencies of electrical effects results. Also that the separation of the frequencies depends upon the degree of coupling between the

two circuits. In my sketch G, opposite page 1039, I illustrate in Figure 3 this complexity of frequencies involved in this action. In my reference to the modern quenched gap system in connection with my sketch O, opposite page 1100, I show in Figure 6-B how this complexity is included in the quenched gap system, an improvement over the system of Figure 4 of Lodge which has now long had extensive use. While it is not altogether desirable to have this complex wave train, yet it has always been necessary to transfer energy from one circuit to another in radio transmitters that have been proposed so that this complexity has inherently existed. The fact is that rather than the presence of a complex wave train proving anything against the system it serves to prove that the system is operative and includes a feature inherent to all of those systems which found use in practice.

Referring to my sketch L, opposite page 1069, of the Lodge transmitter, and comparing this to the transmitter in Figure 37 of claimant's Exhibit 91, opposite page 181 in the testimony of claimant's expert Waterman, it will be seen that the two systems shown are substantially similar, particularly in the matter of having an inductance element common to both the antenna circuit and the local circuit, or what is generally known as direct coupling. Claimant's expert Waterman uses Figure 37 of claimant's Exhibit 91 to explain that the direct coupling is equivalent of the indirect coupling of Figure 36 of the same exhibit, that is the 2-winding transformer. The two systems of claimant's Exhibit 91 inherently produce a complex wave train as does Figure 4 of the Lodge patent.

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22. Question. Mr. Hogan, in answer to question 6, page 1819, refers to the operation of Lodge Figure 4 as a "single chunk" operation. Does the Lodge specification describe any apparatus purporting to act as indicated by Mr. Hogan?

Answer. It does not. I understand from the testimony of Hogan that his reference to the action in the arrangement of Lodge Figure 4 as a "single chunk method of exciting a transmitting antenna" is intended to convey the impression that it is his opinion that the description by Lodge of the operation of his system suggested the thought by Lodge that there was to be but a single rush or pulse of current in the primary circuit upon breakdown of the spark gap

and discharge of the charged condenser, and further that because the Lodge arrangement of Figure 4 can not and does not operate to give but a single pulse that it fails to operate in the way that Hogan has erroneously contended that Lodge describes.

[fol. 1725] That the "single chunk" contention of Hogan as the idea of Lodge as to the working of Figure 4 is erroneous and not based on a thorough study of Lodge's description is particularly obvious from that part of the Lodge patent, page 2, lines 27-32, which reads as follows:

"So that when the coil is in action the capacity areas receive their positive and negative charge by aerial disruption—that is, in a sudden manner—rather than by the slower process of metallic conduction."

Here Lodge specifically refers to the capacity areas of the antenna receiving both positive charges and negative charges by the action of his primary circuit. There is no limitation expressed as to the number of each kind of charge, but if limited to the minimum of but one of each kind of charge two pulses would be necessary rather than the "single chunk" of Hogan.

Nowhere in the Lodge specification is any reference made expressly to "single chunk," or any other equivalent language such as single impulse or single rush of current, which might well have been used by Lodge had he had the idea erroneously ascribed to him by Hogan.

Lodge clearly had the idea of transferring the energy from the primary circuit to the secondary circuit as quickly as possible in order that the period of time involved in the transfer of energy during which all sorts of complicated actions take place would be as short as possible compared to the time in which the energy would freely oscillate in the antenna circuit without any disturbing back connection to the primary circuit, the very basis and fudamental principle of the quenched gap system which later supplanted all other forms of spark telegraphy radio transmitters. Lodge refers to the charging of the antenna from the primary circuit as "in a sudden manner" and "impulsive rush." Appreciating that Lodge was dealing with alternating currents that involved millions of pulses per second, it is physically impossible that Lodge thought in terms of isolating one of these little pulses—a "single chunk"—and depend-

ing upon it for supplying all of the energy needed by his antenna tc make up a radio transmitting system.

Again, if the "single chunk" suggestion of Hogan had any foundation it would mean that each time the spark gap breaks down to start a discharge there would be but one rush of current or spark at the gap, instead of a rapid succession of sparks occurring at the rate of millions per second. That Lodge did not think of the action in terms of but one rush of current across the gap, or but one spark, is clear from his description of the purpose of the coil *k*. Page 2, lines 53-56, Lodge states:

"When the discharge occurs, this wire acts as an alternative path or by-pass, but does not prevent the sparks at the supply gap."

Thus Lodge clearly visualizes that when the discharge had once taken place there would be a succession of "sparks" throughout the duration of which the action of the coil *k* would be helpful. If Lodge had foreseen but one spark comprising the discharge he would never have suggested the use of the coil *k*, as it could have no function in the absence of a plurality of discharges of currents across the gap.

The inaccuracy of Hogan's study of the Lodge patent is further illustrated in that part of his answer to question 6 in which he says:

"Lodge did not propose to take the spark gap out of the transmitting antenna."

[fol. 1726] There seems to be a serious objection among claimant's witnesses to admitting that Lodge provided for taking the spark gap out of the antenna. In a previous answer I referred to the criticism of the witness Weagant of my sketch L, opposite page 1069, because it does not include a spark gap in the antenna circuit, and pointed out that Lodge provided for removing the spark gap, page 2, lines 35-39.

23. Question. In his answer to question 209, pages 1639 and following, the witness Weagant refers to a paper by John H. Morecroft in the Proceedings of the Institute of Radio Engineers, volume 8, 1920, page 75, claimant's Exhibit 248. Is the action of the apparatus described in this paper the same as that of Lodge's Figure 4?

Answer. No. The Morecroft system referred to by Weagant in the part of his testimony covered by your question is in nowise the system of Figure 4 of Lodge. In the Morecroft system deliberate attempt was made to secure a single pulse or rush of current in a primary circuit coupled to a secondary circuit in order to determine the result in the conduct of the energy transferred to the secondary circuit from such a single pulse.

In the first paragraph of page 1640 Weagant makes it clear that the primary circuit of Morecroft was not the primary circuit of Figure 4 of Lodge in that Morecroft merely included a battery as a source of energy and a resistance, with a switch to suddenly start and stop a rush of direct current from the battery to form a pulse of duration determined by the length of time the switch was open. Such an arrangement is in nowise an oscillatory circuit, and can not produce alternating currents. In the case of Figure 4 of Lodge the primary circuit is an oscillatory circuit, including an inductance and a condenser for determining the period of the circuit and a spark gap for starting an oscillatory discharge in the circuit, so that once the spark gap breaks down currents will flow back and forth until some condition increases the resistance of the gap to current flow to such an extent that the remaining energy is insufficient to overcome the resistance. In other words, the primary circuit of Lodge is an oscillatory circuit of the same kind used in all spark radio transmitters for starting the electrical energy in motion to excite an antenna.

Thus referring to Figures 4, 5, 2, and 3 of claimant's Exhibit 248, opposite page 1640, there is shown in the upper part of each one of the oscillograms of these figures a single rise indicative of a single pulse of current in the primary circuit upon the opening and closing of the switch in the Morecroft arrangement. If the arrangement of Lodge Figure 4 were substituted these oscillograms would show alternating current above and below the line as long as the spark-gap remained conductive to the energy in the primary circuit, and with the arrangement of Lodge Figure 4 it would be impossible to produce the single pulse shown in Figures 2, 3, 4, and 5 of the Morecroft paper.

The figures do show very nicely, however, how an antenna circuit, when excited with energy from a primary circuit, oscillates with this energy in its own natural period after the influence of the primary circuit is removed. Thus

Figures 2 and 3 show that there is practically no influence on the oscillations in the secondary circuit when the switch in the primary circuit is closed very quickly after the opening thereof, while Figures 4 and 5 show that when the switch [fols. 1727-1728] is held open for a long time the energy in the secondary circuit reacts back onto the primary circuit to create the complex wave effect slightly noticeable in the second half cycle of the first alternation in Figure 4 and particularly noticeable in the same location in Figure 5. This effect clearly shows the understanding of Lodge of the desirability of removing the effect of the primary or charging circuit on the antenna as soon as the transfer of energy operation would permit, and the soundness of the principle for practical radio communication is demonstrated in the fact that the modern quenched gap system which, through substantial improvement of gap construction and coupling conditions, resulted in so effectively using this principle of Lodge that the system practically supplanted all other spark telegraphy systems.

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24. Question. Mr. Waterman, in answer to question 2, on page 1881, testified that the arrangement of figure 4 of Lodge was useless. Do you agree?

Answer. No. While Lodge did not provide in figure 4 all of the refinements for utilizing in the best manner the principle of quickly charging the antenna and disconnecting the charging circuit therefrom as are now embodied in quenched gap systems, at the very early date of the Lodge disclosure it was a substantial advance over prior suggestions for a practical communication system, and particularly so in the matter of removing the spark gap from the antenna circuit, the first step toward making it possible to excite the antenna and at a proper moment leave it free to oscillate without the influence of the spark gap. The system was particularly useful for operation as it stood, and of outstanding value because of pointing the way to the ultimate highly successful quenched gap system.

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[fol. 1729] 30. Question. In question 44, page 1497, the witness Clark was referred to a portion of your testimony of page 1125 in which you are quoted as stating that it was "not" possible to use the Navy wave changers with the open type of transmitter. Does the quoted part of

your testimony in question 44 give the correct sense of your statement concerning these wave changers?

Answer. The paragraph from which the quotation was taken, when correctly read, clearly shows that inclusion of the word "not" is an error and was not intended by me. In the same paragraph I definitely refer to using the marking on the dial if the wave changers should be used with an open-gap type of system.

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31. Question. Please consider the statements made by Mr. Weagant in answer to question 213, commencing on page 1650, and state whether you agree with his testimony, stating your reasons.

* * * * * * *

[fol. 1730] Answer. I do not agree with the testimony in general of Mr. Weagant in answer to question 213. The testimony relates to the bearing of the letters which passed between Joseph D. Baker and John S. Stone of June 30, 1899, July 18, 1899, and July 22, 1899, defendant's Exhibits F-3, G-3, and E-3, respectively, the original application for patent No. 714756 of John Stone Stone, defendant's Exhibit R-3, and Stone patent 714756, defendant's Exhibit P-3, on the subject matter of Marconi patent 763772 in suit, and Weagant's statements relating to this matter are rather lengthy.

My general understanding of the Weagant testimony is that there is no denial by him that the Stone system in the Stone to Baker letters of June 30, 1899, and July 18, 1899, the original Stone application for patent 714756 and Stone patent 714756 include the elements forming the disclosure of the Marconi patent 763772 in suit, but that he merely argues that Stone provided for tuning but one portion of the antenna circuit instead of the entire circuit, and to this extent falls short of satisfying the Marconi disclosure. In the Stone to Baker letters and Stone patent, Stone referred to the antenna structure of both the transmitting and receiving stations as an "elevated wire" in some cases and an "elevated conductor" in some cases, and also included reference to the antenna as a "circuit" in some cases. In both the letters and the Stone patent Stone showed the antenna to be a complete circuit starting from a wire connection to ground through a coil

to a wire extending vertically, and it is my understanding of Weagant's present testimony that he contends all of Stone's references in the letters and patent to tuning having to do with the antenna were limited to that part of the antenna made up of the vertical wire above the coil in the antenna circuit, this as compared to Weagant's contention in connection with the tuning of the antenna in the Tesla patents being limited to the coil in the antenna only.

Thus again Weagant contends an absurdly impossible physical proceeding. Just as it is physically impossible to attach a weight to a spring and cause the spring to be tuned to vibrate in a period independent of the effect of the weight attached thereto, so is it impossible to include coils of wire in an electrical circuit and cause the circuit to respond in any way other than that involving the effect on the circuit of that part of the total inductance of the circuit contributed by the coil. Long prior to the Stone letters to Baker and the filing of the Stone application for patent, electricians understood that the tuning of a circuit depended upon all of the inductance of the circuit combined with all of the capacity of the circuit, and so this knowledge up in the form of a mathematical expression, which expression is found opposite line 105, page 5, of Stone patent 714756 under discussion. Just above the formula Stone refers to it as "the mathematical expression for the frequency to which a circuit is resonant when it is isolated from all other circuits"—thus, instead of Stone having any such foolish idea as that attributed to him by Weagant that he could mentally select parts of a complete [fol. 1731] electrical entity and refer to it as being tuned to the exclusion of the entire system, Stone most intelligently makes it clear in commenting on the mathematical expression for resonance of circuits that it is not possible to apply the expression to a circuit except when it is "isolated" from all other circuits. As between independent circuits it is, of course, possible to effectively isolate one from another, as by physically separating them, but it is absolutely impossible to electrically separate one portion of a circuit from another portion of the same circuit except by the aid of the imagination.

In addition to the electrical absurdity of Weagant's contention there is no basis physically for contending that

Stone's use of the term "elevated wire" or like terms limited the thing referred to by him to that portion of the antenna circuit above the coil. The fact is that such expressions as "elevated conductor," "vertical wire," and the like, were the forerunners of the later term "antenna," and I have no hesitation in saying that no reputable engineer of the present time would contend that a reference to tuning an antenna limited the idea of tuning to that portion of the whole circuit extending in antenna fashion above the coupling coil.

Weagant further contends that Stone's reference to the words "forced oscillations" or "forced vibrations" used in referring to the high-frequency currents in the antenna following generation of the currents in the primary circuit and transferring them to the antenna circuit through the coupling means that the antenna circuit was to be out of tune with the oscillation coming from the primary circuit. On page 1650 Weagant states as to these terms—"have but one significance in this art, viz., vibrations or oscillations differing in frequency from that of the circuit on which they are impressed."

This definition of Weagant is not a true one in fact even to day, and at the time of the filing of the Stone application for patent Stone was in a position of being more or less a pioneer in creating expressions for actions of this kind. The generally accepted understanding of these terms is in those cases where oscillations are generated and transferred in such a way that there can be no change in the frequency as generated by reason of the transfer, or, in other words, the oscillations are imposed upon the circuit receiving them in the form in which they are created rather than being modified by some influence had on them on the part of the receiving circuit, and if the oscillations are properly created and properly transferred it makes no difference where the receiving circuit is tuned or not, the fundamental frequency at which created will remain the same after transfer to the receiving circuit. For example, it is usual to generate alternating currents for lighting and power purposes by machines which, through the influence of mechanical speed-governing devices, operate to generate the currents at 60 cycles per second, and these 60-cycle commercial currents are transferred from place to place through numerous transformers still maintaining a frequency of 60 cycles per

second, and it makes no difference whether or not the circuits to which these currents are transferred are tuned or not, and in some cases some of the receiving circuits are tuned, or nearly so, under a condition commonly known among power electricians as maintaining "unity-power [fol. 1732] factor," a condition under which the losses are a minimum just as they are in radio practice when the circuits are tuned. In such power systems the 60-cycle oscillations are truly "forced" upon the distributing lines by the constantly acting machine generator under the influence of a constantly supervising governor, so that the electrical constants of the circuits can not alter this forced frequency.

Where oscillations are generated directly within one circuit, as by a spark-gap discharge within the circuit, the condition shown in Marconi reissue patent 11913, there is no way of forcing the resulting oscillations to take up a frequency which ignores the electrical time constants of the circuit; in fact, these electrical time constants have the sole determination of the frequency of the operation, and such oscillations are commonly known as "free" oscillations. It is well known that it is impossible to generate with practical apparatus absolutely pure oscillations, so that all generated oscillations include a fundamental accompanied by many harmonics, and the intensity of the harmonics in comparison with the intensity of the fundamental is a measure of the parity of the generated oscillations. For example, it is not uncommon to be able to receive a broadcast radio transmitter at two widely separated points on the dial if near enough to the station for the energy in the harmonic to reproduce in the receiver. If the oscillations are generated in one circuit to include a fundamental and harmonics obviously by tuning a second circuit to the fundamental a selective discrimination can be made between the fundamental and the harmonics. For example, if a circuit has generated therein a fundamental of 1,000 kilocycles and has a second harmonic of 2,000 kilocycles having 10 per cent of the intensity of the fundamental, a near-by receiving circuit tuned to the fundamental may so discriminate against the harmonic that in the receiving circuit the strength of the harmonic will only be one-tenth of 1 per cent of the received fundamental.

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Thus in the vertical wire system of Marconi reissue patent 11913, with the spark gap located directly within the vertical wire, the generated oscillations had their frequency determined solely by the electrical constants of this vertical wire circuit, and the fundamental frequency with all of the accompanying harmonics were emitted directly out into space from this wire. In a circuit of the elevated wire type, including the damping effect of a spark gap therein, the production of harmonics along with the fundamental is quite pronounced, giving what is generally known in the art as an emission "rich in harmonics," an undesirable form of radio emission, because radio receivers which, in listening for other radio transmitting stations may, in doing so, tune to one of the harmonics and thus hear a signal from a station not wanted. That Stone most intelligently appreciated this condition of affairs in connection with the system of the Marconi reissue patent is certain from the way he handles the subject in his letter of July 18, 1899, to Baker, defendant's Exhibit G-3, and in his patent 714756. In the Baker letter of July 18 he shows in [fol. 1733] Figure 1, page 2, of the letter the transmitting system (at the left) and the receiving system (at the right) of the Marconi reissue patent. Beginning at the bottom of page 2 of the letter Stone states as to this Marconi system the following:

"The discharge of the vertical wire is oscillatory in character owing to the electrostatic capacity and inductance or coefficient of self-induction of the vertical wire, but owing to the fact that the electrostatic capacity is distributed along the wire and is moreover not uniformly distributed (the different points along the wire being at different distances from the earth) the oscillations are not simple harmonic in character, but may be represented by a Fournier's series,

$$A_1 \sin pt + A_2 \sin 2\,pt + A_3 \sin 3\,pt + \text{etc.} + B_0 + B_1 \cos pt + B_2 \cos 2\,pt + B_3 \cos 3\,pt + \text{etc.}$$

The oscillatory discharge of the vertical wire sets up corresponding electromagnetic waves surrounding it, part of which are radiated with the velocity of light. These electromagnetic radiations upon passing the vertical wire at the receiving station set up corresponding electromotive forces in it."

I particularly include in my quotation the mathematical expression of Stone of the fundamental and harmonics because it is of great importance, and technically in no wise warrants the unjustified flippant reference to it by claimant's expert, Waterman, on page 1918, as "a mere excursion into mathematical realms."

A "simple harmonic" oscillation is a fundamental oscillation free from harmonics, or, in other words, is a pure sine wave of but one frequency of oscillation. Stone gives in the quotation electrical and physical reasons why the transmitter of the Marconi reissue patent is incapable of generating pure sine or simple harmonic oscillations, and most ably and intelligently states in the mathematical expression made light of by Waterman what kind of oscillations could be expected, and the expression is a clear picture to any informed physicist Stone's idea of these oscillations, the mathematical expression giving the picture in two lines, whereas if expressed with words several pages would have been required. The expression simply says by its first term "A_1 sin pt" that the oscillation includes a fundamental of frequency p with an amplitude or intensity of some value A_1 accompanied by a second harmonic set out in the second term "A_2 sin 2 pt" having $2p$ or double the frequency of the fundamental and the lesser amplitude or intensity A_2 than the fundamental, and other higher harmonics for each one of the additional terms, and there could certainly be no clearer way than this mathematical expression for Stone to address himself to those with whom he considered himself to be communicating.

The quotation from Stone's letter goes on to make it clear that with such a transmitter as that of the Marconi reissue patent emissions go out into space to reach the receiving antenna, including not only the fundamental "A sin pt" but numerous harmonics represented by the succeeding terms of the expression.

Thus after having clearly pointed out the existence of impure oscillations and the undesirability of them in the Marconi reissue patent system, Stone points out in the letter that if simple harmonic oscillations are to be had in the antenna they must be generated in a medium outside of the [fol. 1734] antenna and be merely impressed or, as Stone expressed it, "forced" upon the antenna after creation elsewhere independently of the electrical constants of the antenna. Stone provided a primary circuit in which to

originally generate the oscillations, and merely coupled this to the antenna circuit. Because of the intelligent and clear understanding of Stone of the production of harmonics in a spark-gap circuit it is conclusive that he did not contemplate that the mere changing of the production of oscillation over to the primary circuit would eliminate the production of harmonics in addition to the fundamental in the primary circuit any more than in the vertical wire he employed for detailed discussion of the matter, and with this understanding the absurdity of contending that Stone's instructions to tune the antenna did not refer to the entire antenna circuit is obvious. Stone wanted only the simple harmonic fundamental in the antenna circuit, and he teaches too much about the matter of tuning and resonant energy transfer to make it believable that he did not know that if he left the antenna circuit untuned with respect to the fundamental in the primary circuit he was likely to resonantly select out of the antenna circuit by this detuning some one of the harmonics in lieu of the fundamental he so decidedly insisted upon having.

The next step of Stone further emphasizes that he had no mistaken ideas as to "forcing" simple harmonic oscillations of fundamental frequency upon the antenna circuit, which step had to do with the matter of coupling between the primary circuit in which the oscillations were generated and the antenna circuit where he desired the simple harmonic of the fundamental only. I have previously pointed out that Stone, in giving on page 5, opposite line 105, of his patent 714756 the mathematical expression for the relation between frequency and inductance and capacity for resonance of a circuit was most particular to point out that the expression holds true only when the circuit under consideration is isolated from all other circuits, so that with this fundamental knowledge so emphatically expressed by Stone he proceeded to isolate his primary circuit in which he generated the oscillations with the aid of a spark gap as much as was practically possible by the aid of "loose" coupling, for he well knew that without a sufficient amount or extent of loose coupling he had no hope of arriving at confining the oscillations transferred to the antenna circuit to the simple harmonic form he so clearly pointed out as necessary to the selective communication system he developed. In his letter of July 18, 1899, to Baker, page 7, he clearly points out a practical manner of securing loose cou-

pling between the primary circuit and the antenna circuit, and in the middle of page 8 of this letter states that he believes himself to be the first one to suggest his particular method of securing loose coupling.

In my previous testimony, pages 1075-1076, I referred to the provision in Tesla patent 645576 of tuning the two circuits at a transmitting station to the same time periods. That Stone knew of this work of Tesla in 1899 is disclosed in his comments in the paragraph commencing at the bottom of page 6 of Stone's letter of July 18, 1899, where Stone says that the transmitting circuit disclosed by him to Baker in a previous letter "is practically the same as that employed by Tesla for the production of high-frequency currents," and Stone goes on to say:

[fol. 1735] "Except that I place an inductance coil L in the circuit to give additional means of tuning and to swamp by its greater inductance the reactions from the induction coil *t*, which would tend to make the oscillations multiperiodic instead of simple harmonic."

Thus Stone made no distinction between his system and the tuned system of Tesla in the matter of tuning as between the two circuits, but that the additional inductance coil of Stone was an improvement over Tesla in two ways, one in the provision of additional means of tuning, and the other in the provision of means giving sufficiently loose coupling between the circuits to prevent the oscillation from being the undesired "multiperiodic" in the antenna instead of the desired "simple harmonic." Thus Stone did not take unto himself any credit for the adjustment of the time periods of the two circuits to be the same, leaving this to Tesla who had already suggested it, but did take credit for himself in the matter of using tuning of the two circuits in proper combination with coupling in order to secure a "simple harmonic" oscillation in the antenna, because he had so clearly pointed out that it is only with this kind of oscillation that the selectivity he desired for his communication system could be had. Thus Stone and Marconi patent 763772 both disclose the adjustment of the time periods of the two circuits to be the same already disclosed by Tesla. Stone takes no credit for it, but leaves it with Telsa, where it belongs, keeping for himself his loose coupling. Marconi claimed for himself the Tesla adjustment, and made no reference to loose coupling, but in an address by

Marconi in 1912, to which I referred in my previous testimony on page 1068, Marconi admitted that this loose coupling of Stone was an essential feature to the successful operation of the system disclosed by him in patent 763772.

* * * * * * *

On page 1650 Weagent states:

"Nowhere in the Stone-Baker letters, or in the subsequent patent, in which he reduced his ideas to practice, is there a disclosure of a variable inductance or other variable tuning element in the antenna circuit.

"While Stone illustrates in detail the principles of operation and the necessary apparatus for the tuning of the circuit associated with the antenna system, he neither shows in the sketches nor describes in words any method of tuning the antenna system."

That these assertions of Weagant are not in accordance with the facts is evident from examination of the language of the Stone-Baker letters and the patent 714,756. In the very first letter to Baker, June 30, 1899, page 2, Stone stated:

"I propose to impress upon this vertical wire oscillations from an oscillator, which oscillations shall be of a frequency corresponding to the fundamental of the wire."

Since Stone arranged to generate the oscillations to be impressed upon the vertical wire in a separate circuit with the aid of the discharge of a spark gap or, in other words, generated "free" oscillations, the electrical time period of the generating circuit was the same as the time period of [fol. 1736] the oscillations, Stone having stated that in such form of generation the period of the oscillation is determined by the period of the circuit. In the quotation above Stone states that the frequency of these generated oscillations is to correspond to the "fundamental" of the wire, the word "fundamental" having no meaning other than fundamental period determined by the electrical time constants of the wire. This, therefore, makes it clear that the primary circuit in which the oscillations were to be generated, the antenna circuit which was to receive the oscillations after generation, and the oscillations were all to have the same frequency, or, in other words, the antenna circuit was to be tuned to the primary circuit.

The last paragraph but one of the same letter clearly contradicts the statement of Weagant that Stone did not describe in words any method of tuning the antenna system. This paragraph reads:

"The tuning of these circuits one to another and all to the same frequency will probably be best accomplished emperically, though the best general proportions may be determined mathematically."

The first feature of importance in this quotation is that Stone definitely refers to the tuning of "circuits" in referring to all four of the circuits, two at the transmitter and two at the receiver, of the complete system shown on page 3, upper figure, of the June 30 letter, and did not distinguish between circuits and "vertical wires." If Weagant's contention were accepted Stone should have written this part of the letter to say the tuning of these circuits *and vertical wires per se* one to another and all to the same frequency, which Stone did not do because he was not the electrical fool that Weagant contends.

The quotation further gives two methods of accomplishing the tuning, rather than no method as contended by Weagant. Stone provided for accomplishing the tuning "empirically," the general practical method in use to-day, adding that the general proportions may also be determined "mathematically," a method also much used in beginning the design of radio devices.

In the next letter to Baker, July 18, 1899, page 5, Stone stated "if the period of the impressed force be the same as that of the fundamental of the vertical wire," after which he points out that the radiations from the antenna will be "simple harmonic" and of the "same period distant." In other words, Stone made it clear that tuning the antenna permitted of maintaining the "simple harmonic" form of radiation in which he was so greatly interested.

On page 8 of this second letter Stone referred to the second set of transmitting and receiving stations shown on page 3 of his letter of June 30, pointing out that in this second system he differed from the first in adding one more circuit at each the transmitting and receiving stations, stating "an additional resonant circuit attuned to the same frequency as the others is interposed between the vertical wires and the transmitting and receiving apparatus for the

purpose of additionally sifting out the undesired harmonics."

I have previously pointed out that Stone appreciated that when he generated the oscillation in the separate circuit he would still have harmonics in addition*al* to the fundamental in the same way as when the oscillations were generated directly in the antenna circuit as in Marconi reissue patent 11913, and by tuning the antenna circuit to the funda-[fol. 1737] mental of the oscillations there would be a discrimination in the transfer of energy from one to the other against the harmonics to which the antenna was not tuned in favor of the fundamental to which Stone said the antenna should be tuned. In this second system referred to by Stone he points out that he goes a step further in the matter of this discrimination against harmonics, stating that the addition of one more tuned circuit has the purpose of "additionally sifting out the undesired harmonics." Certainly this additional step to which Stone went to sift out undesired harmonics by tuning in three circuits makes it absurd to contend that he did not do the same thing in the system having but two circuits.

I further point out that Stone states that this additional circuit is "attuned to the same frequency as the others," the "others" referring to the two circuits of the first system. According to Weagant, Stone did not have the two circuits or "the others" of the first system tuned, but only one other, a wholly inconsistent contention with the plain language of Stone, to say nothing of the technical absurdities involved in the Weagant contention.

Weagant does not deny that the Stone patent 714756 as issued did not fully cover instructions to tune the antenna circuit as a whole, and thus that the Stone patent as issued was fully the disclosure of the Marconi patent 763772 in suit. On page 1658 Weagant states:

"This statement seems to be intended to indicate some sort of tuning of the aerial system, a statement quite unsupported by the drawings of the patent, and it was inserted into the application June 19, 1902, which was after the publication of the British Marconi patent No. 7777.

"These references are cited as showing that everything in the Stone patent 714756 which might be interpreted to mean tuning of the antenna circuits was put into the patent

after Marconi's work was known, and the importance of having all four circuits in tune appreciated."

In order to point out the error of Weagant's statement that everything in the Stone patent 714756 which might be interpreted to mean tuning of the antenna circuits was put into the application by amendment after Marconi's work was known, I refer to the following statement included in the application as originally filed for Stone patent 714756, at the top of page 14:

"When transmitting stations and a corresponding number of receiving stations are employed, by adjusting the electro-magnetic constants of these circuits at the various receiving stations, these circuits may be so proportioned or tuned that the energy of the electromagnetic wave emanating from any given transmitting station will be selectively received and absorbed at a given receiving station."

Remembering that Stone was clearly distinguishing his system from the one circuit system at each of the transmitting and receiving stations of the Marconi reissue patent 11913, it is clear that his reference to adjusting the electromagnetic constants of "the circuits" referred to both of the circuits in his system as distinguished from the one circuit in the system of Marconi reissue patent. It is also pointed out that at the end of the quotation he states that the waves will be "selectively received and absorbed." Thus he includes two operations at his receiving station, [fol. 1738] one being reception and the other absorption. Throughout the Stone literature he referred to the circuit he coupled to the antenna circuit as the "absorbing" circuit, so that the two steps set up by him at his receiving station are given definite meaning by Stone. His reference to "selectively received" makes it clear that the adjusting for tuning the "circuits" in the first part of the quotation included tuning the antenna circuit for selective reception as well as the absorbing circuit.

* * * * * * * *

On page 1654 Weagant states that it is "quite contrary to the usage of the radio art, and it is not customary to speak of a circuit which is in tune as being 'forced' to accept oscillations of its own period."

Stone makes it clear in his patent 714756, page 5, in referring to the mathematical expression of the relation of fre-

quency or period of a circuit to the inductance and capacity of a circuit that the relation only holds true when the two circuits are substantially isolated, or, in other words, that when dealing with free oscillations the circuit can not be made to oscillate at one particular frequency or have any particular frequency of oscillation forced upon it unless substantially isolated by loose coupling, so that the word "forced" in the Stone literature is concerned with the matter of loose coupling and not tuning. Stone makes it clear that under some conditions his antenna is tuned, and under other conditions of multiple telegraphing from one antenna the antenna need not be tuned and, of course, can not be tuned to both frequencies of the multiple telegraph, but in both cases he desires to have only simple harmonic oscillations in the antenna and therefore relies upon loose coupling in order to isolate the antenna circuit from the primary circuit in which the oscillations are generated to a high degree.

On page 1655 Weagant states that my reference on page 1085 to the practice of Stone of removing the resistance of the detector from the tuned circuits is a teaching which I seem to have forgotten when I attempted to make the secondary circuit of Marconi patent 627650 a tuned circuit. I have not contended that the tuning provided for in Marconi patent 627650 is as sharp tuning as that contained in the arrangement of the Stone patent and Stone-to-Baker letters, but that it is the tuning of numerous figures and tunes given in the Marconi patent 763772 in suit in which the detector is included in the secondary circuit.

On pages 1656 and 1657 Weagant quotes from the testimony of John Stone Stone, the patentee of patent 714756, in the suit of Marconi Wireless Telegraph Co. of America v. Kilbourne & Clark Manufacturing Co., which testimony is stipulated in the present case. On page 1657 he includes the following testimony of Stone:

"197. Cross-question. And did you not also testify in that same case as follows:

"'The first very clear and complete exhibition of the use of a continuously variable tuning condenser in shunt to the primary of a transformer in a wireless telegraph receiver, so far as I am aware, is contained in the specifications and drawings of the English patent to Marconi, No. 7777, of 1900.' "

[fol. 1739] Referring to Figure 2 of Marconi patent 763772, there is shown a condenser *h* in shunt connection to the coil *j'* in the antenna circuit, and it is to this particular condenser and its location that Stone referred to in his quoted answer as "the first very clear and complete exhibition of the use of a continuously variable tuning condenser in shunt to the primary of a transformer," which is far from an admission by Stone that the English patent to Marconi, No. 7777, of 1900, corresponding to United States patent 763772, was the first showing of means for tunning the antenna circuit, but merely an admission by Stone that as far as he was aware Marconi was the first to show this particular form of connection by which tunning could be had. On page 1657 Weagant sums up this testimony of Stone to which he referred as Stone himself crediting Marconi with being the first to use "the tuning element in the aerial circuit." It is to be noted that Weagant does not refer broadly to tuning elements, but merely to "the tuning elements," thus limiting his summation to specific ones mentioned by Stone in his testimony and not to all tuning elements by which tuning of the antenna could be accomplished. In this case it is contended that the Lodge patent 609154 in suit, in inserting a mere inductance coil in the antenna circuit, was an outstanding advance in the matter of tuning an elevated conductor system. Both Tesla and Stone included this coil of Lodge in their antenna circuits, and it was also included in Marconi patent 627650 with instructions to adjust the number of turns of the wire in accordance with the frequency of the signal being received.

On page 1658 Weagant states:

"The original disclosures of the Stone patent 714756 in the matter of the drawing did not disclose any means for tuning the antenna circuits, * * *."

The original drawings are the same as the drawings in Stone patent 714756 as issued, and the drawings do disclose means for tuning the antenna circuit. For example, in Figures 5, 6, 7, and 8 both transmitting and receiving systems are fully illustrated by Stone. In every case it is seen that the antenna circuit comprises a vertical wire V connected to ground E through an inductance coil L. With the arrangement thus shown tuning of the antenna

circuit can be accomplished in two ways, or a combination of both, one way being to change coil L_2 in some way as by the simple expedient of changing the number of turns taught by Lodge, though changing the diameter of the turns or the spacing of them could also accomplish a variation of inductance giving the required tuning result. The second way would be to alter the length of the vertical wire, and obviously this expedient could be combined with the first way if desired. It is true that Stone did not include a second inductance coil in the antenna circuit above the inductance coil in the coupling, shown as the coil *g* in Figure 1 of Marconi patent 763772, but reference to the table of tunes for transmitting stations given on page 4 of the Marconi patent shows that out of six tunes specified four were obtained without the use of any inductance at *g*, thus showing that the only difference between Marconi and Stone was not essential to a drawing to show means for tuning the antenna circuit. Further, the testimony of claim- [fol. 1740] ant's witnesses Sarnoff and Taylor in this case as to the early practice of the Marconi Co. clearly brings out that in using tunes "A" and "B," frequently referred to by them, the practice was not to use any variation in turns of a coil in the antenna circuit, but to actually change or substitute transformers permanently wound in the factory one for the other in making change from one tune to the other. Since these transformers were permanent, fixed affairs obviously when installed at some particular radio station on shore or on shipboard the length of the antenna had to be adjusted if there were to be any approach to tuning of the antenna circuit to the lateral circuit. Thus Stone disclosed in his patent and letters more means for tuning the antenna circuit than were used by the Marconi Co. in practice for some years thereafter.

On the bottom of page 1655 Weagant summarizes the matter of tuning of the antenna circuit by Stone and Tesla with respect to the Marconi patent 763772 as having "completely missed, one on one side of the fence, as it were, and the other on the other side. * * *" which refers, of course, to his contention that Stone provided for tuning only that portion of the vertical wire above the coil of wire connecting the vertical wire to ground, and Tesla tuned the coil only independently of the connection of the coil to ground and the vertical wire above the coil, neither of which effect could be accomplished in fact, as there are

no two sides of a fence in connection with the flow of current in an electrical circuit. If there is any basis whatsoever for conceiving a coil in an antenna circuit as one side of a fence and the elevated wire as the other side of a fence, reference to Stone shows that he provided both sides of the fence and introduced the energy into the system on one side of the fence, namely, the coil coupled to the primary circuit, and caused the energy to flow from this one side of the fence to the other side of the fence, namely, the elevated wire, so that the operation was compelled to utilize both sides of the fence in order to function.

On page 1655 Weagant states:

"Marconi drives his antenna circuits in the way in which they want to go, while Stone tries to make them go in some different manner, * * *"

This is not true unless there is read into the Marconi patent the loose coupling so fully and clearly described in Stone. On page 5 of Stone patent 714756, in referring to the mathematical formula. Stone points out that the circuits will not permit of being driven in any one specific way unless there is loose coupling. He also pointed out that he could not expect to obtain selective communication unless he could produce simple harmonic oscillations in his antenna circuit, and for this reason he did not intend to let the circuits choose their own way of oscillating, which would not be simple harmonic if just merely adjusted to have the same time periods, but he would impose upon the operation the requirement that when the oscillations arrived in the antenna circuit they would be simple harmonic in character. The Marconi arrangement falls far short of accomplishing the selective requirements imposed upon it by Marconi in statements of operation desired until it includes the loose coupling not specifically shown.

* * * * * * *

[fol. 1741] 32. Question. Please refer to the testimony in answer to question 33, pages 1835 and following, and state whether or not you agree with the statement, giving your reasons.

* * * * * * *

Answer. The testimony in answer to question 23 is that of claimant's witness Hogan as to the Stone-Baker letters, the Stone application for patent as originally filed, and

Stone patent 714756, and my previous testimony concerning the several phases of the present case.

Hogan, like claimant's witness Weagant, endeavors to construe the plain language instructions of Stone in the Stone-to-Baker letters for tuning the antenna to mean a limitation to only a portion of the antenna circuit—that is, the wire above the coil in the antenna circuit—and thus unites with Weagant in contending plain electrical instructions to involve electrical absurdities.

On page 1836 Hogan states in reference to the apparatus illustrated by diagrams on the third page of the Stone-Baker letter of June 30, 1899:

"However, no wiring diagram can show whether or not the various circuits illustrated are tuned to the same or different frequencies; and for information on that important matter we must turn to the text descriptive of the figures."

In other words, Hogan does not contend, like Weagant, that the showing of an antenna circuit including a vertical wire connected to ground through an inductance coil is not means shown for tuning the antenna, but clearly implies that if there are instructions in the descriptive matter for tuning the means illustrated are adequate for carrying out the tuning of the antenna.

Immediately following, on page 1836, the comment just quoted Hogan itemizes features pointed out in the Stone-Baker letters and as item 2 states the following:

"(2) That certain of the circuits illustrated are to be tuned 'one to another and all to the same frequency' (June 30, p. 4.)"

This statement of Hogan is not true to the text of the Stone-Baker letter to which he refers. The text gives no basis for saying that "certain of the circuits illustrated" are the ones referred to by Stone. On page 3 Stone illustrates all of the circuits needed for transmitting and receiving stations, and clearly defines the circuits by reference to the elements in the circuits shown, including the antenna circuits. After so doing he then states: "The tuning of these circuits one to another and all to the same frequency * * *" which is positive language including all of the circuits and excluding none, and therefore is not

subject to the qualification "certain of the circuits" employed by Hogan.

[fol. 1742] At the bottom of page 1836 Hogan admits that these tuning instructions in the Stone-Baker letter, taken alone, include tuning of the antenna system, but adds the contention that other features of the Stone literature lead away from his conclusion to the extent that in Hogan's opinion Stone, in showing and describing four circuits and instructing that *all* of "these circuits" be tuned to one another and the same frequency referred to only two circuits, the lateral circuits, one at the transmitting station and one at the receiving station. The fact is, there is no more reason for Hogan concluding that these definite instructions of Stone for tuning *all* of the circuits referred to the tuning of the lateral circuits only to the exclusion of the antenna circuits than there is for concluding that Stone referred to the tuning of the two antenna circuits only to the exclusion of the lateral circuits.

At the middle of page 1837 Hogan states:

"Stone's theory was evidently that forced oscillations, not utilizing any antenna tuning, were essential for selectivity."

This is an incorrect interpretation of Stone's idea. His idea was clearly the one of requiring "simple harmonic" oscillations in the antenna irrespective of whether or not he employed the matter of adjustment of time periods of the circuits to be the same for resonant or efficient energy transfer. He describes how, with the aid of loose coupling, he can successfully obtain the simple harmonic form of oscillation in the antenna and, in addition, suggests the tuning of the antenna the same as the lateral circuit, which feature he gracefully attributed to Tesla prior to Stone.

In the seventh line of the second paragraph, page 1837, Hogan states as to "forced oscillation":

"At page 10, lines 19-26, Stone emphasizes his intention of using forced oscillation in the antenna 'in lieu of producing natural vibration.'"

Stone made it clear that he did not intend to use or produce natural vibrations generated by a spark gap included within the antenna circuit, but he did not propose to do away with the natural vibrations resulting from a

spark discharge within a circuit, only requiring that the natural vibrations be produced in a circuit so loosely coupled to the antenna circuit that they would be for all practical purposes vibrations *natural* to the circuit in which they were produced, and remain natural after transfer to and arrival in the antenna circuit, Marconi patent 763772 also avoids the production of oscillations natural to the antenna circuit by the use of a spark gap within the antenna circuit, and if Marconi employs a coupling so loose that there is not a double hump, which Marconi advocated in his address of 1912, he produces oscillations natural to the primary circuit and they remain natural after transfer to and arrival in Marconi's antenna circuit.

At the middle of page 1837 Hogan states that a reference inserted in the Stone application by amendment providing for making "the vertical wire" resonant may be taken as a recognition of the kind of tuning included in the Marconi patent. If reference to tuning a vertical *wire* inserted by amendment is the same as the tuning in the Marconi patent, it is not clear on what basis Hogan formulates his contention that the same language in the Stone-Baker letters is limited to tuning only that portion of the wire above the [fol. 1743] coil in Stone's antenna circuit. Hogan points to nothing that transpired between the Stone-Baker letters of 1899 and the 1902 amendment to change the definition of "vertical wire" from wire in only one part of the circuit to all of the wire in the circuit.

In answers to cross-questions 108, 110, and 111, page 1871, Hogan, in further testifying about the Stone system, admits that the system disclosed by Stone is in fundamental components and mechanical arrangement the same as that of Marconi, merely holding out as a difference the tuning of the antenna circuit in the Marconi patent over his alleged tuning of only that portion of the wire above the coil in the Stone patent, so that all that remains is whether or not Stone, in instructing to tune *all* of the circuits to the same frequency and one to another, is to be absurdly construed into an electrical impossibility or is to be given the clear meaning of the plain language employed by Stone in disclosing his system to another, and there is no question that anyone tuning the circuits "empirically," as instructed by Stone, would arrive at tuning the antenna circuit as a whole because it can be done and would not arrive

at tuning that portion of the wire above the coil because this kind of tuning can not be done.

* * * * * * *

33. Question. Please refer to the testimony in answer to question 13, page 1917 and following, and state whether or not you agree with the opinions expressed therein, giving your reasons.

* * * * * * *

Answer. The testimony in answer to question 13 is that of claimant's expert Waterman in the matter of the construction and mode of operation of the apparatus described in the Stone-Baker letters and my opening testimony concerning these letters.

Waterman begins his testimony with the statement that he agrees with the statements and opinions of Weagant in his testimony as to these letters, and I therefore understand him to adopt the electrically absurd contention of Weagant that the tuning of the antenna in the Stone-Baker letters is limited to a portion of a circuit which can not by itself be tuned.

At the bottom of page 1917 Waterman states:

"Mr. Loftin, in my opinion, entirely overlooks or ignores the point of view of the art and the meaning of terms. What the art then knew as a wireless telegraph system comprised an elevated wire hung in the air and one terminal connected to earth; there was one such wire at the transmitter and the other at the receiver."

That I do not so boldly and audaciously overlook or ignore the art as does Waterman is readily checked from the remainder of the quoted paragraph in which he makes the charge against me. He unqualifiedly states that all the art knew of wireless telegraphy system at the time of the Stone-Baker letters in 1899 was the simple 1-circuit transmitting system and 1-circuit receiving system of Marconi reissue patent No. 11913. In view of the art having [fol. 1744] dates between the Marconi reissue patent's original date and the Stone-Baker letters that have been introduced in this case contradictory of Waterman no testimony could be bolder and more in disregard of the facts. At the date of writing his first letter to Baker, Stone had available Lodge patent 609154 in suit in which Lodge had employed two circuits at the transmitter and two circuits

at the receiver, thus removing the spark gap from the antenna circuit and the detector from the antenna circuit to make systems substantially removed from that of Marconi reissue patent 11913. In Marconi patent 627650 the detector was removed from the antenna circuit at the receiver and placed in a lateral circuit, and tuning of both circuits provided for, a system entirely removed from that of Marconi reissue patent 11913. It did not include the loose coupling between the two circuits specified by Stone. Reference to my sketch L opposite page 1069 clearly shows that the Lodge system available to Stone was decidedly more than the mere vertical wire system of Marconi reissue patent to which Waterman limited himself. The same is shown as to Marconi patent 627650 in my sketch M opposite page 1071. I have also referred to Tesla patent 645576, which showed the spark gap removed from the antenna circuit at the transmitter, and two circuits at the receiver, and Stone specifically referred to this Tesla system in his letters to Baker, thus showing that Stone knew of features well beyond that of the simple arrangement of Marconi reissue patent 11913, including tuning between the lateral circuit and the antenna circuit. I also referred on page 1076 to British patents Nos. 1862 of 1899 to Braun and 12420 of 1899, showing two circuits at each transmitter and receiver. Thus clearly the art expressed for Stone ideas and points of view well beyond the simple knowledge of Marconi reissue patent 11913 to which Waterman alone pointed.

* * * * * * *

On page 1918 Waterman states that the Stone-Baker letters are mathematical in character and show that what Stone was engaged in was a "mere excursion into mathematical realms." Waterman's attempt to confuse these letters by designating them as "mathematical in character" is apparent from the letters themselves, the fact being that Stone uses no mathematics in either the letter of June 20 or the letter of July 18. In the letter of July 18 Stone uses a well-known expression for wave motions merely to make definite to the usual physicist his conception of the type of oscillation in the old Marconi single-circuit transmitting system. The expression is not used by Stone in any mathematical sense in that he makes no attempt to arrive at any deduction from mathematical operations but merely to portray to one knowing the language of physicists a wave form

visualized by Stone from deductions arrived at by physical conceptions, which he fully expressed in words on page 3 of the letter of July 18 immediately preceding his employment of the expression to sum up the results of his verbally expressed deductions. The letter of June 30 is a complete disclosure by the clear use of fundamental electrical terms [fol. 1745] and language and complete electrical diagrams of the systems and is wholly devoid of any mathematics or the use of any expressions of mathematical form of any kind. The letter of July 18 elaborated the theories around Stone's conception of the system of the letter of June 30 and, except for the one visualizing expression, is devoid of mathematical effort, the theories being expressed in clear, fundamental electrical terms and language.

Immediately following on page 1918 Waterman says that Stone undertook to provide for simplifying the radiation by "avoiding the shock of discharge," discharge referring to the discharge of a spark gap used for aiding in generating oscillations in an electrical circuit. That this is misleading as to what Stone actually did is evident from the fact that Stone did not "avoid" the shock of a discharge, as the letter of June 30 and diagrams on page 3 thereof clearly show that Stone employed the discharge of a spark gap and therefore could not "avoid" its shock. What Stone did was to remove the discharge from the antenna circuit to a lateral circuit and to diminish the effect of the discharge on the antenna circuit as much as possible by the use of loose coupling between the circuits.

Further on on page 1918 Waterman states that Stone expected by his procedure to realize simple oscillation in the antenna circuit which "he thought he had mathematically proven to be represented by the simpler mathematical expression 'A_a sin pt.'" That Waterman misrepresents the Stone-Baker letters is evident from the fact that Stone employed no mathematical operations in his letters for proving anything, merely pointing out after clear and intelligently expressed mental deductions that by not trying to generate his oscillations directly in the antenna circuit but in a lateral circuit well removed by loose coupling from the antenna circuit, the harmonic components in the oscillations visualized in the terms in the expression following the first term could be substantially eliminated, thus leaving substantially a simple harmonic oscillation represented by the first term "A_a sin pt."

Immediately following on page 1918 Waterman states:

"Stone's idea was a very natural one. Forced oscillations were well known and according to Stone's mathematics, expressed in the second Baker letter, he should be able to force oscillations on the wire of his mathematically simple form, without regard to the fundamental frequency or wave length of the wire."

It is true that Stone's ideas were natural to the clear-thinking scientist he was. It is not true that "forced oscillations" of the kind with which Stone was dealing were well known. Stone was the first to appreciate that when dealing with "free" oscillations generated in a circuit with the aid of a spark gap it was necessary to employ loose coupling in order to generate the oscillations in simple form, instead of a complex form resulting from reaction of the coupled circuit back onto the generating circuit, and thus to give him a simple oscillation with which he could drive or excite the antenna circuit, for unless he started with a simple oscillation he could have no hope of forcing a simple oscillation upon the antenna circuit. Thus it is clear that Stone did not arrive at success, admitted by Waterman in the quotation above, "according to Stone's mathematics" as inferred by Waterman, but as a result of Stone's intelligent comprehension of the necessity for loose coupling.

[fol. 1746] The latter part of Waterman's quoted statement admits that Stone could produce simple oscillations in the antenna circuit without regard to the tuning of the antenna circuit, which is true, but I do not understand this part of Waterman's testimony to mean that Stone did not intend tuning his antenna, for in the paragraph of Waterman's testimony, page 1918, Waterman states:

"Stone proposed to retain the transmitting wire with its fundamental frequency of oscillation or wave length, * * *" and "he proposed to force thereon 'oscillations from an oscillator, which oscillations shall be of a frequency corresponding to the fundamental of the wire.' "

Analyzing these statements it will be seen that Waterman refers to the fundamental frequency of oscillation of the transmitting wire as the "wave length." Obviously an antenna circuit, including all of the wire in the circuit whether forming elevated wire, wire making up coils, and wire connecting coils to ground, can have but one wave

length, which is the wave length that spreads out into space to travel to distant receiving stations, and Waterman admits that the oscillations coming to the antenna circuit from the primary circuit have this same frequency or wave length, which necessarily implies that the circuit in which the oscillations are generated also has the same frequency of wave length. This reduction of the matter to the expression "wave length" by Waterman emphasizes the absurdity of the contention by claimant's witnesses Waterman, Weagant, and Hogan that the tuning in Stone's antenna circuit referred to that portion of the antenna wire above the coil in the antenna circuit. No one ever succeeded in getting a radio antenna to radiate a "wave length" represented in frequency by only that portion of the antenna circuit above the coil. At the bottom of page 1918 Waterman states:

"Furthermore, Stone's idea was perfectly sound; he could force oscillations on the wire, but the trouble was, and is, that they are of no use, because of their excessive feebleness."

Waterman gives no engineering or physical reasons in support of his condemnation of Stone's system on the grounds of "excessive feebleness" and the fact is that he could not do so because there is no basis whatsoever for the unsupported condemnation. Stone's idea was perfectly sound, as admitted by Waterman, and his system was, as a result, a success.

Waterman next states, at the bottom of page 1918, that Stone's mathematics was defective because of the incompleteness, and without supporting this statement with anything that might be analyzed by a mathematician or physicist he makes the meaningless comment that "nature does not work that way," further adding that the scheme proposed by Stone is "utterly worthless," and again makes no effort to support these sweeping statements with physical considerations that could be analyzed and answered by a qualified respondent. Stone used no mathematical operations in arriving at his deductions, and therefore his deductions could not be defective because of incompleteness of mathematics not used. The expression he did use, quoted on page 1918 by Waterman, to merely portray in the fashion of a physicist some wave forms is more than adequate in

completeness, and deserving of no criticism on the part of Waterman.

[fol. 1747] At the top of page 1919 Waterman states "Stone followed the thought of the art," and immediately following makes it clear that what he means by the expression "thought of the art" is the subject matter contained in Marconi's first patent reissue 11913. Stone did not do any such thing, and it is absurd to imply that Stone had no more before him than the mere one-wire system of the Marconi reissue patent. At the bottom of page 6 of the letter of July 18 to Baker, Stone makes specific reference to his system being the same in substantial respects to that of Teslà, a 2-circuit system well removed from the early Marconi system, and I have heretofore pointed out that between the early Marconi system and the Stone-Baker letters there was much in the way of 2-circuit systems for Stone to consider as advances over the early Marconi system.

In the same paragraph Waterman argues that Marconi's line of thinking differed from that of Stone in that Marconi disregarded his former work, pointing out as a detail of basis of his view as to Marconi that Marconi inserted a coil in the antenna circuit, thereby making the antenna circuit not dependent upon the frequency characteristics of the elevated wire alone but to include the effect of the coil. Why Waterman attributes a new line of thought to Marconi with this as the basis in view of Lodge patent in suit 609154 is not clear, since one of the outstanding features of the Lodge patent in its bearing on the present suit is this matter of the coil in the antenna circuit. He also uses this to differentiate from Stone, who, in practically every figure shown in his letters to Baker and in his patents, included a coil of wire in the antenna circuit.

In the second paragraph, page 1919, Waterman states:

"This is a wholly different concept from that of Stone; Stone's instructions were to erect a wire having a given fundamental wave length, to construct an oscillator that would generate oscillations of that same wave length and to force these oscillations upon the wire."

Stone did not instruct merely "to erect a wire," but clearly showed in his June letter to Baker an antenna circuit having an elevated wire connected to ground through a coil of wire, and on the last page of the letter provided for

tuning this wire, coil, and ground connection as one of the "circuits" of his system to the "same frequency" as all of the other circuits, and thus did not miss Marconi's concept, as alleged by Waterman, but went far beyond it in his further intelligent instructions providing for practical means of securing such loose coupling that the system was effectively operative for carrying out the adjustment of the time periods of the circuit to be the same as an aid to selective communication.

Continuing in the second paragraph on page 1919 Waterman now states that Stone did not obtain the simple oscillations that he expected and, without giving any reasons therefor, states that such oscillations as Stone could have produced would have been more complex than represented by the expression given in the second Stone letter, all of which is directly contradictory of his testimony on the preceding page where he states that Stone's idea was perfectly sound. The truth of the matter is that Stone did approach with the aid of his loose coupling to most practical degree the simple harmonic form of oscillation in the antenna he set out to acquire, and I have previously given in detail the reasons why Stone succeeded in a way readily [fol. 1748] analyzable by the average electrician. I regret Mr. Waterman does not do the same, as I would appreciate knowing the reasons for the unsupported statement contradicting his previous admission of the correctness of Stone's idea.

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In the third paragraph on page 1919 Waterman charges me with reaching my conclusions as to the Stone-Baker letters by disregarding what Stone says and saying something which he does not state, this without taking up specific instances of my references to pertinent parts of the Stone-Baker letters, which I analyzed in detail, and showing wherein my analysis warrants the sweeping statement of Waterman that I disregarded what was said in the letters. Waterman follows the charge against me by the unexplained statement that Stone throughout deals solely with "the existing concepts of the art" without endeavoring to let one know what comprises the "concepts," or showing any relation of these undefined concepts to his further statement that Stone "undertakes to produce forced oscillations of the frequency of the wire itself." Neither does he under-

take to support his statement at the end of the same paragraph that the idea of forced oscillation was a familiar one to the art by any reference to literature of the art existing at the time of the Stone-Baker letters, and the testmony of Weagant, to which Waterman refers, does not supply any references satisfying Waterman's omission thereof.

In the next paragraph (par. 4) of page 1919 Waterman states that my crediting the Stone-Baker letters with the disclosure of an antenna circuit comprising a wire and a coil proportioned to have the fundamental frequency of the circuit in which the oscillations are generated is not stated or suggested by Stone "either in words or in mathematical symbols, nor is it in any way implied by his diagram." I have previously pointed out in connection with the testimony of Hogan that Hogan admits the Stone-Baker letters to include in diagram all that is necessary for tuning the antenna circuit to the same extent that Marconi's patent 763772 provides for such tuning, Hogan merely pointing out that a diagram of itself can not indicate whether or not there is actual tuning between the primary circuit and the antenna circuit, Hogan adding that the text of the descriptive matter is necessary to determine this point, so that Waterman's objection that the tuning in the Stone-Baker letters is not "implied by his diagram" is not a reasonable one. Waterman's statement that the tuning of the antenna circuit is not stated "in words" does not accord with the facts, for the June letter specifically provides for making the tuning or fundamental of the antenna wire, and Stone shows the wire to include the wire of the coil and the wire of the connection to ground, the same as the frequency of the oscillations generated in the primary circuit, and summarizes these instructions on the last page of his June 30 letter as providing for tuning all of the circuits to one and the same frequency. Statement for tuning with the aid of words could not be more definite and clear. It is true that Stone did not refer to the tuning in "mathematical symbols" expressed in mathematical formulæ, but instructions for tuning all of the circuits to the same frequency on the [fol. 1749] last page of the June 30 letter stated that it could be accomplished mathematically, Stone well knowing that this reference to "mathematically" immediately brought to the mental eye of the type of man to whom he was addressing himself all of the symbols of the then well-known mathematical formulæ given by Stone in the second column

of page 5 of his patent 714756, making a physical display of these symbols unnecessary.

In paragraph 5, page 1919, Waterman states that the July letter makes in clear "mathematically" that Stone regarded it quite immaterial, so far as attaining his results of simple oscillation, whether the oscillator was made to have the frequency of the wire or not. Stone makes no attempt to show any such thing mathematically. As before stated, Stone undertook no mathematical operations, and therefore made no mathematical deductions of any kind. By a well-known shorthand language, so to speak, of physicists he drew by an expression a picture full of meaning to a physicist the result of his mental deductions as to the type of oscillation occurring in the simple system involving a spark gap within an oscillatory circuit, a thing in no way involving the practice of setting up mathematical equations expressive of actions and reactions in electrical systems, and soiving these equations to mathematically deduce facts or conclusions. There is not a mathematical equation set up anywhere in the second Stone letter, and without an equation no solution or deduction is possible.

It is true that Stone's second letter does make it clear in words that in so far as the feature of production of simple harmonic oscillations in the antenna circuit is concerned with his system of generating the oscillations in a primary circuit with loose coupling it makes no difference whether the antenna circuit is tuned or not, but Stone nowhere suggests or implies that the statements in his second letter in fuller explanation of his theories as to the system of his first letter are to alter his instructions for construction and use of the system in his first letter. His knowledge that the matter of production of the simple harmonic oscillations he wanted in the antenna circuit is not affected by the state of tune of the antenna circuit he later put to good use in his proposal in patent 714756 for multi-telegraphy from the same antenna circuit having coupled thereto two or more generating systems, so that this additional knowledge of Stone operates to his credit rather than to the discredit that Waterman attempts to impose. Stone was after producing simple harmonic oscillations in an antenna circuit. His letter of June 30 shows that even though Tesla had taken the step of generating the oscillations in a circuit coupled to the antenna circuit and the two circuits adjusted to have the same time period, that Tesla had failed to produce

simple harmonic oscillations in the antenna circuit because Tesla had made no provision for so coupling the two circuits loosely that they would not react upon each other to such extent as to permit the generating circuit to independently determine or set the frequency of the oscillations. Having determined that loose coupling permitted this independency of action of the generating circuit Stone makes the observation in the second letter that the independency persists even though he should depart from the Tesla system to the extent of detuning the antenna, but the observa- [fol. 1750] tion carries no implication that the specifically described tuning of the first letter is to be abandoned.

In my opening testimony as to the Stone-Baker letters I endeavored to support my interpretation of these letters by definite technical considerations and reasoning, and note that Waterman has not attempted to take any one or more features of my technical reasonings and point out errors or fallacies in them, but has in fact dodged the situation by stating at the middle of page 1919 that he does not think it "profitable to consider in detail" my comments, and has accordingly satisfied himself with making sweeping statements that I have disregarded what Stone says, and the like, without pointing out for answer by me wherein any technical considerations disregard Stone's sayings, so that I have been compelled to confine myself to taking up his testimony in detail to point out wherein his sweeping statements are not supported by the facts, and his criticisms of the Stone disclosures are unjustified.

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34. Question. Please refer to the testimony in answer to question 2, pages 1880 and following, and state if you agree with the statements therein, giving your reasons.

* * * * * * *

Answer. The testimony referred to in the question is that of claimant's expert Waterman as to features of Marconi patent 763772 in suit, and specifically as to the duration of oscillations in the primary circuit at the transmitter and coupling between the primary circuit and the antenna circuit. Also as to the bearing of the Marconi address of April 17, 1912, upon the matter of coupling between the circuits, and also as to features of my first testimony as to Marconi patent 763772.

My disagreements with the statements of Waterman's testimony are numerous and I give my reasons as I consider his statements.

In beginning the third paragraph from the bottom of page 1880 Waterman, without qualification, states that "Mr. Marconi founded a wholly new art." He does not state to what part of Mr. Marconi's work he refers in making this unqualified statement, but since it is clear from this case that Marconi's first radio work was that embodied in reissue patent 11913, it is difficult to reconcile Waterman's sweeping statement with the facts concerning the subject matter of the reissue patent. I have shown that the matter of exciting an elevated wire with alternating currents to cause electrical effects to spread out into space for detection was in existence prior to the Marconi reissue patent, and in fact the transmitting system of the Marconi reissue patent was that of Hertz some 10 years earlier than Marconi. I have also pointed out in detail the existence of features of the Marconi reissue patent receiving station prior to the patent, the coherer used by him being a device of Branly, as well as the tapper on the coherer also being prior to the reissue patent. It doesn't seem reasonable, therefore, to unqualifiedly attribute to one person so much when so much existed prior to him.

[fol. 1751] In the same paragraph Waterman gives to patent 763772 the unqualified position of constituting the "second great step," in such way as to imply that nothing of value happened between the time of the Marconi reissue patent and patent 763772, a period of some four years, which, of course, is not the case. In the meantime Lodge, Tesla, Braun, Stone, and Pupin had taken the spark gap out of the antenna circuit at the transmitter, and the same workers had taken the detector out of the antenna at the receiver, with the addition of Marconi having done so in patent 627650. Tesla and Marconi had provided for tuning the antenna circuit to the lateral circuit and Stone had added to this feature the matter of loose coupling in order to permit of dealing with simple harmonic form of oscillation for a high order of selectivity. Clearly much went on between the Marconi reissue patent and Marconi patent 763772 worthy of admission rather than ignoring as does Waterman.

In the last paragraph, page 1880, Waterman states: "It is necessary to bear in mind the fact that Marconi in his

patent, which is reissue 11913, had disclosed a new phenomenon in the electric arts in general."

Waterman makes no statement as to the nature of this "new phenomenon" attributed to Marconi in reissue 11913, and what it can be is difficult to surmise. If he refers to the production of so-called Hertzian waves he can not attribute this to Marconi as new phenomenon without admitting lack of knowledge of the disclosure in the reissue patent, for on page 1, lines 10-14, Marconia opens up the specification of his reissue patent by conceding this phenomenon to Hertz.

At the end of page 1880 Waterman states:

"In the ordinary practice of the electrical art a path for an electric current had to be closed. Current did not flow in an open circuit."

Unquestionably in referring to an "open circuit" he has reference to the antenna circuit of reissue patent 11913. Hertz used an open circuit, his circuit constituting two simple rods with a spark gap between them, there being no wire or other conductive connection between the outer ends of these two rods to close the circuit, and there is no difference between the open circuit of Figure 9 of reissue patent 11913 and the open circuit of Hertz. I have also referred to open circuits comprising an elevated wire connected to ground in connection with the Edison patent, Dolbear patent and the Tesla-Martin book, all long prior to Marconi reissue patent, and being the grounded elevated wire system of Figure 10 of this patent. Marconi, certainly, is not entitled to any suggestion that he was the first one to cause current to flow in an open circuit.

At the top of page 1881 Waterman refers to Marconi having hung the vertical wire in the air and causing high frequency currents to flow between the top of the wire and the earth. While he does not definitely state that he gives Marconi credit for being the first to do this, it may be intended as a part of his broad statement that Marconi founded the wholly new art. All of Hertz's work was carried out with high frequency currents, and in fact very high frequency currents. He did not hang a wire in the air, but did the equivalent thereof by using rods of length. Tesla, Edison, and Dolbear did hang wires in the air and connected them to earth.

[fol. 1752] In the second paragraph of page 1881 Waterman states:

"This was an amazing phenomenon, and, still more amazing, he found that this wire caused electric waves to be radiated like invisible light from a lighthouse, which followed the surface of the earth."

Thus Waterman attributes to Marconi finding "electric waves," when even to-day there is no accepted theory of the mode of propagation of the energy to a distance from a radio transmitting system. Hertz had already found out and published that the energy of a current flowing in a conductor was transmitted through space, and had investigated many of the properties of this energy transmission some 10 years prior to Marconi, and there was certainly no reason for the effect being newly "amazing" when repeated by Marconi, particularly as many others had understood and repeated the effects following the work of Hertz.

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In paragraph 7, page 1881, Waterman states that Lodge made the mistake in his Figure 4 of getting a combination which afforded two radiating circuits, and accordingly the arrangement was useless, the statement being made without showing wherein there are two radiating circuits and why, if there were two such circuits, the arrangement would be useless. My sketch L, opposite page 1059, shows in the left hand figure the Lodge transmitter, and it is clear that there is but one elevated wire or radiating circuit, and that the arrangement is substantially the same as the telefunken type transmitter in plaintiff's Exhibit 79, opposite page 180, introduced by claimant as an example of an infringing device rather than a useless system.

In the next paragraph, page 1881, Waterman attributes to the Marconi patent in suit the taking of the spark gap out of the antenna and the detector out of the receiving antenna, though this had already been done by Lodge, Tesla, Braun, Marconi in patent 627650, and Stone.

At the top of page 1882 Waterman classifies the antenna circuit of Marconi's transmitter as a "good radiator," and the lateral circuit as a "persistent oscillator," taking the expressions from the Marconi patent attributing advantages in a radio transmitting system of these types of circuit. Of course, the mere naming of these circuits with the

terms used in the Marconi patent did not give to Marconi any advantage of these kinds of circuits already used by Lodge, Tesla, Stone, Marconi in patent No. 627650, and Braun. In the case of Lodge he provided that the lateral circuit be arranged so that it could not act as a "persistent oscillator," but Tesla, Stone, and Braun did not impose this limitation upon their lateral circuit.

At the end of the second paragraph, page 1882, Waterman states:

"Only by the tuning of the two circuits together in this manner can the energy be transmitted quickly and efficiently from one circuit to the other."

It is not true that this mere tuning of the two circuits or adjusting the time periods to be the same permits of [fol. 1753] efficiency, a matter which is clear from Waterman's own testimony in the first instance in this case, he having stated on page 153, paragraph 1, as follows:

"If the circuits are too closely associated, defects of operation occur as the result of the reaction of the secondary circuit back upon the primary circuit. It will be seen that since two circuits are associated so that energy may be transferred from the non-radiating circuit, to which the charge is initially given over into the radiating circuit it necessarily follows that the oscillations occurring in the secondary circuit would have a tendency to react and produce oscillations in the primary circuit and thus transfer the energy which the antenna circuit has received back into the primary circuit instead of radiating it. If the association of the two circuits is so close that this tendency to the backward transfer of energy is great enough to cause the spark gap in the primary circuit to be broken down, then energy will be retransferred back to the primary, thus wasting it in the spar gap instead of radiating it as an electric wave."

Thus in Waterman's prior testimony loose coupling was an essential feature of efficiency in the transfer of energy from the lateral circuit to the antenna circuit in the system of the Marconi patent.

In the third paragraph, page 1882, Waterman refers to the provision of the local circuit, permitting the use of a large condenser capacity as compared with the relatively

small capacity of the antenna circuit as "the concept, which is fundamental to this invention." If this is so, the fundamental concept did not arise in the Marconi patent. Lodge, Tesla, Stone, and Braun had lateral circuits in which the capacity of the condensers used in these lateral circuits could be chosen large compared to the capacity of the antenna circuit, and thus these prior systems had local circuits which could act as reservoirs and good conservers of energy, attributes given the Marconi arrangement by Waterman.

In the last paragraph but one, page 1882, Waterman states that transformers whose couplings were as low as 85 per cent would have been quite useless except for very special purposes. Why he makes this statement is inconceivable, as couplings of less than 1 per cent are used and have long been used with great utility. As early as 1894, it was proposed to use high-frequency currents in wire communication practice, in systems not involving iron core transformers and accordingly with couplings much less than with the couplings used in transformers for transmission of power by wires. Stone, prior to Marconi, provided for very loose couplings; that is, of the order of 1 per cent, and this loose coupling of Stone has been found useful in many ways.

At the bottom of page 1883 Waterman states that I have based my entire deposition as to Marconi patent 763772 upon the assumption that the expression "a persistent oscillator," used in the patent, is to be understood with a significance that I myself imposed, which is not a fact. The patent not only refers to the lateral circuit as a "persistent oscillator," but adds the instruction that the oscillations in this circuit are to be "maintained for a long time in the primary circuit," which is not only plain English but has definite technical significance, when taken in conjunction with the complete system described by Marconi. It, in fact, takes the place of the loose coupling definite instructions in the Stone patent and Stone-to-Baker let- [fol. 1754] ters. Marconi shows a system having 4 circuits, 2 at the transmitter and 2 at the receiver, and clearly states that the purpose of this system is to obtain selectivity, adding that all 4 circuits are to be adjusted to have the same time period. If Marconi had started wrong at the transmitting station by transferring the energy too quickly to the antenna circuit, and thus not maintain it for a long-

time in the primary circuit, he would have produced a 2-frequency or double hump form of transmission in the presence of which the tuning of the two circuits at the receiver would have meant nothing, and his selectivity system would have collapsed. It is, therefore, not necessary to rely upon the mere expression "persistent oscillator," for the Marconi patent is replete with information as to why the maintaining of oscillations in the primary circuit for a long time was a real factor of the system.

In the third paragraph of page 1884 Waterman states that he finds the number of oscillations in the primary circuits, when not connected to the antenna, of the tunes of the Marconi system listed on page 4 of the patent, stating these to be 12 oscillations for tune 1, 5 oscillations for tune 2, 15 oscillations for tune 3, and 5 oscillations for tune 4. In arriving at the resistance of the spark gap, he refers to page 185 of the work entitled "Principles of Electric Wave Telegraphy and Telephony," by J. A. Fleming, 1906 edition, claimant's Exhibit 320, taking the resistance he used from the curves shown on said page for "zinc or brass balls," but does not state in figures just what value or values he did take from these curves, but I assume that he took all he could, as we have disputed the value of resistance in a spark gap, my contention having been that 1 ohm used by Waterman in a calculation in his prima facie testimony was too high. I note, using the curve in Fleming, page 185, for the length of spark gap given in the table of Marconi tuning, the resistance would be approximately 1 ohm. However, I note that the data on the curve sheet shows that the resistance measurements of the gap were taken with a capacity equal to 1,070 micro-microfarrads, whereas in the tunes of Marconi to which Waterman referred the smallest capacity is 4,112 micro-microfarrads for tune 3 and the largest is 16,849 micro-microfarrads for tune 4. I note in the text, on page 185 of Fleming accompanying the curves, the statement that the "spark resistance increases with spark length very rapidly for small capacities, but much more slowly for larger capacities." Since the curves were taken with a capacity of about one-fourth the smallest capacity in the Marconi tunes, and about one-fifteenth of the largest capacity of the tunes, evidently they do not represent curves that would have been had with the much larger capacities of Marconi, and the information in the text is that the resistance of the gap would be less for the large

capacities of Marconi, so that I still consider that the one-half ohm I originally gave is more nearly the correct value for practice.

Since the number of oscillations is greater for a small gap resistance, obviously the number of oscillations given by Waterman for each of the tunes is too low because of this one feature.

Again, Waterman states that in arriving at his findings he assumed that the spark gap would cut off the oscillations at 10 per cent, an assumption he was not entitled to make. The fact is that with an open gap the oscillations would [fol. 1755] continue well beyond 10 per cent. Even in a well designed quench gap very special adjustments have to be made in order to reduce the energy to substantially zero in order to get a cut-off, so that properly considering this feature Waterman should add more oscillations than given according to his findings.

It is also noted that the findings are based on the assumption of the primary circuit disconnected from the antenna circuit, which, of course, is not an operating condition and one which does not give all the factors for using the mathematical formula around which calculations of this kind are made. On page 1054 I set up this formula, which shows that the decrement *d* of a circuit depends upon the ratio of the capacity to the inductance of the circuit, so that the larger the inductance the smaller the decrement, and therefore the more the number of the oscillations. When the lateral circuit is coupled to the antenna circuit, the inductance factor in the formula is increased by the amount of the mutual inductance between the circuits, so that had Waterman assumed the correct operating condition he would have had factors to go into the formula to give a smaller decrement or a still greater number of oscillations.

I therefore find that the findings of Waterman as to the oscillations in the primary circuits of tunes 1, 2, 3, and 4 of the Marconi patent are subject to three additional considerations, each one of which increases the number of oscillations over those given by him.

On page 1885 Waterman states that the number of oscillations required to transfer the energy of the primary circuits to the secondary or antenna may be found with the aid of a formula on page 247 of Morecroft's Principles of

Radio Communication, 1921, claimant's Exhibit 321. The Morecroft formula referred — requires knowing the degree of coupling between the circuits in order to make the required calculation. Waterman states that many years ago he actually measured the couplings of tunes 1, 2, 3, and 4 of the Marconi patent, but is not now able to find his records of these measurements. If Waterman ever attempted to make measurements of the couplings involved in these tunes I do not understand on what basis he can contend that the results of his measurements represent anything disclosed in the Marconi patent, as there is not enough data given in the patent for determining any specific degree of coupling. In the absence of Waterman's notes I have no way of knowing his mode of approach of the problem, but it is certain that no answer having any value could ever be arrived at without knowing all of the features of the transformers employed by Marconi in coupling the primary to the antenna circuit. The table of tunes on page 4 gives the information that Marconi used transformer No. 1 in tunes 1 and 2 and transformer No. 2 in tunes 3 and 4. On page 3 of patent 763772, lines 26 to 40, Marconi gave details of the construction of these transformers, but these details are incomplet so far as they will permit of determining the degree of couplings between the circuits in which they are used. The descriptions refer to Figures 3 and 4 of the drawing, and referring to Figure 4 it will be seen that the turn of wire *d*, which goes into the primary circuit, is spaced from the two turns of wire *d'*, which go in the secondary circuit. The [fol. 1756] degree of spacing between these wires is all vital in the matter of determining the coupling between the circuits, and this spacing is not given, so that Waterman has no right to say that he has constructed tunes 1, 2, 3, and 4 of the Marconi patent in such a way as to arrive at a degree of coupling between the circuits.

I have no objection to the formula of Morecroft used by Waterman, but insist that he has no right to use the formula which depends entirely upon knowing the coupling, without pointing to some more certain way of ascertaining the coupling than his recollection of notes made by him many years ago and since misplaced.

It is particularly odd and interesting to note that Waterman states that he has "a definite independent recollection

of the first and the third tunes," and states his recollection of the first as 10 per cent and of the third as 11 per cent. Reference to the data given by Marconi as to tunes 2 and 4 shows that in one case the antenna coils *g* had 45 turns and in the other case 100 turns, while no turns were used in this coil in tunes 1 and 3. This use of 45 and 100 turns in coil *g* makes it conclusive that if Waterman had been actually able to set up definite systems on definite specifications of Marconi and found couplings of 10 per cent and 11 per cent for tunes 1 and 3, respectively, he would have found that the addition of 45 turns in the case of tune 2 and 100 turns in the case of tune 4 would have reduced the coupling enormously, and, in my opinion, to less than 1 per cent. Stone clearly brings out that a removal of a large part of the inductance of one of the circuits from the transformer used as the coupling element makes the loose coupling Stone pointed out as a part of his system, and Waterman subscribes to this principle on page 1886, near the top, where he states in reference to tune 6:

"The coupling is therefore close, since there is no added inductance in the antenna, and all the inductance of the primary circuit is coupled to the antenna."

In the case of tune 2, there are two turns of wire in the antenna circuit involved in the coupling transformer and 45 turns of wire in coil *g* not involved in the coupling, which difference means a very small coupling—in fact, so much so that it may be that the reason Waterman could not remember these two particular couplings is that they were too small to remember. In any event, there is no basis for Waterman contending that Morecroft's formula shows that the number of oscillations required to transfer all of the energy from the primary to the secondary circuit in tune 1 is 5 and in tune 3 is 2½. Furthermore, if he had applied Morecroft's formula to any reasonable estimate in the case of tunes 2 and 4, which he forgot, he would have found something in the neighborhood of 50 complete oscillations would be required, theoretically, but in fact the oscillations in the primary circuit would have dissipated their energy and extinguished before this number could be reached.

I also again find difficulty with Waterman's mathematics in this matter of oscillations in the circuits, having in my

prima facie testimony shown that in a case where Waterman calculated but 3 oscillations I calculated 24. I now note that with his questionable 10 per cent coupling in tune 1 he finds 5 oscillations by applying the Morecroft formula, and with but 1 per cent difference, or 11 per cent, in tune 3, he finds from Morecroft but 2½ oscillations. My calcula-
[fol. 1757] tion from Morecroft's formula for tune 3, using the questionable 11 per cent coupling, gives 4½ oscillations.

I note that the page of the Morecroft book (p. 247) to which Waterman referred for the formula points out that after all of the energy is transferred from the primary to the secondary circuit by a given degree of coupling the energy will then flow back from the antenna circuit to the primary circuit, unless there is something to prevent it from doing so. To accomplish this prevention, Morecroft states: "Such an action is accomplished by a 'quenched' spark gap to be described in detail in Chapter V."

Thus Morecroft brings out that the oscillations in the primary circuit calculable from his formula up to the time all the energy is transferred to the second circuit are not all of the oscillations that will take place in the primary circuit, as the energy will come right back from the antenna circuit and keep up its oscillations in the primary circuit, unless something is inserted in the primary circuit to prevent this, and he states that the "quenched" form of spark gap is the thing that makes such prevention possible. In other words, the subject matter of Morecroft used by Waterman does not apply to the Marconi system, but to the "quenched" gap system which came some 9 or 10 years later.

In the third paragraph, page 1885, Waterman sums up his recollection of the tunes of the Marconi patent to give the range of coupling from 5 to 20 per cent. On pages 1037 and 1038 I calculated the effect of coupling on producing double frequency, taking a specific example, and showed by calculation that with a coupling of 20 per cent the receiving circuits could not be tuned the same as the transmitting circuits with any hope of receiving any one of the two frequencies emitted, and that it was not until the coupling was reduced to the order of 3 per cent would the two humps or frequencies come close enough together for a tuned receiver to be effective. Waterman's questionable range of from 5 per cent to 20 per cent for the Marconi patent

therefore deprives it of being the selective communication system that Marconi so explicitly stated was his purpose, and Marconi's comments in his 1912 address make it clear that he was not then in accord with Waterman's present attempt to make Marconi a tightly coupled system to the extent that tuned receivers can not function with it. On page 1889 Waterman quotes an excerpt from this Marconi address, in which Marconi states that unless weak coupling is employed two frequencies will be created to divide into two waves of different lengths, and further refers to the receiver having to be tuned to but one of these, thus making it clear that Marconi himself admitted that under these conditions his system of tuning all four circuits to the same frequency could not be utilized.

These calculations of mine and the frank admission of Marconi and Waterman's own testimony on pages 152 and 153 makes Waterman's present statement on page 1886, paragraph 3, that "Marconi, of course, was entitled to make the association of the two circuits anything that he pleased" rather strained.

Near the bottom of the same paragraph Waterman states that the word "coupling" in this connection was not known at the date when the patent in suit was written. It may be true that the word itself was not in vogue, but the effect of [fol. 1758] coupling and how to properly use it was decidedly well understood by Stone before the writing of the Marconi patent.

At the bottom of page 1886, Waterman states:

"The matter of coupling is wholly a matter of degree: it expresses the extent to which the local reservoir circuit is loaded by its association with the antenna."

This can not be true with respect to the Marconi patent because Marconi specified that all of the four circuits of the system be adjusted to have the same time periods. If the degree of coupling is exceeded beyond the neighborhood of 3 per cent, the tuning of the receiving circuits in the manner described by Marconi effectively begins to cease having any useful function.

On page 1887, paragraph 1, Waterman states that the coupling used in the transmitter is a matter of choice with the constructor, forgetting that Congress took this matter in hand very early, taking the choice away from the

constructor, placing by law such limits upon him that he could not use degrees of coupling that would interfere with other communication. Keeping within the law required of the constructor some very careful selection of coupling.

The rest of Waterman's testimony on page 1887 infers that the choice as between the open type of gap and the "quenched" type of gap depends upon the amount of power to be handled, which is not the case in practice. Both types of transmitters have been built and used of the same orders of power running from a fraction of a kilowatt to tens of kilowatts, except that when the efficiency of the "quenched" gap type of transmitter was demonstrated, the open gap type fell out of the race, particularly high powers, where efficiency became an economical factor.

In the first paragraph of page 1888 Waterman objects to my reference to "cumulative resonance" in connection with the Marconi patent, stating in effect that the provision in Marconi for adjusting the two circuits to have the same time periods is what Marconi means in his use of the word "resonance." It may be that all Marconi knew of the matter at the time was this one feature of adjusting the two circuits, but the one feature was not sufficient to give the results he sought: that is, such selectivity that the two circuits at the receiving station could also take part in the resonance of being tuned to the same frequency as the transmitting circuits. This could only be carried out by including loose couplings and therefore depending upon "cumulative resonance."

In the third paragraph, page 1888, Waterman states that my pointing out that in the Marconi patent the lateral circuit must produce oscillations which are transferred "without changing characteristics" is a meaningless expression not contained in the Marconi patent. Just because the expression is not contained in the Marconi patent is no basis for it being without meaning. Marconi illustrated an electrical system, and irrespective of what he stated in the patent the system is dominated by the laws of nature. That what I have stated is loaded with [fol. 1759] meaning is clear from the analysis of Prof. J. H. Morecroft of the effects accompanying the transfer of spark generated oscillations from one circuit to another, this analysis being covered in part on pages 238, 239, and 240 of his book entitled "Principles of Radio Communication," published by John Wiley & Sons, New York, copyright

1921. Figure 23 on page 238 shows in the lower half the current i_2 in the secondary circuit when there is a coupling of 42.4 per cent between the circuits. The photographic reproduction is not very clear, but careful inspection brings out that the wave form is badly distorted. The effect is better shown in the more perfect reproduction in Figure 24 on page 239, which illustrates conditions obtained when the coupling was 28.2 per cent. Figures 25 on page 239 and 26 on page 240 show the result when the coupling is 11.4 per cent and 7.07 per cent, respectively. All of these figures are oscillograms or photographs of current movement in the circuits investigated by Morecroft, and clearly show that the characteristics are badly distorted, the distortion being expressed mathematically by Morecroft in equation 73 on page 238. That this distortion of characteristics of the current in the circuit is not meaningless as contended by Waterman is evident from comparing these oscillograms of the coupling effects when tight coupling is used with the oscillogram on page 249 of the same Morecroft book, Figure 37, which shows the current in the secondary circuit in the lower half of the figure when the coupling is "weak," Morecroft's term for "loose coupling." It is seen that this secondary current with loose coupling conditions is substantially pure, and could be expressed in a mathematical equation like that of equation 73 on page 238 of Morecroft by one simple term rather than the complication of terms Morecroft gives for the tightly coupled systems, or in other words the "Asinpt" term of the Stone-Baker letter when Stone wrote to Baker how his system would provide for simple harmonic oscillation in the antenna circuit.

On page 240 and top of page 242 Morecroft gives information why this matter of change of characteristic has most important bearing on the Marconi system. He points out that in the case of two circuits each adjusted for a natural period of 100 cycles, taken alone, but coupled 20 per cent, there results two frequencies upon discharge of the condenser, and he shows his calculation of these frequencies from equations previously given to be 91.2 cycles and the 111.7 cycles, instead of 100 cycles, the time periods of the circuits. He then points out that if a third circuit be loosely coupled to either one of the two circuits of the system and tuned to a natural period of 0.01 second (100 cycles) the current induced in this third circuit so tuned will be much

smaller than if the third circuit was tuned to a natural frequency of either 111.7 or 91.2 cycles. These three circuits of the Morecroft system are the electrical equivalents of the two circuits at the transmitting station and a third circuit at the receiving station of the Marconi system, and Morecroft makes it clear by elaborate scientific analysis that if the transmitting system of Marconi generates two frequencies by employing a tight coupling such as 20 per cent, which tight coupling Waterman erroneously contends he has arrived at from unfounded construction of the several [fol. 1760] tunes given in the Marconi patent, that the circuits at the receiving system can not be adjusted to have the same natural periods as the circuits at the transmitting station, and it is an outstanding feature of the Marconi patent and claims thereof that the circuits at the transmitting station are to have the same time periods as the circuits at the receiving station. Reference again to the figure on page 249, a condition of loose coupling, shows that when the current in a secondary circuit is "without change in characteristics" the circuits throughout can all be tuned alike with real meaning and regard for the laws of nature. Because Waterman gives no analizable support for his sweeping statement that my analysis of the Marconi system is "an entirely meaningless expression" I have gone into the matter somewhat in detail in order to emphasize the extent to which Waterman goes in his testimony without foundation.

In the same paragraph I have just discussed Waterman tries to imply that the persistency of oscillations referred to in the Marconi patent takes its meaning from the mere removal of the spark gap from the antenna circuit to the lateral circuit, coupled with the use of the Lodge coil in the antenna circuit which has the effect of prolonging the circuit oscillations. Since Lodge also removed the spark gap from the antenna circuit as did Tesla, Braun, Pupin, and Stone, Waterman seems to leave little of glory for Marconi in his feature of "persistent oscillation."

In the beginning of paragraph 5, page 1888, Waterman accredits to me the impression that "because the circuits are in resonance, therefore the energy can not be transferred quickly from one circuit to another." I have given no basis for any such impression, but have brought out that because all four circuits of the Marconi system must be tuned to have the same periods it is not possible to combine

with the efficiency of resonant transfer the addition of rapid transfer by tight coupling. I think my testimony is clear that the adjustment of the time periods of the circuits to be the same provides for the most rapid transfer at any given percentage of coupling, but have pointed out that because of the reaction of the secondary circuit onto the first to create distortion of energy transfer the coupling must be kept sufficiently loose that the efficient transfer of resonance must itself be maintained at a low rate of transfer.

Immediately following this paragraph Waterman states that in the Marconi patent the transfer of the energy in the primary circuit takes place in "from $2\frac{1}{2}$ to 10 oscillations." I have previously pointed out that Waterman has no basis for alleging that he could determine any number of oscillations involved in the Marconi patent because of lack of sufficient constructional data from which to do so, and further that this low figure of "$2\frac{1}{2}$" he arrived at by a miscalculation in the simplest of mathematical operations, the correct figure from his erroneous assumptions being $4\frac{1}{2}$ oscillations, and that the roughest sort of estimate of tunes 2 and 4 in Marconi patent, which Waterman could not recollect, would give theoretical numbers of oscillations by using the Morecroft formula employed by Waterman in the neighborhood of 50.

At the top of page 1889 Waterman characterizes my statement concerning the Marconi address of 1912 as "nonsense," but as usual avoids any attempt to point out wherein the "nonsense" lies. That neither Marconi nor I dwelt in nonsense is perfectly clear from the review I have [fol. 1761] just made of the scientific analysis by Professor Morecroft of the matter dwelt upon by Marconi in his address. What Marconi said is precisely what Morecroft analyzed on page 240 of his book about a third circuit or receiving system being unable to resonantly receive the emanations from tightly coupled two circuits of a transmitting system. There is positively no "nonsense" associated with these facts of nature.

Waterman goes on to point out that Marconi immediately followed his remarks by reference to development of a spark gap which was capable of opening the primary circuit to thereby prevent any retransfer of energy, also referring to the "Wien spark gap." It is quite natural that Marconi in 1912 should have known about this new form of quenching gap which permitted of overcoming the diffi-

culty of his system he pointed out. Wien's work dated back to 1907 or 1908, and was immediately followed in 1908 or 1909 by valuable improvements of Count Arco and Rendhal and Seibt, German scientists, and by 1912 many quenched-gap systems resulting from the most practical work of these men were in wide use.

Waterman states about the middle of page 1889:

"Mr. Marconi did not state that the circuits were not in resonance if the spark gap did not prevent the retransfer of energy, as Mr. Loftin assumes."

The fact is Marconi did so state. He points out that the tight coupling makes oscillation of two frequencies so that the receiving circuits must be attuned to one or the other of these frequencies, and not adjusted the same as the two circuits at the transmitter. Waterman seems particularly zealous in ignoring the fact that the Marconi system is one of four tuned circuits, rather than one of merely adjusting the time periods of the two circuits of the transmitting station to be the same.

In closing his answer to question 2, on page 1890, Waterman admits that the introduction of a spark gap capable of opening the local circuit at the end of the energy transfer period was an improvement. He does not point out that this new quenched gap permitted of so tightening the coupling that the energy transfer period could be reduced to practically nothing compared to the period of useful oscillation from the antenna at the end of the energy transfer period and that the enormous distortion occurring during the energy transfer period in the characteristics of the energy being transferred would have no great practical effect on the emanations from the antenna, and that the enormous distortion during the energy transfer period precludes any possibility of energy transfer in resonant fashion, thus making the quenched-gap system an entirely different one from the slow feeding, resonant transfer systems of the Marconi patent and Stone.

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35. Question. Please refer to the testimony in answer to question 3, page 1890, and state your opinion of the corrections made by Mr. Waterman of prior testimony of his as to calculations made by him.

* * * * * * *

[fol. 1762] Answer. In Waterman's prima facie testimony he stated that he had calculated the number of oscillations in the primary circuit of tune 1 of the tunes on page 4 of the Marconi patent as 3 oscillations, and because of my practical knowledge of electrical effects I detected a possible gross divergence from the fact immediately upon reading this part of Waterman's testimony, and thereupon made the simple calculation required to verify my doubts, finding 24 oscillations instead of 3, as I had suspected. The calculation is an extremely simple one expected of any schoolboy, being a mere substitution of numerical values in the simple formula I show on page 1054, the operation thereafter being mere arithmetic. I note that Mr. Waterman accounts for his wide divergence from the fact in two steps, the first being that his assumption that he was counting the number of oscillations up to the point of the energy falling to 1 per cent of the original should have been to only 10 per cent of the original, and therefore the 1 per cent is "clearly a typographical error." Why the 1 per cent should be a typographical error is not technically clear, because Waterman was testifying about the point at which oscillations in the primary circuit would cease, and the fact is that once the spark gap breaks down to ionize the gap the oscillations do not cease when the energy has fallen to 10 per cent of the original. Even with a correctly designed quench gap it is necessary to bring the energy through the gap to a substantially zero value at the first beat if oscillation in the primary circuit is to be stopped at this point. It is my opinion that Waterman's 1 per cent in the first instance more nearly accords with the actual operation of the gap.

After changing the point of cut-off all the way from 1 per cent up to 10 per cent Waterman still found that he had 12 oscillations instead of 3, and this he accounts for by a miscalculation in a schoolboy operation in the simple formula on page 1054. It is quite possible that anyone would make a miscalculation now and then, but an engineer can generally be relied upon because of his empirical knowledge of effects not to permit such a gross error to slip by, as the result that Waterman alleges to have obtained (three oscillations) immediately called attention to the possibility of an error.

I have also referred to the fact that Waterman claimed to have calculated the result using a gap resistance of 1

ohm, whereas in my opinion the gap resistance should be of the order of one half ohm, so that in fact there were more than the 24 oscillations I arrived at by calculating with Waterman's 1-ohm assumption.

Noting the freedom with which Waterman has charged me with lack of understanding, nonsense, and the like, he must have found the necessity for using the schoolboy excuses as to the gross divergence of his three oscillations [fol. 1763] from the fact pointed out by me most embarrassing. It is to be noted that Waterman has resorted to numerous unfounded contentions that the number of oscillations in the primary circuit of the Marconi system is low, although Marconi specifically states that the oscillations in the primary circuit are to be maintained for a long time, and the fact is that Marconi is correct in spite of all of Waterman's efforts to the contrary.

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36. Question. Please refer to the testimony in answer to question 4 on page 1891 and state whether or not you agree therewith, giving your reasons.

* * * * * * *

Answer. This is Waterman's testimony in answer to my criticism of his sketch No. 2, Waterman stating as usual that I am wrong, with which I do not agree. One objection I made to Waterman's sketch No. 2, opposite page 151, and which I repeat in Figure 1 of my sketch K, opposite page 1057, was that Waterman showed the oscillations of the secondary circuit of equal intensity to the oscillations in the primary circuit, and the energy in the secondary circuit greater than the energy in the primary circuit in that the oscillations in the secondary circuit, of equal intensity to those in the primary circuit, endure for a greater length of time.

Waterman now contends that his sketch 2 does not illustrate current flow, but "the charge as determined by voltage across the condenser." Being overanxious to charge me with being wrong, he evidently failed to recollect his own testimony as to this sketch. On page 151 he states as to the sketch:

"I illustrate by Waterman's sketch No. 2 (here produced) the *oscillation of current* in the two circuits of a transmitter,

properly adjusted as described in the specification." (Italic mine.)

If I can not use Waterman's own words for what he shows without being wrong there seems to be little hope of my ever being right. That I am right, however, is clearly shown by actual photographs of the currents in the two circuits on page 249 of Morecroft's book, to which I have previously referred. In Figure 2 of my sketch K I showed my opinion of the way in which Waterman should have made his sketch 2, in order to correctly give an idea of the currents in the two circuits. My sketch is substantially a duplicate of the actual photograph of these currents by Morecroft on page 249.

I also objected to Waterman's sketch 2 on the basis that he showed too few oscillations in his primary circuit just as he calculated too few oscillations. Comparison of Figure 2 of my sketch K with the figure on page 249 of Morecroft, and a like comparison of Morecroft and Waterman, shows again that I was not wrong. It is difficult to keep the facts out of photographs.

At the bottom of page 1891 and top of page 1892 Waterman charges that I apparently do not know that the laws of nature and not my opinion determines the meaning of the language of the Marconi patent. I have formulated my opinions around a very sound foundation for appreciating a great many of the laws of nature, and I think I can safely leave it to Morecroft's photograph, page 249 of his book, [fol. 1764] of the laws of nature in operation in the Marconi circuits as to whether or not my opinion is in accord with the governing laws.

In the first paragraph, page 1892, Waterman states that my amendment to his sketch shows about 14 oscillations in the primary circuit as compared to his 4 oscillations, and that my 14 oscillations are outside of the Marconi patent and his 4 oscillations within the patent. I again refer to the photograph of Morecroft of the laws of nature in operation, page 249 of his book, and find by count that the photograph shows about 14 oscillations in the primary circuit.

In the second and third paragraphs, page 1892, Waterman states that it is immaterial whether or not the spark gap in the primary circuit fails to cut off and permit some retransfer of the energy. In his prima facie testimony, page 153, paragraphs 1 and 2, he states all sorts of defects

of operation when the coupling is so close as to bring about this retransfer, all in accord with Marconi's own statements as to this effect in his 1912 address, and which is in accord with my testimony and the scientific analysis by Morecroft on pages 238, 239, and 240 of his book to which I have referred.

In the fourth paragraph, bottom of page 1892, Waterman states:

"Mr. Loftin has apparently confused the action of a resonant receiving system gradually building up energy received from a far-distant transmitter."

From this I understand Mr. Waterman to mean that there is a transmission in the laws of nature in the matter of transfer of energy somewhere in the range of receiving energy from near by and from a distance, but like many of the other mysteries that have been attributed to radio over general electrical effects, he makes no attempt to point out when or where this transition comes into play. The fact is there is no such transition, and I again refer to the photograph of the laws of nature in operation on page 249 of Morecroft's book, which illustrates energy being transferred from one circuit to a physically closely associated second circuit. It will be noted that the photograph of the secondary current shows it to gradually build up in intensity even though the primary current is initially at a maximum. This gradual building up in the second-circuit under a continuous influeace from action in the primary circuit is the "cumulative resonance" effect to which I have referred and which Waterman has criticized, and it is well known that it is only by such effect that actual resonant transfer of energy from one circuit to another can be had. For example, I note that Waterman in his answers to cross-question 84, cross-question 86, and cross-question 87, pages 1972-73, was referred to oscillograms or photographs of electrical currents in circuits when coupled, which oscillograms were used by him in his testimony in the case of Radio Corporation of America *v.* Splitdorff Electrical Co., and particularly oscillogram CD120089, shown on printed page 229 of the Splitdorff record. In these oscillograms Waterman shows the currents in two cases of tightly coupled circuits, and it is seen from them that the currents in both circuits are badly distorted as shown by Morecroft for tightly coupled circuits on pages 238, 239, and 240 of his

book to which I have referred. These oscillograms of Waterman clearly show that there is no resonant building up of the oscillations in the secondary circuit in the manner [fol. 1765] shown in the case of the loosely coupled circuits, the currents of which are illustrated on page 249 of Morecroft. In fact, Waterman's photographic investigation of the transfer of energy in the Splitdorff case clearly sustains my contention that where the coupling is tight, as it is in a quenched gap system during the period of charging the antenna before quenching of the gap, there is no such thing present as resonant transfer of energy.

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In the last paragraph but one, page 1892, Waterman states that I am mistaken in my contention that the primary circuit in the Marconi system has much to do with determining the duration of the oscillations, but given no reasons why I am mistaken. The fact is that the patent itself provides that the oscillations be maintained for a long time in the primary circuit, and I have heretofore given many technical reasons that Waterman might have attempted to refute why the patent is correct.

In the first paragraph, page 1893, Waterman charges me with displaying "sheer ignorance" in my contention that in making the oscillations in the two circuits of his sketch 2 of equal intensity illustrates a coupling of the order of 100 per cent, which does not exist, evidently making this statement on his present contention that the intensities represent condenser charges instead of "oscillation of current" as originally stated by him on page 151 to which I have previously referred.

In the next paragraph, page 1893, Waterman states that I am "wrong again," followed by a statement:

"Mr. Loftin seems to have the erroneous impression that the current in a circuit can be going in two directions at once. Mr. Weagant has clearly pointed out Mr. Loftin's error in this regard, as found in Loftin's sketches G and H, defendant's Exhibit O-2 and P-2."

I note that in cross-question 76, page 1970, Waterman was asked to point out where in my testimony I had made any statement to the effect that current can be going in one circuit in two directions at once, and that he failed to do so. The fact is I made no such statement and in no way testi-

fied in a manner to convey this impression. In my sketches G and H, referred to by Waterman as having been criticized by Weagant, I did what is often done in the matter of explaining wave motions; that is, divide the one current of complex wave form into two illustrative components merely for the purpose of clearer analysis and explanation, a practice not unusual in ordinary textbooks involving wave motions of different kinds including electrical currents. For example, on pages 241 and 242 of the Morecroft book to which I have just previously referred he shows three figures in which the "actual current," shown in the wave form of full line, divided into two explanatory components shown in dotted lines in the several figures.

While Waterman was unable to point out where in my testimony I had stated that there were actually two currents flowing at once in any circuit, I note that in his testimony [fol. 1766] in the case of *Radio Corporation of America v. Splitdorf Electrical Co.* quoted to him in cross-question 83, pages 1971 and 1972, he made the statement: "There will be in those circuits as *many currents* as there are circuits" (italics mine), this testimony being in connection with the transfer of energy between coupled circuits. Of course Waterman did not mean in this Splitdorf testimony that there were actually "many currents" flowing in the same circuit at once, but one current having many frequency components, but it emphasizes how loose he may be in his expressions at one time and how unjustly critical he becomes at another time.

In the third paragraph, page 1893, Waterman states that "with a coupling of 8 to 10 per cent there are not necessarily two frequencies," which is not true. I showed on page 1037 of my testimony the mathematical formula for the two frequencies depending upon coupling, the degree of coupling being the term K in this formula. The only way to avoid the two frequencies is to make the value of K zero instead of 10 per cent mentioned by Waterman, and there is no way to couple and operate the circuits to avoid the law of nature expressed mathematically in the formula.

In the last paragraph, page 1893, Waterman states that he fails to see from the point of view of the Marconi invention that there is any consequence whether or not the coupling in the Marconi patent is somewhat tighter than required for best results; thus seeming to forget as always that the Marconi patent is a 4-circuit system and can not

be permitted to emit two frequencies because of the requirement that the circuits at the receiver be adjusted to have the same time periods as the circuits at the transmitter, a feature which was emphasized by Marconi himself in his 1912 address in which he pointed out the necessity for loose coupling.

In the second paragraph, page 1894, Waterman charges me with misquoting him as to the coupling effect in connection with his sketch No. 2, and quotes his testimony verbatim in connection with his charge that I have misquoted him. After going to all of this trouble he does not point out wherein my abbreviated statement of his testimony differs in any way to mislead, and I fail to see any difference.

In the last paragraph, page 1894, Waterman states:

"A qu*nc*ched spark gap transmitter, such as he frequently refers to also gives but one frequency and hence, by his own definition, is also 'loosely coupled.'"

I have not contended, nor do I know of anyone who has contended that the quenched gap type of transmitter emits but one frequency. In my sketch O, opposite page 1100, I showed in Figure 6b the type of emission by a quenched gap transmitter, which shows three frequencies, the two small ones in red and green being the coupling frequencies before the gap quenches and the large one in purple being the desired oscillation at the natural period of the antenna circuit after the gap quenches and the coupling relation between the circuit ceases to exist. This same triple frequency of the quenched gap system is shown in Seibt patent, Figure 6, defendant's Exhibit C-3, to which I have referred in my early testimony.

On page 1895 Waterman attributes to the Marconi system all sorts of characteristics only possible in the quenched gap system, and even more, for at the bottom of the page he states:

[fol. 1767] "An effective, that is to say, a spark gap working in the most desirable fashion, will but off by *or before* the time when all the energy has been transferred to the antenna."" (Italics mine.)

There is no possible way in the quenched gap system for the gap to cut off before all of the energy in the primary has been transferred to the antenna. In the quenched gap sys-

tem a most careful adjustment is necessary to have the current through the gap at the time of the first beat at substantially zero to get the gap quenched even at this point, which is the point of transfer of all the energy to the antenna, and there is no way to get a quenching in the gap before this point is reached. In the loose coupled Stone and Marconi systems the gap does quench before all of the energy is transferred to the antenna, this because no attempt is made to quickly transfer all of the energy to the antenna and thus arrive at an early first beat when there is still present a large amount of energy in the system. When the oscillations in the primary circuit are long drawn out there is not much energy left in either the antenna or the primary circuit when the gap opens up, so that it just naturally extinguishes from lack of energy anywhere to maintain it in operation.

Waterman follows the quotation above by another as follows:

"The antenna begins to radiate as soon as the spark starts and it continues not only till the spark ends but until long after."

This feature emphasizes one of the outstanding differences between the loosely coupled Stone and Marconi systems and the quenched gap system. In both systems the antenna begins to radiate immediately the spark starts, but in the quenched gap system the radiation that takes place before the gap quenches is useless for communication, and is in fact a defect, as it goes out in two widely separated coupling frequencies as shown in Figure 6-b of my sketch O opposite page 1100. In the Stone and Marconi systems the radiation from the start is at the desired frequency, and thus starts useful communication from the beginning, this as shown in Figure 5-b in my sketch II opposite page 1043.

37. Question. Please refer to the testimony in answer to question 6, page 1896 and following, and state whether or not you agree with the testimony, giving your reasons.

Answer. In my previous testimony I pointed out wherein Waterman in making his sketch No. 3 as an alleged simplification of Figure 2 of Marconi patent 763772 had taken unwarranted liberties in transposition of apparatus not provided for in the patent. Waterman now states in the testimony in answer to question 6 that it was a trivial matter be-

cause the condenser j^3 which he moved had a large capacity compared to the other condenser h'. I can find no basis in the patent for his present contention that the condenser j^3 had a large capacity compared to condenser h'. Waterman states that a value of 0.004 "is commonly used," by which I infer that he speaks of the present-day practice in connection with a condenser used in modern circuits, which of course gives him no basis for turning back some 25 years as to what was intended by Marconi, and certainly practical facts are against him. At Marconi's time it was not known how to make up large capacities in small compact forms and a condenser of the capacity now mentioned by Waterman would have been a cumbersome piece of apparatus if made in the fashion of constructing condensers at Marconi's date.

[fol. 1768] 38. Question. Please refer to the testimony in answer to question 7, page 1897 and following, and state whether or not you agree with the statements therein, giving your reasons.

Answer. I do not agree with the statements in many respects. This is Waterman's testimony in answer to my prior testimony concerning Seibt patent 1216615, defendant's Exhibit C-3, which I used to point out the differences between the quenched gap system and the loosely coupled system of Marconi and Stone.

On page 1897 Waterman begins his testimony with the statement that he is compelled to infer that I have never used the transmitters I discussed and "knows little about them." The fact is that the United States Navy has dealt more extensively in quenched gap transmitters than any other one institution, and I was closely associated with this Navy dealing in quenched gap transmitters for some 12 years in many practical ways, including supervising actual installations with calibration and adjustment for use. Waterman's replies to my practical analysis of the quenched gap system compels me to infer that his association with this form of apparatus has been merely that of the professional patent expert rather than as a practical engineer.

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At the top of page 1898 Waterman states that the local circuit of the transmitter of Seibt patent is a "good conserver or persistent oscillator," which is not the case, the reverse being true. The whole purpose of the Seibt patent

is to combine a tight coupling with a quenching gap in such a way as to get the oscillations out of the local circuit quickly and keep them out, and thus avoid any duty of conservation on the part of the local circuit and any possibility of persistent oscillation therein.

In the first paragraph, page 1898, Waterman enumerates the elements in the Seibt transmitter system in a way to point out the elements of the Marconi patent in suit. The same enumeration of elements is possible with respect to the Stone-Baker letters and Stone patent 714756.

In paragraph 2 Waterman states that Weagant has discussed the Seibt patent and its file wrapper, and has pointed out disclosures of Seibt to the effect that these two circuits are in resonance with one another, and Waterman states that he agrees with this testimony of Weagant.

In my testimony on page 1098 I pointed out how Seibt was careful in his patent, page 1, lines 62-70, to make clear that his reference to resonance referred merely to the adjustment of the natural periods of the circuit, and not to the resonant transfer of energy therebetween, and the figures of the drawings of the Seibt patent further emphasize lack of resonant transfer with the tight coupling in view, this being clearly shown in Figures 2 and 3 of the Seibt patent. Weagant takes up a communication between a patent attorney and the Patent Office during the prosecution of the application, page 1633, and tries to use the lack of discrimination of which a non-technical man is capable as evidence [fol. 1769] that the clear discrimination made by Seibt himself in his patent is not to be taken with its full force.

In paragraph 3 of page 1898 Waterman states that the only difference between Seibt and the Marconi patent is that Seibt employed the spark gap of Marconi reissue patent 11913 characterized by a succession of gaps in series. This is not the fact, as the gap of Seibt includes many features other than the mere placement of gaps in series, these being such features as air-tightness, large area, very short gap, and materials of rapid heat conductivity and non-disintegration. No one has succeeded in making a quenching gap out of a mere arrangement of open gaps in series that will function in the Seibt arrangement, that is with the circuits tightly coupled to produce an early first beat at which quenching could take place. The correct type of gap was not shown anywhere until the work of Wein and Arco & Rendhal some 10 years after the Marconi reissue pat-

ent. Marconi required a gap suitable for long drawn out oscillations in the primary circuit, namely an open gap working at rather high temperature, and Seibt a gap to quickly cut off oscillation in the primary circuit, namely, a properly designed quenching gap of rather low temperature.

In the last paragraph but one, page 1899, Waterman states that I ignore the fact that if the circuits did not react on one another they could not act on one another. I have done no such thing. On pages 1037 and 1038 I carefully went into the matter of how much in practice the circuits could act and react on one another without creating frequencies so widely separated as to interfere with tuning of the receiving circuits, and showed that with a coupling of the order of 3½ per cent and below good tuning could be followed throughout the system. A coupling of 3 per cent will resonantly transfer a substantial amount of energy, and the apparatus will be decidedly operative rather than inoperative as suggested by Waterman.

At the top of page 1900 Waterman points out that I have said that the adjusting of the timing periods in the two circuits of the Seibt patent has nothing to do with a resonant transfer of energy between the circuits, and comments that I have given no proof of this statement and that it is wholly incorrect. In my sketch O, opposite page 1100, I show in Figures 6 and 6a how the energy in a quenched-gap system during the transfer period before the quenching oscillates violently at two different frequencies, making a resonant transfer during this period impossible. The main criticism of Waterman of this showing is his foolish suggestion that I have erroneously illustrated two currents flowing in the same circuit at once. He does not take up the actual facts, namely, that the current that does flow has no frequency corresponding to the natural period of the circuits, and therefore is incapable of flowing in them in any manner satisfying the actions and reactions governing resonant flow.

In the first paragraph, page 1900, Waterman states that the figures of the Seibt patent indicate a coupling of about 10 per cent, while the range indicated by Marconi patent is from 5 to 20 per cent. There is no basis for Waterman asserting that the figures of the Seibt patent indicate any specific coupling for a properly designed quench-gap system. Waterman has evidently counted the number of oscillations illustrated by Seibt in Figure 2 up to the Point T_1.

[fol. 1770] and applied this count to the Morecroft formula to estimate coupling. In this figure Seibt is not dealing with the proper operation of a quenched-gap system, but merely illustrating his technical discussion to the effect that with a tightly coupled system the current flow in the circuits waxes and wanes alternately as the energy is transferred from one circuit to the other. The mere use of five oscillations in drawing a diagram of this kind between the state of maximum current and minimum current is not entitled to any significance as to the coupling proposed by Seibt. Seibt makes it clear that the longer the period of time before this point of minimum current at which the gap can quench the less perfect is the result, which in itself is an instruction to make the coupling as tight as possible. As before pointed out Waterman has no basis for saying that the range of couplings in the Marconi patent is from 5 to 20 per cent. It is clear that the two couplings in tunes 2 and 4 which he could not recollect are extremely loose, and well below 5 per cent, and that the Marconi patent does not furnish sufficient data around which to determine any degree of coupling.

Further on page 1900 Waterman objects to my pointing out that the system of Seibt is based on the fundamental principles set up in the Lodge patent, that of quickly transferring the energy from the local circuit to the antenna circuit without regard for resonance, and disconnecting the local circuit from the succeeding action; that is, to leave the antenna circuit free to determine all by itself the frequency of the oscillations with which actual communication could be carried on. In refutation of this Waterman again turns to his ridiculous proposition that Lodge provided two radiating circuits with but one antenna circuit with which to do so, and in answer to which proposition I have pointed to the structure in plaintiff's Exhibits 79, page 180; 91, page 181; and 101, page 190; as systems coupled to the antenna like Lodge's Figure 4, alleged by claimant's witnesses to infringe the Marconi patent in suit.

In the first paragraph, page 1901, Waterman says there is no basis in fact for my contention that the Seibt arrangement is based upon a number of features of Lodge's Figure 4 found in the Marconi patent in suit is also based upon Lodge's Figure 4. I have pointed to a number of features of Lodge's Figure 4 found in the Marconi patent in suit, particularly in pointing to Waterman's attributing to Mar-

coni the use of two circuits at the transmitting station, one open and one closed, and the removal of the spark gap from the open circuit. Lodge has these features. Marconi differs from Lodge in providing for a long drawing out of the oscillations in the closed circuit, and resonant transfer of them to the antenna circuit, which is also a difference between Marconi and Seibt.

In the second paragraph, page 1901, Waterman states that "the sole basis" for my contention that there is not a resonant transfer of energy in the Seibt arrangement is that the number of oscillations during the transfer period is relatively small compared to the number of oscillations from the antenna circuit, which is not the fact. I have pointed out in detail in connection with my sketch O, opposite page 1100, that during this short period of transfer the current flow involves two widely separated frequencies, and therefore can not resonate with the circuit. If during [fol. 1771] this period of transfer it were possible with tight coupling to keep the current from distorting itself into two frequencies, and to cause it to have one frequency, that of the circuits, I would say that in spite of the short duration of the transfer that it is resonant transfer, merely qualifying it as not being sharp or cumulative resonance because of the lack of time for resonant building up. In other words Waterman's reference to "the sole basis" entirely misrepresents my contention as to the operation that takes place.

In the latter half of the second paragraph, page 1901, Waterman outlines the operation of the quench gap system including features wholly inconsistent with fact.

He first says: "The spark gap by discharging sets up oscillations of the desired frequency in the nonradiating circuit." It just can't be done. The tight coupling between the nonradiating circuits and the antenna circuit throws its influence so strongly upon the current movement that an oscillation of two widely separated frequencies persists as long as the gap plays its part in the operation.

He next says: "Associated with this circuit is the open circuit which can, theoretically, be set into oscillation at any frequency, but because its electrical time period has been made the same as that of the spark gap circuit, it offers the lowest possible resistance to being set into oscillation at this frequency to which it is tuned." The only way the circuit could act in the way thus outlined by

Waterman is for it to be a free and independent circuit, and not being so because of a tight coupling to the non-radiating circuit Waterman is wrong. It indulges in a scramble form of oscillation at two widely separated frequencies until the transfer of energy period is over.

Waterman next states: "While it is thus absorbing energy it is also radiating, and it thus is able to radiate more energy * * *." It is true that during the energy transfer period the antenna circuit is radiating, but in no useful way. The radiations are at two widely separated frequencies, so that a distant receiver tuned to the desired frequency gets no benefit out of this radiation occurring during the energy transfer period, shown in detail in Figure 6b of my sketch O, opposite page 1100. On the other hand, the Stone and Marconi systems do usefully radiate throughout the entire transfer period, the coupling being sufficiently loose that the two frequencies do not spread apart any noticeable amount from the point of view of tuning at the receiving station.

At the top of page 1902, Waterman again refers to my analyzing the double frequency current of a tightly coupled energy transfer in my sketches as showing "nothing but Mr. Loftin's ignorance of electrical science," the remembrance of which statement must have embarrassed him considerably, if he is capable of embarrassment, when he was called upon to answer in cross-question 83, pages 1971 and 1972, his quoted testimony in Radio Corporation of America *v.* Splitdorf Electrical Company. At the top of page 1972 his quoted testimony makes the unqualified statement that there are "many currents," and in his answer he transposes this into having said that the current of complex form might be analyzed into the many currents referred to by Stone. In Waterman's conception of my ignorance I pictorially analyze the current just as he, in the last analysis, said he would do. In other words it took Waterman a long time to become as bright as I was in the first instance.

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[fol. 1772] In the first paragraph, page 1902, Waterman says I seem to suggest that the Seibt patent implies that with other forms of spark gap the operation will always be what I illustrated in my sketch G, and adds that he understands the Seibt patent to state quite the opposite. The

fact is that I give no basis for seeming to suggest what Waterman says. On page 1040 I clearly pointed out that Figures 2, 2*a*, and 2*b* illustrate the conditions encountered under tight coupling with a gap that will not quench, and the Seibt patent in referring to Figures 2 and 3 thereof deals with the same composition; that is, the figures do not deal with other forms of gaps merely as suggested by Waterman, but include in a most definite manner the effect of tight coupling on a nonquenching gap system.

Waterman then points out that Seibt discusses a type of gap in which the conductivity is maintained until the energy at the discharge has been entirely dissipated, pointing out that the retention of conductivity by a gap depends upon vaporization of the metal of the electrodes, small heat conductivity and increase of current, features I previously pointed out in connection with the Marconi patent as being helpful in securing long-drawn-out oscillations in the primary circuit specified by Marconi. He then points out that Seibt mentioned that these characteristics for retained conductivity of the gap are never "fully attained" in practice, which is quite true, but in no way warrants the meaning then given by Waterman to this comment, namely, that it would take a very bad spark gap condition, or a very insufficiently loose coupling to bring about the results shown in Seibt's Figure 2 and my sketch G. The fact is the features shown by Seibt and me are precisely the ones referred to by Marconi in his 1912 address when he stated that the open gap had to be used with loose coupling in order to avoid this repeated retransfer of energy. In other words, Marconi knew that the features of prolonged conductivity were sufficiently "fully attained" in his open gap to produce the results shown by Seibt and me.

On page 1903 Waterman discusses the Seibt patent as if it makes no difference at which one of the beats the quenching takes place, which, of course, is absurd. Seibt makes it clear that the whole purpose of his patent is to bring about a quenching at the first beat, and the reason for this is most clear. Seibt shows that he can produce a first beat after a very few oscillations in the primary circuit, for example, four or five, but if the gap fails to quench at this first beat, then the next beat occurs at double the number of oscillations later—that is, around the twelfth to fifteenth—and since the total number of oscillations can not be much greater than this higher number, it is obvious

that if the quenching waited until the second or third beat most of the duration of the entire operation would be taken up by a period of scrambled as to frequency energy transfer without resonant effect, and very little time would be given to final useful oscillation at the desired frequency in the antenna circuit. Every practical engineer knows that when a quench-gap system is not adjusted to quench at the first beat the system is not in fact a quench-gap system. [fol. 1773] On this bogus theory that quenching can take place at any one of the beats Waterman then states that it has been the endeavor from the outset—that is, from the first introduction of the apparatus of the Marconi patent—to provide better and better cooling for the spark gap. The fact is that the matter lay dormant for many years until the disclosure of Wein as to the possibility of the quenched gap effect being used came as a sudden enlightenment of the art, and Wein was so clear in his disclosure that Seibt and the engineers of the Telefunken Co. of Germany almost immediately put the ideas into commercial practice.

At the bottom of page 1903 Waterman states that I "seems to intimate" that Seibt dealt with the operation of an apparatus containing the quench gap in my quotation on page 1099 from the Seibt patent, page 2, lines 74-87, but does not state wherein this seeming to intimate arises. I did not intimate any such thing. Immediately following the quotation I stated on page 1100 that Seibt illustrated in Figure 4 of his patent the quenching of the spark gap at the first beat, thus making it clear that I considered that only Figure 4 of Seibt properly illustrates the quench-gap system. Waterman states that Seibt was talking about the characteristics exhibited by "prior circuits," and that Seibt says that they may act in any of the ways shown in his Figures 2 to 5, inclusive. What Seibt did show was that if anyone attempted to operate the prior circuits with a coupling as tight as that needed for a quench-gap system there would result pandemonium in the oscillation until a quench gap opened up the primary circuit at the very first beat.

At the bottom of page 1904 Waterman limits the Marconi patent to merely calling for an association in a transmitter of a radiating and nonradiating circuit whose electrical time periods are equal, a limitation entirely unfair to Mar-

coni and one which Marconi himself refused to accept in his 1912 address because he knew that his system was a 4-circuit system that would not permit ignoring the tuning of the circuits at the receiving station, so that he specifically made reference to loose coupling as a compulsory feature of his system. However, the patent itself was in effect clear on this point of loose coupling in prescribing that the oscillations in the primary circuit should be maintained for a long time. By ignoring this maintaining of the oscillations for a long time in the primary circuit Waterman states that the instructions of the Marconi patent are "faithfully embodied" in the quenched gap transmitter employed by the Navy. The fact is that the quench-gap system deliberately departs from the Marconi instructions and secures a different mode of operation by so doing.

In the second paragraph of page 1906 Waterman quotes me as making the unqualified statement on page 1117 that Figure 13 of the Lodge patent is a tunable circuit, and follows his unfair method of quoting with the statement that the quotation is decidedly misleading. My testimony at this point was as follows:

"Following such line of argument the secondary circuit of the Lodge receiver, Figure 13, is a tunable circuit if this is what Mr. Waterman means to imply."

This is far from an unqualified statement on my part that the secondary circuit of a receiver of Lodge's Figure [fol. 1774] 13 is a tunable circuit, but a statement placing the burden directly on Waterman. In my testimony immediately preceding I pointed out that Waterman had contended that receivers of defendant not including means for keeping the tuning of the secondary circuits the same as the tuning of the antenna circuit infringe the Marconi patent, and therefore if Waterman's line of argument was acceptable Figure 13 of the Lodge patent was entitled to a like position.

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39. Question. Please refer to the testimony in answer to question 8, page 1907, and state whether you agree with the statements therein giving your reasons.

Answer. In the answer to this question Waterman takes up my prior testimony that the quench-gap transmitters of defendant are based on the fundamental principle of im-

pulse excitation set forth in Lodge's 1898 patent, stating that he does not agree. He gives as his reason that Lodge does not have in his transmitter a local circuit of the non-radiating type and does not have such a circuit associated with an open radiating type, but does not attempt to state that these two circuits are necessarily a part of impulse excitation. The fact is that Lodge does have these two circuits as brought out in my sketch opposite page 1069. He further says that Lodge does not have two circuits whose electrical time periods are equal to one another, again failing to state why it is necessary to have the two circuits so adjusted in order to have impulse excitation, and the fact is that it is not necessary. The Simpson and Thompson transmitters in the Kilbourne and Clark case did not have the two circuits so adjusted, and it is my understanding of the decisions in that case that these transmitters were held to have impulse excitation of the Lodge patent. Waterman further says that the quench-gap transmitters of defendant do not have Lodge's multiplicity of spark gaps. They in fact have a multiplication of the multiplicity of Lodge's gaps, as in general the quench-gap elements in defendant's transmitters employed from 10 to 15 gaps in series, whereas Lodge showed as many as 4, providing for cutting one of these out if desired, the one in the antenna circuit. Waterman also says that defendant's quench-gap types do not have the multiplicity of radiating circuits alleged for Lodge by Waterman in spite of his single-antenna circuit. Just what Waterman means by this multiplicity of radiating circuits in Lodge to the exclusion of other systems has never been clear to me, but if it has anything to do with the ability of Lodge to radiate two frequencies during the period of time before the opening of the gaps, then precisely the same feature is found in defendant's quench-gap systems. They all do it, and Seibt drew a picture of it in Figure 6 of his patent.

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[fol. 1775] 40. Question. Please refer to the testimony in answer to question 9, pages 1907 and following, and state whether you agree with the statements therein, giving your reasons.

Answer. I do not agree with many of the statements in this part of Waterman's testimony which has to do with the matter of resonance, including the question of whether or

not there is a resonant transfer of energy between the primary circuit and the antenna circuit in a quench-gap system during the period before the quenching of the gap. On page 1908 Waterman defines the word "resonance," limiting the definition to application to the Marconi patent, and as used in the art, as "equality of electrical time period," which is not enough. It is true that equality of electrical time period is an essential feature of electrical resonance, but it is also true that resonance can be prevented even where the equality exists. Resonance has to do with response, so that if there is not a definite frequency of electrical activity to which an electrical element can respond there is certainly no possibility of a resonant response taking place, and certainly no electrical circuit or other responding system can at the same time resonantly respond to two different frequencies. The simple physical fact that two coupled electrical circuits produce more than one frequency of noticeable separation with tight coupling makes it impossible to talk in terms of resonance under such conditions, and this was the very purpose of the oscillogram introduced by Waterman in his Splitdorf case identified by him in his answer to cross-question 84, page 1972, this case.

On page 1908 Waterman refers to a quotation from the Navy Manual of 1913, plaintiff's Exhibit No. 78, page 57, and states that this quotation immediately follows the discussion of the quench-gap transmitter in the manual, evidently with the purpose of leaving the impression that the quotation refers to a quench-gap system, but there is no basis for this. The manual takes up on page 55 the discussion of the quench gap under a special heading to this effect, and completes this discussion on page 57 before taking up the matter quoted by Waterman, and gives the new matter a special heading entitled "Methods of Producing Electric Waves." In other words, the treatment had finished with the quench gap and proceeded under another subject. The Figure 29a that is specifically mentioned in the quotation which begins this new subject is shown on page 52, and is an open-gap system under the title of "Slaby Arco," a trade name for wireless systems existing prior to the quench-gap system. The "Arco" part of this name comes from Count Arco, who later associated himself with the Telefunken Co. and developed with Rendhal there the quench-gap system.

On page 1909 Waterman quotes from page 43 of the

same Navy Manual a statement of the writer to the effect that "two circuits" having the same electrical length are said to oscillate in resonance, as being identical with that of the Marconi patent. It is to be noted that the writer specifically limits his statement to "two circuits," and not to a system of coupled circuits. Marconi showed coupled circuits, and for this reason qualified his statement to the effect that the oscillations in the exciting circuit should be maintained for a long time, apparently appreciating that [fol. 1776] with the spark-gap method of producing oscillations he could not maintain them for a long time by tight coupling.

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On page 1908, bottom, Waterman refers to a quotation in his prior testimony from page 162 of the Navy Manual of 1913, plaintiff's Exhibit No. 78, and says that I avoid his quotation of a part of paragraph 224 by saying that it is a very general statement, whereas he points out the matter following the statement makes specific reference to closed and open circuits. It is true that the subject matter following does make reference to what is to be done in using the principles of the general statement when dealing with systems having closed and open circuits, but this is no basis for contending that the general principles can only be applied to such systems. I have not the 1913 edition of the Navy Manual before me, but note that in the 1915 edition, page 163, there is a paragraph 224 corresponding to the 224 paragraph in the 1913 edition. This paragraph 224 of the 1915 edition concludes with the following statement: "After calibration the adjustment of these circuits to the same wave length and to the desired coupling is called tuning."

Waterman has contended throughout that the tuning in the Marconi patent does not involve the matter of coupling, but is simply the adjustment of the lateral circuit and the antenna circuit to have the same time period. This quotation from the Navy Manual makes it obligatory to include the matter of coupling in the adjustment in order for the adjustment to be "called tuning." I note that the 1915 edition of the Navy Manual to which I have referred is claimant's Exhibit No. 78.

On page 1910, (first paragraph, Waterman attempts to draw an inference from my not referring to this quotation

from page 163 of plaintiff's Exhibit No. 78, Navy Manual of 1913. Waterman points out that this portion of the manual is a résumé of matters taken up by the manual prior thereto. Since Waterman had taken up the matters of which this portion was a résumé, and I had answered these, for the sake of brevity in the record I did not deem it necessary to repeat what had gone before as to the résumé. It is noted that the résumé does not differ from the earlier references to the relation between the closed circuit and a radiating circuit, calling for "resonance" between these circuits. The preceding subject matter makes it clear that the tuning for resonance involves not only the adjustment of the time period of the circuits but the coupling between them as well.

At the bottom of page 1910 Waterman takes exception to my testimony that the use of the word "resonance" in the Signal Corps book in referring to the adjustment of the time periods of the closed circuit and the antenna circuit of a quenched-gap system is a loose and incorrect use of the term. In my testimony, page 1515, I pointed out in detail why there could be no resonant transfer of energy during the charging period of the antenna. The 1915 Navy Manual to which I have just referred makes it clear that the matter of coupling is also to be included in an adjustment [fol. 1777] before the adjustment can be called tuning, and it is only an adjustment that includes loose coupling that permits of tuning for resonance.

At the top of page 1911, Waterman states that Figure 56 of the Signal Corps book shows three oscillations in the primary circuit of the quenched gap transmitter the book refers to, and applying Morecroft's rule, this means a coupling of about 16 per cent and one which he alleges is within the range of the examples in the Marconi patent. I have shown that a coupling of 16 per cent will produce two widely separated frequencies which would not permit of tuning receiving circuits to have the same time periods as the transmitting circuit, and Marconi in the 1912 address pointed out that this double frequency condition would not do in his system.

Waterman then refers to a former quotation of his from the Signal Corps Manual which refers to the adjustment of the two circuits of a quench gap system with the term "resonance," and again infers that I overlooked this quotation for a purpose. This quotation was merely a repeti-

tion of like ones pointed out by Waterman in both the Navy Manual and the Signal Corps Manual, and I did not feel called upon to continue the repetition and my comments as to the use of this term under conditions where resonant transfer of energy could not take place.

In the middle of page 1912, Waterman makes a comparison of the amount of energy that could be put into the Marconi system of patent 763772 over the system of his earlier reissue patent. His object in this is not understood because he could have made the same comparison between for the Tesla system, the Stone system and the Lodge system, all of which removed the spark gap to a lateral circuit from the antenna circuit, it being this feature around which Waterman makes the comparison.

Waterman has laid considerable stress on the Navy Manual as to this matter of adjusting the circuits of a quench gap transmitter. One of claimant's witnesses, George H. Clark, is included in the preface of these manuals as a contributor to the compilation of the manual. In question 13, on page 1490, Clark was asked for a statement by him of the method he used in tuning quench gap transmitters, and in answer thereto he set forth the steps in detail. He states that he first separates the secondary coupling coil some distance away from the primary coil; or in other words, starts with very loose coupling. As the next step he states that he adjusts the primary circuit to the wave length desired using a wave meter for the purpose, which is an instrument for determining the time period of a circuit. As the next step he states that he adjusts the antenna circuit to resonance with the primary, it being noted that he does this without changing the loose coupling with which he starts.

If he was adjusting the Marconi system this would be all that he would have to do, as these three steps have provided for making the time periods of the two circuits the same and he would have the loose coupling necessary to prevent the production of two frequencies. In fact, if he did not have the loose coupling he would not be able to know when the antenna circuit is in resonance with the primary by using the indication of maximum current in the antenna ammeter as his guide, as stated by him.

[fol. 1778] He then goes on with additional steps, the first being to move the secondary coil closer to the primary coil to increase the coupling, stating that he continues to do

this until he obtains the maximum antenna radiation consistent with a small decrement of the emitted wave. His looking for this small decrement of the emitted wave means that he sought the point at which quenching took place at an early first beat, for unless this quenching takes places there will be a complex wave form with a large decrement.

He then states that he measured the wave length sent out by the transmitter using a wave meter coupled with the antenna circuit alone, and if not finding the emitted wave length to agree with what he desired he carried out the procedure all over again, the primary wave length being changed to a new value which experience led him to believe would result in the desired wave length, and he continued this repetition several times if necessary.

These additional features of Clark make it clear that the adjustment of the quenched gap system is not a mere matter of adjusting the time periods of the two circuits to be the same. Waterman and Weagant have both testified that because of the spark-gap in the primary circuit of the Marconi system it is not necessary to have the adjustment of the time periods of the two circuits to be precisely the same, and in fact that a discrepancy of 1 or 2 per cent makes no noticeable difference. The wave meter mentioned by Clark as his guide for the adjustment is well capable of measuring practical circuits with an accuracy within 1 per cent, so that if the quench-gap system to which his testimony relates was nothing more than the Marconi system as contended by Waterman, it would be quite sufficient for Clark to measure the wave length of each of the circuits with his wave meter and call his job complete. However, with a quench-gap system it can't be done.

Clark points out that after having adjusted both circuits to have the right time period when loosely coupled he invariably finds on tightening the coupling that the wave length emitted is not the desired one. That he has to start all over again by making a modification in the primary circuit, which of course means detuning, and try all over again by an empirical method of step-by-step trys as taught him by experience. In other words, Clark departs from his wave meter at this point and gropes for that nicety of adjustment obtained by a detuning of the circuits to get that perfect beat so necessary to certain quenching at the first beat of a tightly coupled quench-gap system. Clark, like many other practical handlers of quench-gap systems, prob-

ably never appreciated the reason why the quench-gap system calls for all of this additional effort and therefore could not write it in the book to which Waterman refers.

That Clark did not really understand just what he was doing and why he did it is clear from the awkward procedure he used outlined by him. If he had really understood the fundamentals of the system he would never have commenced by tuning the closed circuit first and thus working from this end. In the quench-gap system the antenna circuit sets the frequency of the desired oscillations after quenching takes place, and when he does this it is entirely free from any coupling to the primary circuit by reason of the opening of the gap. With this knowledge it is obvious that if the start is made with the antenna circuit [fol. 1779] carefully adjusted by a wave meter to have the desired natural period it would not have to be touched again and Clark could then go to the primary circuit starting immediately with a slight detuning of it from the experience he mentions, and thus greatly shorten the adjustment procedure he has outlined.

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[fol. 1779] 41. Question. Please refer to the testimony in answer to question 11, page 1913, and following and state if you agree therewith giving your reasons.

Answer. The question refers Waterman to my previous testimony concerning Judge Veeder's opinion in the case of the National Electric Signalling Co., in which I quoted a part of Judge Veeder's opinion to the effect that the defendant's apparatus in that case had loose coupling stating: "The loose coupling emits only one wave and admits of electric resonance." Waterman states on page 1914 that the quotation from my testimony indicates that I was again merely hiding behind my unwarranted restriction on the meaning of the word "resonance," which I assume to refer to my contention that resonance in the matter of energy transfer between two circuits can only take place when the coupling is sufficiently loose to in effect permit only one wave to be emitted, and it seems to me that this is precisely what is stressed in the language of Judge Veeder's opinion. At least it is not an "unwarranted restriction" on the meaning of the word resonance, but an essential to giving the word any meaning whatsoever.

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Cross-examination.

By Mr. Vaill for claimant:

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[fol. 1780] 42. Cross-question. In answer to question 3 on direct examination you criticized Mr. Weagant's use of tubes in the demonstration which he made regarding oscillations and stated that he "departed from the usual tube construction of a globular glass container of proportionally large diameter." You are familiar, are you not, with various forms of vacuum tubes produced for radio purposes which have been put on the market during the last 8 or 10 years?

Answer. Fairly familiar.

43. Cross-question. I show you four tubes which have been obtained by plaintiff as illustrating various forms of tubes which have been on the market for some years. These are respectively indicated or known as the "audiotron," the "diode" of the Electrad Co., a tube having a series of five triangular plates having holes through them and through which the filament passes and known as the "Schickerling" tube, and a tube known as "radiotron UX-199," all of which have cylindrical glass bulbs. You recognize these, do you not, as being tubes which have been for sale on the market for some time during the past 8 or 10 years?

Answer. I know that the UX-199 has been fairly extensively on the market since 1923, the one marked "audiotron" I have seen several samples, the particular construction of the one marked "diode" I do not recall having encountered, and the one referred to by you as the "Schickerling" I do not recall having encountered.

Mr. Vaill. The tubes and base on which the "diode" tube is mounted are offered in evidence, respectively, and it is asked that they be marked as follows by the notary: The "auditron" tube, claimant's Cross Exhibit No. 352; the "diode" tube, with the base on which it is mounted, as claimant's Cross Exhibit No. 353; the "Schickerling" tube having the triangular plates, as claimant's Cross Exhibit No. 354; and the "radiotron UX-199" tube as claimant's Cross Exhibit No. 355.

44. Cross-question. In answer to questions 14 and 18 you referred to Marconi patent 627650 and stated your

understanding of the operation of the apparatus shown in that patent. Will you please state whether or not you have ever constructed a receiving arrangement or apparatus made strictly in accordance with the directions set forth in said patent and have tested the same in actual reception of signals?

Answer. I have not constructed the precise arrangement of patent 627650 in all of its details, but I have operated its equivalent.

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45. Cross-question. In answer to question 31 you stated: "Stone wanted only the simple harmonic fundamental in the antenna circuit, * * *" and on page 5 of his alleged letter of July, 1899 (defendant's Exhibit G-3) Stone stated: "The current in the vertical wire in my system may there- [fol. 1781] fore be presented by a simple harmonic $A_2 \sin_2 pt$." This formula represent a pure sine or harmonic wave or oscillation, does it not?

Answer. Yes.

46. Cross-question. On page 9 of the same alleged Stone letter of July, 1899, is a diagram designated as "Figure 2," which apparently indicates a spark gap S and a condenser K in the primary circuit of what is termed "the transmitting circuits" immediately above the diagram. According to your understanding would the arrangement of this Figure 2, when transmitting, produce the usual damped oscillations in the primary circuit containing the spark gap S and the condenser K?

Answer. Yes.

47. Cross-question. Do you mean to contend in your answers on direct examination that such damped oscillations produced in the primary circuit of Figure 2 referred to in the previous question would produce undamped oscillations in the secondary or antenna circuit of said Figure 2?

Answer. No.

48. Cross-question. Are you familiar with the mathematical expression which represents the current flow when a condenser discharges through a spark gap in a circuit containing inductance?

Answer. To what characteristic of current flow does your question refer?

49. Cross-question. My question refers to the same characteristics as indicated by the formula given on page 5 of

the alleged Stone letter of July, 1899, above referred to, except as modified by the action of the spark gap.

Answer. I do not think your question is yet in form that will permit of a definite answer. The expression in the Stone letter to which you refer involves several characteristics of current flow. If you have reference to any particular characteristic and will state it, it may be that I will be enabled to answer.

50. Cross-question. Do you not consider that the following mathematical expression: $Ie^{-at} \sin pt$, is a mathematical expression for a damped electrical oscillation or oscillations in which "e" represents the base of the Naperian system of logarithms, "a" represents the damping factor, and "t" the time as it did in the formula which you have referred to in the alleged Stone letter?

Answer. It is a correct formula to show the first term in the Stone letter in a way to include the damping involved in the fundamental or harmonic frequency of a damped oscillation.

By Mr. Vaill. The title page and pages 2 and 3 of a book by J. A. Fleming entitled "The Principles of Electric Wave Telegraphy and Telephony," 1919 edition, in the form of photostat copies of said pages, are introduced in evidence as one exhibit and it is requested that the same be marked as claimant Cross Exhibit 356 (the formula included in the last question being found on page 3 of the said exhibit).

51. Cross-question. Do you consider that Stone has illustrated or described in his alleged letters of June and July, 1899, (defendant's Exhibits F-3 and G-3) any means by which he could produce a pure sine wave in any of the circuits illustrated or described by him in the letters referred to in the transmitting antenna?

[fol. 1782] Answer. If you use the term "pure" in the absolute my answer is no, and I think my testimony has been clear to the effect that absolute purity can not be expected.

52. Cross-question. In answer to Question 34, you stated that Mr. Waterman assumed that the spark gap would cut off the oscillations at 10 per cent, and as being an assumption he was not entitled to make. According to your experience, at what per cent would the spark gap cut off under the conditions which you had in mind in making that statement?

Answer. The conditions I had in mind in making the statement of course included a well designed open gap operating with as low resistance throughout the entire wave train as practical construction would permit, this low resistance operation being an obviously desirable factor to any constructor. I am unable to state any definite percentage of the total energy at which the gap will extinguish, but in view of the difficulty in extinguishing quenching gaps with almost a perfect beat condition or no energy through the gap, I have no hesitation in saying that the open gap will not cease its operation until below 10 per cent if the gap is a proper one.

53. Cross-question. Have you ever personally made any tests or experiments to determine at what percentage a spark gap, whether an open gap or quenched gap, would cut off?

Answer. Yes; in adjusting the quenched gap I have found that the adjustment must be such that the energy is substantially zero in order to get a cut off; that is, in the neighborhood — 1 per cent and fractions thereof.

54. Cross-question. I believe you stated on your direct examination that you appreciated that Mr. Waterman finds that the number of oscillations in the primary circuit were when the primary circuit was not connected with the antenna, but you have apparently referred to a complete quenched gap transmitting system in your last answer. I therefore ask you whether, under the conditions stated by Mr. Waterman, and which you considered and criticized in your answer on direct examination, you have ever made any tests to determine the cut off percentage in an oscillating circuit containing a spark gap of the kind referred to by Mr. Waterman.

Answer. Your question indicates, in referring to my testimony, that you have not grasped the purport of my criticism of the numerical values given by Mr. Waterman as the number of oscillations in the circuits of the tunes of the Marconi patent. I think if you will study my testimony you will find that I objected to Waterman using in the formula merely the constants in the primary circuit to arrive at the oscillations that would occur in the primary circuit when the primary circuit was coupled to another circuit, which coupling alters the constants affecting the oscillations in the primary circuit. As to measuring the

precise point of cut-off of an open spark gap, I do not know of any way of carrying out such an experiment with precision, and have not attempted to do it myself.

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[fol. 1783] 55. Cross-question. In answer to question 34 on direct examination you commented upon the number of oscillations which Mr. Waterman had found in connection with the four "tunes" of the Marconi patent 763772. I will ask you if you have ever constructed and operated the tunes according to the apparatus exactly as described by Marconi in his patent?

Answer. You ask if I have constructed the apparatus "exactly" as described by Marconi. I do not find in the Marconi patent specifications that will permit of any construction that is entitled to the qualification "exact," and accordingly I have never made any construction that satisfies the terms of your question.

* * * * * * *

56. Cross-question. In view of your criticism of the details of the construction of the apparatus of the Marconi patent 763772, both in your last answer and in your answer to question 34 of your direct examination regarding the question of coupling, I invite your attention to page 3 of Marconi patent No. 763772, lines 26–35, where he gives detail directions as to the construction of the transformer shown in Figures 3 and 4 of that patent. He says: The transformer is made on a block or core or insulating material which is 0.17 meters wide, and that the primary coil *d* is 0.946 meters in length, the core being of one turn. Dividing the latter number by 4 would indicate that the block is approximately 0.236 meters square. In other words, the block would be 17 centimeters wide and 23.6 centimeters square. The description also states that the insulation of both primary and secondary consists of 1.25 millimeters of rubber and 1 millimeter of jute, making the total thickness of the insulation 2.25 millimeters. With these facts in mind and with the spacing indicated in Figure 4 as related to the surface of the block, when taken in connection with the detailed description of the coil *d* in the transmitting circuit, is it not possible to tell very closely the amount of coupling between the primary and secondary or antenna circuit of the apparatus as illustrated in Figure 1?

Answer. I can't understand how you arrive at the information given in your question from the information given in the patent at the part to which you have referred.

* * * * * * *

57. Cross-question. In answer to question 34 you stated that——

"When the lateral circuit is coupled to the antenna circuit, the inductance factor in the formula is increased by the amount of the mutual inductance between the circuits, so that had Waterman assumed the correct operating condition he would have had factors go into the formula to give a smailler definite or a still greater number of oscillations."

Can you produce or refer to a formula which justifies your statement that the inductance factor in the formula is increased by the mutual inductance?

Answer. Yes. One of Newton's laws of motion, the second, as I recall it, clearly provides for this. The law simply says that for every action there must be equal and [fol.1784] opposite reaction. The current flowing in the primary circuit encounters not only the reactions of the elements in the primary circuit itself, but the reactions of associated elements through the coupling responsible for the association. In the coupling of two circuits through inductance elements there is a mutual inductance involved that must be taken into consideration to satisfy Newton's law.

* * * * * * *

58. Cross-question. Is your last answer the best you can do as an answer to my previous question?

Answer. I do not find in your question any incentive for finding a better authority than Newton. So far as my experience goes his laws have not been questioned.

* * * * * * *

59. Cross-question. In order to save you the trouble of having any greater incentive to answer my question I show you a formula on page 100 of a book entitled "Wireless Telegraphy and Telephony," by W. H. Eccles, which I believe has been referred to by you in your previous testimony as instanced on page 1130, although possibly of a different edition, and ask you if you do not find on page

100 the formula under the heading "With load on secondary" which is as follows:

$$X'_1 = X_1 - \frac{M^2 w^2}{Z_2^2} X_2$$

in which the letter M is stated in the book to represent the mutual inductance between the primary and secondary and letter X'_1 represents the total primary reactance. This equation indicates, does it not, that the total inductance factor or reactance of the primary circuit is reduced by the presence of mutual inductance rather than being increased?

Answer. Your question seems to imply that you have the impression that what you refer to as "the total inductance factor" and what you refer to as "reactance" are one and the same thing, which is not necessarily true. Reactance may be either inductive of capacitive, depending upon whether the inductive reactions in the circuit predominate, or the capacitive reactions predominate when an alternating current flows in a circuit at a frequency out of step with the natural period of the circuit. You therefore can not say that a generic reference to the term "reactance" means an inductive reaction. The equation to which you refer me refers generically to reactance and does not discriminate as between inductive reactance and capacitive reactance. I refer you to an equation on page 82 of Morecroft's "Principles of Radio Communication," 1921 edition, to which I have previously referred, equation 59, which distinguishes between capacitive and inductive reactions in a circuit. You will note in this equation that the reaction of the current I_1 through the mutual inductance M is added to the reaction of the current I_1 through the inductance L_1.

* * * * * * *

(Photostat copy of page 100 of the 1915 edition of "Wireless Telegraphy and Telephony" by W. H. Eccles, which by comparison appears to be the same as that of the 1918 edition from which defendant's Exhibit E–5 was taken, is offered in evidence as claimant's Cross Exhibit No. 357.)

[fol. 1785] 60. Cross-question. In your last answer you referred to equation "59" on page 82 of Morecroft's book. I note that this equation applies, according to the text, to

the diagram of Figure 72 on page 79 of the same book in which the primary and secondary circuits are apparently directly coupled through the same coil M which represents the inductance of the coupling coil. Inasmuch as we are referring to inductive coupled circuits such as you referred to in your testimony quoted in my cross-question 57 and you referred to the formula given on page 1037 of your first deposition, I will ask you if Morecroft's formula on the same page to which you referred, to wit:

$$wL_3I_1 - \frac{I_1}{wC_1} - wMI_2 = O \qquad (61)$$

does not represent the mathematical expression for inductively coupled circuits such as shown in Figure 73 on page 79 of the same book, and the letter M represents the mutual inductance between the two coupled coils L_b and L_c?

Answer. Mr. Waterman and I have fully agreed in this case that there is no difference between the inductive or indirect type of coupling and the direct type of coupling, the former being that of Figure 73 and the latter being that of Figure 72. The Morecroft equation 59 on page 82 is correct for either form of coupling and that it is so is shown by Morecroft in equations 63 and 65 just below the 61 to which your question refers me. Morecroft apparently made equation 61 incomplete, and says that equation 61 becomes equations 63 and 65 by giving his terms values to indicate inductance in the circuit and the mutual inductance in the circuit. In equation 63 as well as equation 65 it is seen that the reaction of the current I_1 flowing through the mutual inductance M is added to the reaction of the current I_1 flowing through the inductance L_1.

Mr. Vaill. Photostat copies of pages 79 and 82 of Morecroft's book entitled "Principles of Radio Communication," 1921 edition, are offered in evidence and it is requested that the same be marked "Claimant's Cross Exhibit No. 358."

61. Cross-question. Are you prepared to deny that the mutual inductance between two inductively coupled circuits to which you are apparently referring in the quotation from your testimony made as part of cross-question 57 decreases the inductance factor or decreases the total inductance of the lateral circuit as referred to in said quotation?

Answer. The matter of denying or affirming the effect as one of personal investigation I am not prepared to do. I have accepted it as a matter of course that the formulae evolved to cover the actions and reactions of this kind, based on Newton's laws of motion, have been sufficiently developed by others over a long period of time to be acceptable to me.

[fol. 1786] 62. Cross-question. You are willing to agree with me, are you not, that if in the quotation from your testimony which I made as a part of cross-question 57, the word "decreased" was used in the place of "increased," the consequence of this would be that the factors which go into the formula would give a smaller number of oscillations instead of a larger number?

Answer. Yes, but not any greater amount of change in the result, as the coupling is loose and the degree of change depends upon the degree of coupling.

63. Cross-question. In further answer to question 34 on direct examination you stated that you had calculated the effect of coupling on producing double frequency, using the formula on page 1037 of your previous deposition (defendant's Exhibit X-2). I invite your attention to that formula and ask you if you appreciated at the time you made your calculation that section 59 on page 88 of Zenneck's book from which the formula was taken related to "primary circuit without spark gaps," whereas in the tunes referred to by Mr. Waterman the spark gap was included.

Answer. I note that Zenneck also states that the relations of the formula also apply to a primary circuit with spark gap. I believe I took into consideration what Zenneck had to say about the formula.

64. Cross-question. In answer to question 37 and other places in your direct examination you referred to your understanding of the operation of the apparatus illustrated in Figure 4 of Lodge patent No. 609154. Have you ever constructed and operated apparatus made strictly in accordance with the description and drawing of Figure 4 of said Lodge patent?

Answer. No.

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Mr. Edward H. Loftin, being recalled as a witness on behalf of the defendant, testifies in answer to interrogatories by Mr. Edwards, as follows:

Direct examination.

By Mr. Edwards:

1. Question. I show you a copy of defendant's Exhibit B-7, being a copy of an agreement dated November 26, 1918, between the Marconi Wireless Telegraph Co. of America and the United States. Please say whether or not this agreement or a copy thereof was before or known to the radio patents board of which you were chairman at any time prior to the rendition of the report of that board in or about 1921.

Answer. To the best of my present recollection it was not.

Cross-examination.

By Mr. Vaill:

2. Cross-question. Did the interdepartmental radio board make its investigation and awards without any search or [fol. 1787] study as to whether or not the United States had any rights from the owners of patents which might cover any of the apparatus on which the board's awards were based?

Answer. Efforts were made to find rights flowing to the Government as offsets against claims made. For example, particular attention was given to following up purchase contacts as to source in order that a claimant should not be given credit for apparatus furnished by the claimant.

3. Cross-question. Did you know prior to the time the interdepartmental radio board made its award to the claimant in this case that the defendant had purchased the stations referred to in the agreement of November 26, 1918, upon which the direct question is based?

Answer. The board merely made a recommendation for an award to this claimant. I seem to recall that I did know as a matter of general information in the Bureau of Engineering that the Navy had taken over the ship and coastal radio stations of the claimant company, though it is a fact that the transaction was completed some months prior to my taking up my duties in the Bureau of Engineering.

4. Cross-question. Prior to the interdepartmental radio board's award to the claimant, do you know whether or not Captain Hooper had knowledge of the agreement of November 26, 1918, between the United States and the Marconi Wireless Telegraph Co. of America covering the sale of the latter's stations to the United States?

Answer. As a matter of definite recollection or personal knowledge of the matter set forth in the question, the answer is "No." However, it is a type of matter that would ordinarily be intimately handled by the officer holding the office held by Captain Hooper at the time.

5. Cross-question. Were you present during the hearings held in December, 1918, before the congressional Committee on the Merchant Marine and Fisheries of the House of Representatives with reference to the bill to regulate radio communication known as H. R. 13159?

Answer. No.

6. Cross-question. After the hearings took place as mentioned in the previous questions or at the time of the same, did you discuss with Commander Hooper, now Captain Hooper, the subject matter of the hearings and his testimony given before the committee?

Answer. I have no recollection of having discussed these specific matters including in the question, though I was first assistant to Commander Hooper and naturally much that occupied him did reach me as matters came up. However, I did not commence my duties until some months after the hearings referred to, and it is quite likely there was no occasion for taking up the matters of the hearings with me.

7. Cross-question. In your last answer, what do you mean by "my duties"?

Answer. My duties in the Bureau of Engineering (Radio Division), Navy Department.

8. Cross-question. Were you not ordered to the Bureau of Engineering, Navy Department, immediately after the signing of the armistice in November, 1918?

[fol. 1788] Answer. I was, but was in France at the time, so that following a leave of absence it was not until the latter part of January that I assumed my duties in the Bureau of Engineering.

9. Cross-question. About what date did you return from France?

Answer. As I now recall it, shortly before Christmas, 1918.

10. Cross-question. Have you ever examined the printed copy of the report of the hearings before the Committee on Merchant Marine and Fisheries in the House of Representatives, previously referred to in my questions, and in order to have you answer specifically, I now hand you a printed copy of the report of the hearings of that committee.

Answer. I do not recall having done so, though, of course, I have been required to follow up many documents of this kind, so that I can not be positive. I see nothing in the contents that attracts my attention as having been something specifically followed up.

11. Cross-question. I will ask you to refer to pages 18 and 19 of the report of the hearings for the Committee on the Merchant Marine and Fisheries which you have just examined as a whole, pages 18 and 19, for convenience, being copied below, and to state whether you discussed with Commander Hooper the subject matter of the statements made by Commander Hooper on those pages.

"Commander Hooper. May I say a word at this point, Mr. Secretary?

"The Secretary. Yes; certainly.

"Commander Hooper. If we had not purchased those sets for the Shipping Board we would have had to pay $1,000 a year on the apparatus for those remaining ships and as they intend to be in business, I understand, for about five years—or at least that seems to be the general opinion—that would have made $5,000 a set that we would have had to pay as rental.

"Mr. Edmonds. Is that with an operator or without an operator?

"Commander Hooper. They have to pay extra for the operator; the operator is paid for in addition.

"Now, those sets cost only about half of that amount, so it was a very good business proposition for the Shipping Board for us to buy those sets; and they asked us to buy them; and we had to take the shore stations in order to get them; and we are saving money for the Government by taking the whole business; and we would have been foolish not to have them throw the shore stations in if the Marconi Co. did not want them.

"Mr. Hardy. As I understand, you mean that if you were only using them temporarily you would still be effecting an economy by purchasing them with the shore stations?

"Commander Hooper. That is it exactly; that is the whole thing; we would have had to pay too high a rental.

"Mr. Humphreys. How much rental would you have had to pay?

"Commander Hooper. $1,000 a year for those sets.

"Mr. Humphreys. And how much did you pay?

"Commander Hooper. Well, we paid for the apparatus $1,200 for some sets and $2,500 for others.

"Mr. Edmonds. The companies kept them in repair on the ships, did they not?

"Commander Hooper. Yes; but that did not cost over $50 a year.

[fol. 1789] "Mr. Edmonds: That would depend on the age of the material, would it not, just like it would cost more to keep an old automobile in repair?

"Commander Hooper: Well, we counted on an average of $75 a year, putting it at the top figure.

"Mr. Edmonds: Does that include the overhead expenses, or just the cost of the actual physical repairs?

"Commander Hooper: That is, as we estimate, a fair price for everything. And Mr. Marconi told me that his average price was about $50 a year. Our relations have been very cordial and agreeable on the whole matter; there is no diversity of opinion between the Marconi Co., who made this deal, and the department as to the propriety of this deal. They did not want to keep the coastal stations, I believe, because they saw there would be no business for them; and it was a good business deal for them to sell just as it was a good business deal for the Government to buy them and save the Shipping Board all of that money.

"But the interest of those companies in fighting this bill is not on account of those coastal stations that you gentlemen are asking about. The opponents of this bill are fighting it on the high-power stations, which we have not bought; and that is the question that all of the arguments are coming up about; the matter of the coastal stations is something that they are satisfied with, and the Navy Department is satisfied with.

"Mr. Edmonds: Would they not sell you those sets without the coastal stations?

"Commander Hooper: No; that is the reason we had to buy them.

"Mr. Edmonds: Well, were you not buying sets on ships?

"Commander Hooper: We were buying sets of our own.

but the Marconi Co. had contracted with those shipowners of the ships that were requisitioned by the Shipping Board, which bound them to keep the sets that the Marconi Co. owned, and to pay that rental of $1,000 a year; and they wanted to get rid of that, so as to get it on the same basis as all the rest of the Shipping Board's ships.

"Mr. Edmonds: I do not suppose that any committee of Congress appreciates any more than this one the kindness of the Navy Department, in endeavoring to save the Shipping Board a few cents—certainly somebody ought to be able to do that for them. [Laughter.]

"Commander Hooper: Yes. Well, I think we hand-ed all of that business well, and I think the Shipping Board handled that business well; we saved them money; they wanted us to handle it all for them; and we made a good deal which nobody in the radio world objects to; we got what we wanted; the Shipping Board got what they wanted; and the Government made money on the whole business.

"Mr. Edmonds: Well, I suppose that the Shipping Board has spent that money that you saved by this time. [Laughter.]

"Mr. White: What did those sales include? Did they include any patent rights, or just the physical properties?

"Commander Hooper: They include the patent rights on the apparatus that we purchased—absolutely, clearly, and for all time. But not for any other apparatus except what we purchased; but as to the apparatus we purchased it is specifically stated in the contract that the patent rights are clear."

[fols. 1790-1797] Answer. I have no recollection whatsoever of having ever gone into the set of details set forth in the two pages to which the question refers me with anyone.

12. Cross-question. Commander Stanford C. Hooper was a member of the interdepartmental radio board in 1919, was he not?

Answer. A member in name, but not greatly active in the details of the functioning of the board.

13. Cross-question. Was the United States Navy or the Navy Department doing any commercial wireless communication business subsequent to June, 1910?

Answer. Yes, some commercial traffic was handled.

14. Cross-question. And the Navy made regular toll charges for such commercial business during the period that is was conducting such business, did it not?

Answer. Yes.

15. Cross-question. Can you tell over what period or periods the Navy carried on commercial wireless communication business for which it charged toll?

Answer. My information as to traffic operations in the Navy is not very reliable, as I mainly occupied myself with the engineering features of radio. I can not answer the question with any definiteness.

16. Cross-question. As regards such commercial toll business carried on by the Navy Department, you were aware, were you not, that it included the transmission and reception of communications between shore stations and ship stations at points on the Atlantic and Pacific coasts and the island possessions of the United States?

Answer. I know that the Navy handled commercial traffic and covered it by regular commercial traffic accounting, but just what regions were involved I never had occasion to consider.

17. Cross-question. Such commercial traffic as you referred to in your last answer was within the territory of the United States, was it not?

Answer. It was.

18. Cross-question. The United States Navy was engaged in commercial wireless communication traffic up to the time of the World War, was it not?

Answer. Yes.

(Claimant's counsel, with the consent of defendant's counsel, offers in evidence photostat copies of the title page and pages 18 and 19 of the Hearings before the Committee on Merchant Marine and Fisheries, etc., referred to by the witness, and the same is marked "Claimant's Exhibit No. 365."

Defendant's counsel offers in evidence the oscillogram CD120089 marked for identification at page 1406, and the same is marked "Defendant's Exhibit X-6"; defendant's counsel also offers in evidence plaintiff's Exhibit No. 22 and defendant's Exhibit 11-A in the Splitdorf case referred to on page 1406, and these are marked "Defendant's Exhibit Y-6 and Z-6," respectively.)

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(Tr. 2)

GEORGE H. CLARK, a witness produced on behalf of the claimant, having been first duly sworn by said Commissioner, was examined, and in answer to interrogatories testified as follows:

Direct examination.

By Mr. Blackmar:

Q. 1. Will you please state your name, age, residence and occupation?

A. George H. Clark; age 57; residence, 1055 Summit Avenue, New York; occupation, with the Department of Information, Radio Corporation of America, New York.

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(Tr. 3)

Q. 4. Will you state briefly what you had to do with the design of radio apparatus and particularly radio receivers purchased by the defendant for the United States Navy from July, 1910, to 1919, when you left the Navy Department.

A. I compiled all specifications for all these receivers, and also served as the electrical expert for the Navy Department and more particularly for the Washington Navy Yard on receivers and detectors, since that yard had been given the duty of being the official yard for those factors of a radio set. In that capacity I assisted in the design of many of the receivers and also assisted in the design of these receivers by a firm in Washington, the National Electrical Supply Company, which furnished a great many receivers to us. In my official trips to various other concerns my specialty was to keep in touch with them and helped them where I could on receivers and detectors.

[fol. 1799] (Tr. 126)

Q. 280. Mr. Clark, are you personally familiar with the type CN 208 receiver?

A. Yes, sir.

Q. 281. Were you familiar, at the time of the CN 208 receiver, with the instruction book of which Plaintiff's Exhibit No. 409 is a copy?

A. Yes, sir.

Q. 282. Will you please explain the antenna circuit of the CX 208 receiver, using the diagram appearing on the first page of Plaintiff's Exhibit No. 409, and also the photograph annexed thereto?

A. The antenna circuit passed from the binding post for the antenna to a variable condenser in series; from that point to a primary load coil, and from that point to another primary load coil marked as adjustable; from there to a primary coupling coil, and thence to ground.

Q. 283. At what points in that circuit were connections run to binding posts on the face of the panel of the receiver?

A. On the face of the panel there were binding posts for the antenna and for the ground, these being placed close to each other so that a protective sparking-gap could be mounted between the two in case of sparking or other high power; on top of the panel were two binding posts situated close to each other, and normally they are connected by a [fol. 1800] metal strap. If this strap were removed, an external loading coil could be connected across these posts, and it was so marked as a primary load coil.

Q. 284. Have you made a sketch of the antenna circuit shown schematically, and also shown with the binding posts as they existed in this type CX 208 Receiver?

A. Yes.

Q. 285. Will you state, please, what is also shown on that sketch, in general terms?

A. Fig. 1 shows the schematic circuit as it exists in the instruction book of CX 208. Fig. 2 is the same schematic diagram with the binding posts added and with the strap normally on the circuit, and the words primary load coil also added. In these two figures the connection is the series connection. Fig. 1a is still the series connection and shows the same binding posts as Fig. 2, excepting they are placed as they normally appear on the face of the panel. And Fig. 2a shows how, by change of connections on the exterior of the panel, the normal series condenser may be placed in shunt with the entire primary inductance.

Q. 286. Does this particular instruction book, Plaintiff's Exhibit No. 409, say anything, so far as you can find, with respect to the connections which result in the antenna condenser being connected in parallel with the antenna tuning inductance (handing exhibit to witness)?

[fol. 1801] A. No.

Q. 287. Were there other receivers purchased by the Navy which had the same arrangement of antenna circuit and binding posts as the type CN 208?

A. Yes, a great number.

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(Tr. 129)

Q. 289. Did you have anything to do with the design of the SE 95 or the SE 143?

A. Yes; I had much to do with the design of both.

Q. 290. What?

A. I had to do with the general electrical arrangement of the set and the actual building of some of the models both at the Navy Yard and by contractors, and had to do with certain specific arrangements of circuits which were novel in these receivers. These specific changes were originated by me and placed in here according to my ideas.

Q. 291. What do you mean by "placed in here"?

A. Placed in the design, I mean.

Q. 292. Did these designs differ as to the antenna circuit and particularly as to the location and connection of the antenna condenser from prior Navy types of receiver?

A. Yes; It was that change to which I referred as the change which I had placed in the design.

Q. 293. In what respect did these types differ from the earlier types?

[fol. 1802] A. In that the primary variable condenser was placed in the antenna side of the circuit rather than in the ground side, as had been done in all previous receivers.

Q. 294. What was accomplished, as a practical matter, by that arrangement?

A. The only reason for doing it was that by so placing the condenser it was placed across two binding posts which appeared on the face of the receiver, and that therefore any change such as the parallel change I referred to could be made without getting inside of the receiver and unsoldering leads. That was not the case when the condenser was in the ground circuit, as in that case one terminal of the condenser would be connected to the lower end of the coupling coil, and that had no binding post connected to it.

Q. 295. At the time of designing these receivers, had you had any practical experience covering a comparison of the desirability or efficiency of the series connection of the

antenna tuning condenser as compared to the parallel connection?

A. Yes. I have specialized in receivers from my first work in radio, and had found in a very great number of experiments that the parallel connection was better in certain conditions, and the series connection in other cases was better.

[fol. 1803] Q. 296. What were those cases; what was the difference between the two?

A. I found by many experiments that on long-waves signals were materially louder where the primary circuit contained the parallel connection, and found later that on shorter waves the series connection gave the louder signals.

Q. 297. Have you any particular instances of tests in mind, Mr. Clark?

A. I recall making such tests in 1903, when I first went into radio, and that was one of the first tests I made,—tests between Cambridge, Mass., and Lynn, Mass.

Q. 298. For whom were you working at that time?

A. For the Stone Telephone and Telegraph Company of Boston. I recall making a test on the U. S. S. Arkansas in 1912, between Key West and the Panama Canal Zone, and my log book record shows that the signals were three times as loud with the shunt connection and a long wave from the transmitter than the series connection gave.

Q. 299. What vessel were you on, Mr. Clark?

A. On the U. S. S. Arkansas, and the receiver used was the one to which I have already testified in this case, the E5. A third test, I recall clearly now, was the test made on the U. S. S. Salem in the so-called Arlington tests, when the transmitter at Arlington, then under construction, was being tested for acceptance, and the receiver on the Salem [fol. 1804] was being tested in receiving long waves from the temporary arc transmitter at Arlington, in 1913. On this cruise I tried both series and parallel connections, and I have examined my records and find that the parallel connection gave louder signals materially than the series connection.

Q. 300. In what ratio, do you know, Mr. Clark?

A. Two or three to one.

Q. 301. In favor of which one?

A. In favor of the parallel connection.

Mr. Blackmar: I offer in evidence the sketch which the witness has made and produced and which he has referred to in connection with the type CX 208 receiver as Plaintiff's Exhibit No. 410.

The Commissioner: So accepted.

(Said sketch was by the reporter marked as indicated, of this date.)

Q. 302. Mr. Clark, referring to your sketch, Plaintiff's Exhibit No. 410, just what would an operator have to do to arrive at the arrangement of your Fig. 2a?

A. The operator had to remove the lead from the antenna from its normal binding-post position A and change it over to the lefthand binding-post of the primary load system. I say the lefthand binding-post because if any load [fol. 1805] coil were in place, such load coil would not be shunted by the condenser, and that was the practice we adopted. The other change would be to connect this now open binding-post A to the binding-post G for ground, directly, beneath, by short wire. Those two changes were all that were required if it were done by wire connections.

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(Tr. 133)

Q. 303. Under what circumstances and in what conditions, met with in Navy practice, was it desirable to make the arrangements of Fig. 2a rather than that of Fig. 1a?

A. In two cases it became necessary on account of certain naval procedure which had just come into use. The two circumstances were these: The parallel connection would [fol. 1806] enable the receiver to be used with a smaller, shorter antenna than that for which it was normally designed. The receivers were always designed with the primary and secondary roughly of the same range of wave-length, but since the primary circuit contained the antenna this meant that the receiver primary coil was designed for a certain definite antenna, which was given to the contractor in electrical terms when the order was granted. If the antenna were smaller than this standard form, the capacity would be less and the wave-length range with the series connection would not correspond in the primary to that of the secondary. This could be obviated by putting the condenser in parallel for the longer wave-lengths.

By the Commissioner:

Q. 304. May I interrupt to ask if it was not also obviated by the use of the loading coil?

A. It could have been so done. It was not the practice.

By Mr. Blackmar:

Q. 305. What would the comparative effect be of using the loading coil and using the parallel connection of the condenser?

A. The loading coil would have given an increase of wave-length, but at a loss of efficiency.

Q. 306. Continue your answer?

[fol. 1807] A. (continued:) The smaller antenna was brought into the picture just about the time of the design of this first receiver, SE 95, by reason of the fact that the Navy for the first time used its outside antenna in which a low, single wire receiving antenna was placed below the deck of the ship on spars extending from the deck, so that it could not be shot away easily in action. That type of antenna had a very small capacity compared to the standard antenna for which the receivers were designed.

Q. 307. Before recess you were explaining the circumstances under which the parallel connection of the condenser would be desirable, and, I think, had explained one circumstance, namely, where a smaller antenna than the standard antenna was used. Continue your answer?

A. The second major importance of using the parallel condenser arrangement in the primary was in the case where a receiver to be used had a greater wave-length than the maximum for which it was designed. This feature was very prominent in the Navy after 1915 when arc transmitters were coming into play. These arc transmitters used wave-lengths much longer than the previous spark sets, but in the case of a number of vessels there were not enough receivers available to give them one for the spark and one for the arc.

[fol. 1808] By the Commissioner:

Q. 308. What date was this?

A. About 1917.

By Mr. Blackmar:

Q. 309. Continue with your answer.

A. (continued:) Therefore, temporarily arrangements had to be made to receive the arc transmitter beyond this

range. This was done by placing a loading coil in the secondary circuit for which binding posts and a short-circuiting link were provided; this could have been in the primary, but for greater efficiency the primary condenser was placed in parallel with the inductance instead of adding the loading coil. This use was very common in the majority of ships in the Navy around 1915, and the condition was removed possibly two or three years later by a greater supply of long-wave receivers which could be given to the ships for use exclusively on arc-reception.

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(Tr. 146)

Mr. Blackmar: I offer in evidence as Plaintiff's Exhibit No. 413A the copy of the Instruction Book of the type SE 143 receiver.

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(Tr. 181)

Q. 438. Will you please produce the simplified diagram of SE 950 receiver, and briefly explain what is shown thereon, referring to Sheet 8 of Plaintiff's Exhibit No. 416A?

[fol. 1809] A. Fig. 1 of my abstract is taken directly from the picture of the cam and its switches on Sheet 8 of Plaintiff's Exhibit No. 416A. Fig. 1A shows the schematic diagram, including all of these springs which are in operation for series connection, and including the antenna and ground and inductances, which are in series. These latter are taken directly from Sheet 8 of Plaintiff's Exhibit 416A.

Q. 439. What do the letters and numbers on those diagrams refer to, Mr. Clark?

A. The letters were taken from sheet 8 of the exhibit. The numbers I placed in myself so that we could identify it from the springs themselves, as the working drawing to the schematic diagram, Fig. 2 shows the cam in the position when it is rotated to the opposite position as shown in Plaintiff's Exhibit No. 416A; that is to say, the switch arm is no longer on contact A for the shortest wave, but is on contact G for the longest wave. In this position, as I have testified from the instruction book, the receiver has parallel condenser connection in the primary automatically provided by switching mechanism. The switches shown in Fig.

2 are the same as those in Fig. 1, excepting for the new position of the cam. Fig. 2A shows the schematic diagram of the shunt connection making use of the figures on the springs of Fig. 2 and the electrical data in Sheet 8 of the instruction book.

Mr. Blackmar: I offer in evidence as Plaintiff's Exhibit No. 419, the sketch prepared by the witness, and which [fol. 1810] has just been described.

The Commissioner: So accepted.

(The sketch was by the reporter marked as indicated, of this date).

Q. 440. Will you please produce your diagram with respect to the type SE 1012 receiver, and explain it in connection with the diagram on page 12 of Plaintiff's Exhibit No. 417, and also in connection with the change or correction in that diagram, as to which you testified yesterday?

A. In the simplified drawing which I have made for SE 1012, Fig. 1 shows the cam and the switches in their position as shown on Sheet 12 of the instruction book. The numbers which I have on my sheet for the terminals of the springs are the same as on Sheet 12. As I said in my testimony yesterday, the diagram is incorrect, in that springs 5 and 38 remain closed, no matter what is done with the cam switch. I, therefore, have re-drawn the cam switch and its springs in Fig. 1A, which shows spring 5 extending beyond spring 4, with an insulated section between the two and a bend on the end of spring 4, so that, under certain conditions, spring 5 and spring 4 can make contact. As they are shown in Fig. 1A, in the parallel connection, they are insulated. Fig. 1 and Fig. 1A show the inductance switch in the parallel position, that is to say for the longest wave range of the set; and Fig. 2 shows the cam turned to the shortest wave length. In the first case the connection is the parallel connection of condenser. In the second, it [fol. 1811] is the series connection. Taking the same numbers for the switch terminals, and also the numbers given on page 12 of the schematic drawing, I have shown in Fig. 1B the complete connection for the shunt arrangement, which obviously shows on the face of it that it does have the condenser 6, 7 in parallel with all of the inductances then in use on that step. Fig. 2A shows clearly that the condenser 6, 7 is in series with the inductance.

Q. 441. Referring to your Fig. 1A, is the change made therein, as compared to Fig. 1, in accordance with the construction diagram which you testified that you examined in the Navy Department files?

A. Yes, it is.

By the Commissioner:

Q. 442. Is it in accordance with the actual construction of these sets?

A. The construction diagram of the sets, sir. I looked that up in the Official files of the Navy Yard. I did not look up the set.

Q. 443. I take it you are more or less familiar with these sets, having used them?

A. I can say this, sir. It certainly worked correctly, as if the switches were correct. And they were correct in operation.

By Mr. Blackmar:

Q. 444. And if the arrangement were in accordance with Fig. 1, would it have so worked?

[fol. 1812] A. No, sir, it could not have worked at all.

Q. 445. Is there another position of this inductance switch which will give the parallel arrangement of the condenser more or less as shown in your Fig. 1B?

A. Yes. I have shown it on the eighth or last step. And the same is true on the seventh or next to the last step. All of the others are in series.

Q. 446. In general, what would be the difference between the next to the last step and the last step?

A. None whatever, excepting that the switch, 37, in Fig. 1B would go to a different point on the inductance, namely, instead of going to point 30 it would go to point 35.

Q. 447. Is point 35 shown on Fig. 1B?

A. No. It is shown in page 8 of the instruction book, however.

Q. 448. Approximately where is it on Fig. 1B?

A. It forms a considerably less portion of the antenna inductance than when the coil is tapped at point 30, as in position 8. That is so that overlapping will be obtained between the two positions.

Q. 449. That is to say, it is another tap on the same coil that 30 is a tap on?

A. Yes, it is.

Mr. Blackmar: I offer in evidence as Plaintiff's Exhibit No. 420 the sketch referred to by the witness, with respect to the receiver type SE 1012.

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[fol. 1813] Q. 464. What would have to be done to that receiver to connect the antenna tuning condenser in parallel with the primary inductance?

A. The antenna would have to be placed on the left-hand binding post of the primary load coil and the antenna binding post would have to be connected to the ground binding post, and thence to ground.

By the Commissioner:

Q. 465. If that were done, what effect would that have on the primary circuit with respect to the wave-length range?

A. If the anenna being used at the time on the ship was less in capacity than the one for which the set was designed, it would tend to bring the wave-length range of the primary up toward its normal range for which it was designed. It might do that, or it might not, depending on the antenna. If the antenna for which the set were designed were in use, the effect would be to increase materially the wave-length range of the primary circuit over that of the normal range of the secondary. The latter could be brought into the same relation by adding a loading coil.

Q. 466. There would be two different purposes achieved?

A. Those are the two standard emergency purposes, yes, sir.

[fol. 1814] Q. 774. Mr. Clark, in the cases of the CN 208, CN 239, CN 240 and SE 95, I note that the instruction books refer to the nominal wave-length range of the secondary circuit and also to the actual range, the actual range in every case being greater than the nominal range. Do you know what was meant by actual range and nominal range?

Mr. Edwards: I object to his stating what was meant by it. He is in no position to state what they meant. All of us can read English.

The Commissioner: Ask the witness what it means to him.

Q. 775. In view of your familiarity with the radio art and these receivers, Mr. Clark, what do "nominal range" and "actual range" of secondary circuits mean to you?

A. In cases where the range of the primary circuit and the range of the secondary circuit are indicated as different, the nominal range of the secondary would be that range which agreed with the primary and, therefore, the range to which the whole receiver would operate. The actual range means the total maximum wave-length to which the secondary circuit could be tuned which would be greater than the nominal range.

Q. 776. In the case of the actual range, did that include the use of an external loading coil in addition to the coil in [fol. 1815] the receiver?

A. No. The reference was to the constants of the receiver within the panel.

Q. 777. By what means could these receivers be used in the actual range of the secondary where this actual range was of a longer wave-length than the nominal range and longer wave-length than the given range of the primary circuit?

A. Assuming that the antenna used was that for which the primary was designed, the range of the primary could be increased to agree with the actual range of the secondary either by placing a loading coil in the primary or placing the normal series primary condenser in parallel with the primary inductance, or by a combination of both.

Q. 778. Referring to your sketch, Plaintiff's Exhibit 410, does that apply to the Type SE 952 or SE 952A receiver in the same way that you applied it to the CN 208 receiver as appears in your answer to Q. 285 at page 128 of the record?

A. Yes, it does.

Q. 779. Does it also apply in the same way to the SE 899 and SE 998, the SE 1220 and the SE 95?

A. Yes.

* * * * * * *

[fol. 1816] (Tr. 306)

Q. 782. You have explained at least one of the circumstances where the parallel connection of the antenna tuning condenser was used in Group III receivers, namely, where the antenna was smaller than the antenna for which the set was designed and where it was desired to increase the normal range of the primary circuit where a standard antenna was used. What was the importance of the first circum-

stance, the smaller antenna, with respect to short wave receivers as compared to the long wave?

A. It applied about equally to each case.

Q. 783. Referring to the second circumstance, increase in the range of the primary circuit, what was the comparative importance or utility of the parallel condenser arrangement as applied to short wave receivers compared to applying it to long wave receivers?

A. It was of much greater importance in the case of the short wave receivers.

Q. 784. From your personal experience, with which type of receiver was the parallel arrangement more used?

A. It was used more often, considerably, with the short wave type of receiver.

Q. 785. Can you explain the electrical reasons for that?

A. Yes. From actual tests and experience I know that in using a long wave receiver when we had to go to a longer [fol. 1817] wave still, the increase in the new wave would not be very much greater than the original range of the receiver; it was already a long wave receiver. But if we wished to go to the new long wave from a short one, using a short wave receiver, it would have required a great change in the constants of the receiver. Had that change been made, as we sometimes tried, by putting loading coils in both circuits, there would have been no coupling left between the two circuits on account of the fact that all the inductance was in the loading coils.

Q. 786. You are speaking of the short wave receiver?

A. Yes. It was much more efficient to cut out your loading coil in the primary as much as possible by putting the condenser in parallel. That has been supported by many tests and experiments. I am not explaining it by theory but by actual tests I made.

Q. 787. Then, for example, where you had a long wave receiver, such as many of these are, with a maximum nominal range of 10,000 meters, and you wanted to receive a 14,000 meter wave, what was the usual practice?

A. It was more likely that a loading coil would be placed in both circuits.

Q. 788. And was that also true on the short wave receiver?

A. The usual practice was to use a load coil in the secondary and to make use of the parallel condenser in the primary, and, if necessary, add a small coil in the primary

to bring up the wave length if that was not already done by the condenser.

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[fol. 1818] (Tr. 321)

Q. 821. Can you state from your knowledge, Mr. Clark, whether any of the wireless receivers which have been covered in this proceeding were designed by men actually in the employ of the Navy Department?

A. All the types which begin with the letters "SE" were so designed, and the greater part of the design of the receivers beginning with other letters was also done by Navy personnel.

Q. 822. What does "SE" in the Navy Type Number Book mean?

A. Designed by employees of the Bureau of Steam Engineering at the Bureau or any of the yards.

Q. 823. In Navy type numbers what does the letter "C" following by another letter such as "N" and preceding the type number mean?

A. "C" means the design was made in a commercial factory, and the letter "N" following it in this particular case identifies the National Electrical Supply Company of Washington, as they are a commercial concern.

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[fol. 1819] By Mr. Blackmar: (Tr. 322)

I offer in evidence as Plaintiff's Exhibit 430 photostats of the following pages of Navy specifications RE 13A 242B, of which, I understand, a copy is also on file in the Court of Claims in response to Schedule F of plaintiff's call on the Navy Department. It is understood that the photostats to be submitted are subject to verification and correction from the copy on file in Washington: The cover which is marked Sheet 1, and pages 3B, 4A, 5B, 9A, 10A, 12A, 13A, 14A and 15A, and drawing RW 46A 417A annexed to the specifications.

* * * * * * *

Q. 824. Mr. Clark, from your experience with Navy types of receivers, is there any practical advantage in locating the antenna tuning condenser on the ground side of the antenna inductance rather than on the antenna side?

A. Other things being equal, yes, there is.

[fol. 1820] Q. 824a. What is it?

A. The connection of one side of the condenser to the ground means that when the operator brings his hand up to the knob of the condenser to tune it he does not introduce a false capacity which is removed when he takes his hand away and therefore leaves the circuit slightly mistuned.

Q. 825. And that spurious effect is present under which circumstances?

A. That would be present if the primary tuning condenser is placed in any other part of the primary circuit other than with one side of it grounded.

Q. 826. And when it is connected on the antenna side of the inductance coil, is either connection of the condenser grounded?

A. No.

* * * * * * *

(Tr. 388)

Q. 990. Will you please refer to Plaintiff's Exhibit 430, sheet RW 46A 417A thereof, and describe the operation of the device marked "Antenna inductance switch"?

A. This switch cut in various sections of the primary coupling coil and the antenna inductance or loading coil, and simultaneously therewith placed the antenna tuning condenser either in series with the total primary inductance on the shorter wave lengths or in parallel with this total inductance on the longer wave length.

* * * * * * *

(Tr. 580)

[fol. 1821] GREENLEAF WHITTIER PICKARD

GREENLEAF WHITTIER PICKARD, a witness produced on behalf of the plaintiff, having been first duly sworn by the Commissioner, was examined, and in answer to interrogatories testified as follows:

Direct examination.

By Mr. Blackmar:

Q. 1. Please state your name and residence.

A. Greenleaf Whittier Pickard, Seabrook Beach, New Hampshire.

Q. 2. Will you state your occupation?
A. Consulting electrical engineer.

* * * * * * *

Q. 4. Will you please state your education and experience in the radio field, and particularly in the commercial and manufacturing side of that field?

A. My academic education was at Harvard College and Massachusetts Institute of Technology; but in as much as I was then in training for the field of chemistry I think my principal qualification is my professional record.

I entered the field of radio while still in college, as summer work, during 1898 and since then, and down to the present time I have been continuously engaged in the field of radio communication. From 1907 to 1931 I was consulting engineer, for the entire period mentioned, and an officer and director for the major part of this period, of the Wireless Specialty Apparatus Company and its successor, the RCA-Victor Company of Massachusetts. My work for this company involved not only research work in the field of [fol. 1822] radio communication but the designing and, to a very considerable extent, the development of production and final forms of apparatus for manufacture.

I have been, through this entire period, quite familiar with manufacturing costs, this coming in the line of duty as an officer of a manufacturing company.

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(Tr. 581)

Q. 5. Did you state what the business of the Wireless Specialty Apparatus Company was, Mr. Pickard?

A. The Wireless Specialty Apparatus Company was during its entire existence a manufacturer of radio apparatus, starting with receiving apparatus in the first few years of its existence, and finally both receivers and transmitters.

* * * * * * *

[fol. 1823]

(Tr. 677)

Q. 46. Referring to the Marconi patent No. 763,772, the Court of Claims, as you know, has held Claim 16 of that patent covers a new and useful invention, but that the remaining claims thereof, which were in issue in the case, are invalid in view of prior art and certain prior disclosures. Will you first, please describe the subject matter of Claim

16, as distinguished from the other claims of that patent?

A. Claim 16 of the Marconi patent relates only to a receiver, whereas the other claims or certain of the other claims of the patent relate to transmitters and to combinations of transmitters and receivers.

Q. 47. What is the difference between the subject matter of claim 16 and that of the other receiver claims of the patent?

A. The distinction is that in the antenna circuit described by Claim 16 the tuning element in the form of an adjustable condenser is placed in a shunt connection around the transformer coil; that is, around a portion of the inductance in the antenna circuit.

Q. 48. And are the other elements of that claim common to other claims in the patent?

A. Yes, they are.

Q. 49. Is the arrangement of that claim illustrated in any figures of the patent?

[fol. 1824] A. Yes. Figure 2 of the patent. All of the elements of claim 16 are shown including an antenna circuit, f′ A, g′, j′, E, and the shunt condenser h; also the secondary circuit called for in Claim 16 is shown in Figure 2, consisting of the coils j^2, g^2, the condenser h′ and the detector T, with its associated indicating circuit C′-B-R-C^2.

Q. 50. I believe that the record herein shows that in certain of defendant's receivers the antenna circuit differs from that shown in Figure 2 of the Marconi patent, in that the shunt condenser in the receivers is connected not only around the primary coil of the transformer but also around a loading coil. Do you know of any of the exhibits on the main case—exhibits on the main case referred to by the Court of Claims in its findings and opinions—showing the arrangement of the defendant's receivers that we have been speaking of?

A. As I recall the exhibits, in all cases the shunt condenser h of Figure 2 of the Marconi patent is placed around both the primary of the oscillation transformer j′—referring again to Figure 2 of the Marconi patent—and the loading inductance g′. I do not offhand recall any exception to that.

Q. 51. Will you please look at Plaintiff's Exhibit 79, which is printed opposite page 180 of the original record in this case, and tell me whether that arrangement is the arrange- [fol. 1825] ment of Figure 2 of the patent?

A. That is the arrangement of Figure 2 of the patent, even to the lettering, so far as the lettered elements are the same. However, in Exhibit 79 there is no separate loading coil, g', such as appears in Figure 2 of the Marconi patent.

Q. 52. Yes. And in comparing it you are referring only to the antenna circuit, I take it?

A. Yes, that is correct.

Q. 53. You are not trying to compare the other details of Figure 2 with the apparatus; is that correct?

A. That is correct.

Q. 54. Will you also please look at Plaintiff's Exhibit 95, which appears opposite page 183 of the main record, and tell the Commissioner how that compares with Figure 2 of patent, so far as the antenna circuit is concerned?

A. Briefly, it differs only from Figure 2 of the Marconi patent in that the condenser h is placed in shunt or parallel connection around both the primary of the oscillation transformer and the exterior loading inductance, whereas in the Marconi patent the condenser h is only shown as connected around the primary of the oscillation transformer.

Q. 55. In other words, is there a coil in the antenna circuit of Exhibit 95, referring to the receiver that corresponds to the coil g' of Figure 2 of the patent?

[fol. 1826] A. Yes. It not only corresponds, but in the Figure 2 of Exhibit 95 the coil is lettered g' as it is in the patent.

Q. 56. The antenna tuning condenser when connected in parallel arrangement in Figure 95 extends around both the primary of the coupling coil and the coil corresponding to g' of the patent. Is that right?

A. That is correct. I omitted to state that in Exhibit 95 the condenser is actually shown in two positions. It is shown in dotted or broken line also with the same letter h, in a series connection in the antenna circuit, but I omitted mention of that because in reading a diagram one customarily refers to only that portion shown in the full line, that being the convention- or the operating connection shown.

Mr. Blackmar: I call the Commissioner's attention to the fact that the Court of Claims in Finding LXIII held that the receiving apparatus of the Kilbourne & Clarke Company shown in Exhibit 95, which is the one which the wit-

ness has just been referring to and the receiver made by the Telefunken Company, illustrated in Exhibit 79, which is the one previously referred to by the witness, each has apparatus coming within the terminology of Claim 16.

Q. 57. Mr. Pickard, referring to the subject matter of Claim 16 of the Marconi patent, will you please state [fol. 1827] what, if any, advantages there are in connecting an antenna tuning condenser in parallel with the antenna inductance coil or coils as compared either to connecting such condenser in series with those coils, or omitting such condenser, and in your answer will you briefly explain the reason for each advantage?

A. There are several advantages: first, with a given set of coils—that is, with a given oscillation transformer and a given loading coil, such as those shown in Figure 2 of the Marconi patent as j′ g′ the range in wave length tuning of the antenna circuit is multiplied some two- or more -fold as compared with the range which would result if the condenser were either omitted or were used in a series connection in the antenna circuit. Secondly, there is a very practical advantage.

Q. 58. Will you briefly explain the reason for the first advantage, please?

A. The reason, briefly, is that: when the condenser is in a series position in the antenna circuit, the range of tuning is limited. For example: taking any particular value for [fol. 1828] the antenna capacity and any particular value or values for the maximum and minimum capacity of the tuning condenser in the series connection, the two capacities—the antenna capacity and the tuning capacity—are in series, so that a resonant circuit is formed, in which, disregarding the inductance of the antenna itself, there is lumped inductance corresponding to the primary of the oscillation transformer and the loading coil, which may be considered as one inductance for this purpose paralleled by the antenna capacity in series with the tuning or primary condenser capacity. The sum of the capacities in a series circuit is given by an exceedingly simple formula which is that the capacity of two condensers in series is equal to the reciprocal of the sum of the reciprocals of the two capacities. To get this into the record numerically, or, rather, algebraically, if the two condensers have capaci-

ties of a and b, respectively, for the antenna and for the tuning capacity, the value of their sum in the series relation is

$$\frac{1}{\frac{1}{a}+\frac{1}{b}}$$

It is very rarely indeed that a ratio of more than ten to one between maximum and minimum is found in the type of variable air condenser used during the period in which we are interested, in radio receivers. If the tuning condenser were the only capacity in the circuit, then the range of capacity variation would be ten to one, and the [fol. 1829] range in wave length variation would be three to one. That is the square root of the capacity variation. But when the condenser is in series with the antenna capacity, the range is much less, so that only a comparatively small variation in wave length range can be obtained by the series connection. Where the condenser is placed in parallel, the capacity in the circuit available for tuning the circuit is simply the sum of the two capacities; that is, the antenna capacity and the shunt capacity. And with the condensers of the type ordinarily used at that time, as, for example, in the SE 143 receiver, the capacity ranged from a minimum value of about .0008 microfarad to approximately .0045 microfarad, and the antenna which was specified for use with the SE 143 had a capacity of .0008 microfarad. Under those conditions, and in view of the fact that the maximum capacity of the condenser, the primary tuning condenser, was some five- to six fold that of the antenna capacity, the actual range of capacity available for tuning in the parallel position was from .0016 microfarad to .0053 microfarad. That is a range of between three- and four-fold in capacity and a range of something under two-fold in wave length. In the series position with the same values a smaller range of tuning was all that could be obtained, owing to the fact that when the capacity of the series condenser became materially larger than the capacity of the antenna, the capacity of the antenna be- [fol. 1830] came the conrtolling capacity in the circuit, and further increase in the capacity of the primary condenser only brought about a small increase in the wave length of the tuning.

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A. Continuing my answer to your last question, a second advantage of the parallel antenna circuit tuning of the Marconi patent has to do with selectivity; that is, the ability of the circuit to accept or reject desired or undesired frequencies. While this, if one goes into all its ramifications, is an exceedingly complex matter, I think I can briefly explain it in this manner: referring now to Figure 2 of the Marconi patent, and considering that the condenser h is actually placed in a shunt or parallel circuit with that of the lumped inductance of the antenna circuit, electrically we have three elements in parallel: first the inductance; second the tuning condenser, and third the antenna view as a capacity.

When, as, for example, in the SE 143 receiver, the capacity of the tuning condenser is greater than that of the antenna, then the current flowing into the third branch of the circuit to which I have referred as a three-element circuit, is small as compared with the current flowing in either the condenser h or the inductance g′, j′. In consequence, the losses in that part of the antenna circuit exterior to the receiver are minimized.

For example: a frequent source of loss or high resistance in an antenna circuit lies in the ground connection, and [fol. 1831] this loss is an ohmic loss; that is, it is a real resistance which can be measured in ohms. The loss of energy in such a resistance is proportional to the square of the current flowing through such a resistance, and if the current flow in that part of the circuit can be minimized, the loss can also be minimized, and when we consider the selectivity as shown, for example, by the resonance curve of the circuit the thing that determines the breadth or narrowness of the curve—that is, the question of whether it is a highly selective or a poorly selective circuit—is nothing more or less than whether or not there is small or great loss; that is, high or low resistance in the circuit. So it has been found as a practical matter. I have repeatedly made measurements on different antenna systems such as are customarily found at stations and with the parallel connection, of course, with the longer wave lengths, the energy losses due to resistance in the antenna system are minimized and the sharpness of tuning is accentuated, increased, as compared with a simple series tuned circuit—that is, a circuit in which the antenna capacity, the tuning condenser and the inductance are all

in a single series circuit, and it follows also that anything which increases the selectivity of a circuit, just because whatever increases the selectivity does it by lowering the resistance or decreasing the losses, makes the circuit more efficient.

[fol. 1832] In other words, the weaker the field, the weaker signal field from a transmitter would give a useful signal in the detector circuit; whereas with high losses in the circuit no useful signal would result, and, finally, from both the manufacturing and the operator's viewpoint, the use of the parallel condenser simplifies the operation of the receiver. One building a receiver only has to use one-quarter or less of the inductance in the antenna circuit required to cover a given wave le gth range which would be needed if parallel tuning were not employed. The inductance coils can, therefore, be made smaller. That is to say, more cheaply, and, to some extent, more efficient. From the operating standpoint, either by the throw of a switch or as in some receivers during the period with which we are concerned, done automatically, the receiver can pass from series to parallel tuning without any loss of time. Those three advantages that I have just mentioned are, as I see them, the principal advantages of pa llel tuning, such as is shown in the Marconi patent.

Q. 60. And are there also any advantages, comparing a receiver that has both series and parallel connection available with one that has only the series connection or no antenna condenser?

A. Yes, there are quite marked advantages. In the first place, if the receiver is restricted to merely series tuning, the coil system will have to be rather largely increased in [fol. 1833] magnitude, and, what is perhaps even more important, additional switch points will have to be provided, so that the inductance can be more accurately controlled; and that brings to my mind one other advantage; the subdividing of inductance by bringing out leads to a switch is always a source of loss, principally because the leads coming out from the coil to the switch are of different radio frequency potentials and there is a considerable dielectric loss in the panel holding the switch and in the leads coming out to the switch. It is an advantage, therefore, to have as few loss points as possible; and in the absence of parallel tuning, in order to cover any extended range, it is necessary to subdivide the inductance in the antenna cir-

cuit into quite a large number of steps, and that is not good practice. It introduces, as I have just said, losses. Also, if we are restricted to merely a series condenser, the selectivity of a circuit will be lessened, particularly on the longer wave lengths, so that from both the manufacturing standpoint and an operating viewpoint the receiver is less efficient.

Q. 61. Is there any advantage in the parallel connection over the series connection with respect to transfer of energy into the secondary circuit from the primary?

A. Yes, there is this advantage: that the parallel circuit with respect to the source of electromotive force, which, of course, is the antenna itself, is a divided circuit which is [fol. 1834] tuned to resonance. The current in each branch of such a circuit is considerably greater than the current flowing in the antenna, because, to put it mathematically, the current in the antenna is the vector sum of the two currents in the loading inductance, and the parallel condenser, and as these two currents, or the vectors which represent them are only 180 degrees apart in angle, the impedance of the circuit formed by the parallel condenser and the loading coil is high, as viewed from the antenna and ground connections, so that the current, being high in this circuit, a comparatively small coupling coil is all that is required for the maximum useful coupling into the secondary circuit; so there is potentiality, at least, of better transfer of energy into the secondary circuit when using parallel tuning than when using series tuning. Of course, one could get equivalent coupling transfer of energy into the secondary circuit with series tuning by increasing the dimensions—that is, the inductance in primary of the oscillation transformer.

Q. 62. Is the use of the parallel connection of the antenna tuning condenser particularly advantageous for any one range of wave length as compared to any other range of wave length?

A. Yes, it is peculiarly advantageous for the longer wave lengths.

[fol. 1835] Q. 63. Will you briefly state why it is advantageous?

A. It is advantageous for the reason that in parallel tuning the capacity of the primary condenser is arithmetically added to the capacity of the antenna, so that the sum of these capacities, being large, for a given coil system,

the resonant period is long—that is, the system is tuned to a long wave length, as compared, for example, with the same coil system with the series condenser, where the capacities are in series, and therefore their sum, which is the reciprocal of the reciprocals of the capacities, is smaller than either one.

Q. 64. You have referred to an advantageous selectivity with the parallel connection. Is that advantage more or less noticeable on the longer wave lengths as compared to the shorter wave lengths?

A. It is more noticeable on the longer wave lengths. There are several reasons for that. One of them has to do with what we call dielectric loss—that is, the loss of energy at radio frequency or in radio frequency fields of all dielectrics traversed by these fields, such, for example, as the insulation on coils, the imperfect dielectrics surrounding or near the antenna itself, which may not be only insulators supporting the antenna but structures which support the antenna system itself. These dielectric losses vary directly with wave lengths. The longer the wave the greater the dielectric loss in any ordinary dielectric, so [fol. 1836] that the selectivity of the system, which depends primarily upon the amount of the losses, is greatest when the currents and therefore the voltages in the antenna system are kept at a minimum; so the selectivity, other things being equal, is greater at the lower frequencies; that is, greater at the lower frequencies when parallel tuning is used. If series tuning were used at the lower frequencies—that is, the longer waves, the loss in the coil system due to radio frequency voltages producing fields through the coil insulation would be greatly increased, because there would be many more turns on the coils and many more chances for loss, and the same thing, of course, holds on the various losses in the antenna system itself.

Q. 65. Is there any relation between the number of communication channels available on the longer wave lengths and selectivity?

A. Yes, there is. The number of communication channels is a matter of frequency, rather than wave length. We have, of course, two systems, the earliest of which was to express things in terms of wave length. Nowadays we generally speak in terms of frequency, so many kilocycles per circuit, and the width of a communication channel, whether it be used for radio, telegraph, or for voice or

music or what-not, is a definite number of cycles or kilocycles. The lower frequencies—that is, the longer wave lengths, have, therefore, comparatively few channels, and [fol. 1837] in order to fully utilize these longer waves it is necessary to space or allocate station frequencies rather closely together. If that were not done, there would be very few channels available between, say, 10,000 and 20,000 meters. So it is increasingly important as we go up in wave length to get high selectivity.

Q. 66. Has the decrement at the transmitter anything to do with this or was that covered in your discussion as to dielectric losses?

A. No, that was not covered, and it has a very important bearing indeed, because if the transmitter were one of high decrement, that is, short train of waves emitted regardless then of the selectivity available at the receiver very little discrimination could be made between stations on closely adjacent wave lengths. The way in which we look at that matter today is simply this: that a station of high decrement occupies a wide band. It is the equivalent of a transmitter radiating at a number of different frequencies simultaneously. As the wave train lengthens, that is, as the number of useful oscillations in the wave train increase, the transmitter approaches more and more nearly to a single radiated frequency, at which, of course, the selectivity of the receiver is at a maximum. So with high decrement transmitters there would be no particular purpose in using very selective receivers. No advantage would be obtained.

Q. 67. How far, if at all, do these various advantages of the parallel connection, which you have discussed, apply [fol. 1838] to the various types of receivers purchased by the defendant as set forth in the record?

A. They apply in somewhat varying degree, because these receivers have been divided already into classes in which the change from series to parallel tuning is accomplished automatically by a switch connected with the inductance varying switch and another class in which a double pole double throw switch, for example, is mounted outside the receiver but still practically instantly available to the operator, and a third class in which there is no switch available but one can change by change of connections. From an operating standpoint the first two classes that I have

mentioned are the best, because no time is lost in making the change.

Q. 68. You stated that the advantages apply in varying amounts to the various receivers. Do they apply to all of the receivers to some greater or lesser extent?

A. I only discussed in my last answer the advantages from an operating standpoint. All three classes of receivers have electrically the same advantage; that is, they have the possibility of change from parallel to series tunings. They differ only in the ease with which that can be accomplished, but when it is accomplished the advantages are the same for all.

Q. 69. Have you prepared a chart showing the effects of the series and parallel connections with the type SE 143 [fol. 1839] receiver, as that receiver is described in the instruction book, Plaintiff's Exhibit 413-B?

A. I have done so, and I hand you the chart.

Mr. Blackmar: I ask that this be marked for identification as Plaintiff's Exhibit 440.

(The paper just offered was by the reporter marked as indicated, together with his name and the date.)

Q. 70. Mr. Pickard, will you kindly explain that chart and also what it shows? In the first place, state what constants you have used and the origin of the constants.

A. I have used in making this chart the constants given in the instruction book of the SE 143, these constants being an antenna capacity of .0008 microfarad, a primary or antenna tuning condenser having a capacity range of from .0008 to .0045 microfarad and a total antenna inductance—that is, the sum of the coupling or oscillation transformer primary inductance and antenna load coil inductance of from 81 microhenries to 4220 microhenries.

The four curves shown in this chart are, proceeding from the bottom to the top, the series connection of the antenna condenser with the minimum value 81 microhenries of inductance, and there the tuning range runs from 340 meters to 442 meters. Owing to the scale of ordinates required to show all of the ranges this lowest curve is rather flat in appearance. The abscissa of this chart are degrees on the [fol. 1840] condenser scale—that is, assuming a condenser having 180 degrees of rotation from maxima to minima and the ordinates are wave lengths in meters. The second curve

going up from the bottom is for the parallel connection of the antenna condenser with the minimum inductance, 81 microhenries and there the range is from 677 meters to 1236 meters. The third curve is a series connection with the entire antenna and oscillation transformer primary inductance in circuit and there the range is from 2450 to 3187 meters, and finally the topmost——

Q. 71. Before you take the next one, that accords substantially, does it not, with the maximum range of the primary circuit as indicated in the instruction book (handing book to the witness)?

A. Yes, that is correct, it does; and finally the fourth and upper curve is for the parallel connection with primary condenser with the maximum value of inductance, and there the wave length range is from 4900 to 8717 meters. I may say, in connection with this chart that it must not be taken too exactly at the lower end of all of these curves—that is, where the wave length is at a minimum and the capacity of the tuning condenser whether series or parallel is at a minimum. The reason for that is that the antenna characteristics other than its capacity come into the picture there. So, for example, on the lower curve, which, considering the antenna as a pure capacity without any induct- [fol. 1841] ance, runs from 340 to 442 meters, the lower end of that might easily be a number of meters higher or lower, depending upon the length of the antenna, and therefore on its distributed constants, particularly its inductance, as we progress to longer and longer wave lengths, the antenna behaves more and more like a pure capacity, and the two upper curves, for example, except perhaps for the first few degrees of the condenser scale, may be considered as quite closely correct.

Q. 72. In preparing that chart did you make any assumption as to the form of plates of the variable condensers?

A. Yes; I assumed the then ordinary semi-circular plates and I assumed a calibration curve for this condenser in which for the first few, the first first five or six degrees and the last five or six degrees of the condenser scale—that is, for the portions of the curve near the minimum and near the maximum—that the calibration curve would round over and would be approximately horizontal, but for the major part of the condenser scale the calibration would be linear—that is, the capacity would vary directly as the movement.

That I know from many, many laboratory measurements on such condensers as the fact with all of these condensers, and if you wish I will produce the calibration curve which I actually used.

Mr. Blackmar: I ask that it be marked for identification as Plaintiff's Exhibit 441.

(The paper just offered was by the reporter marked as [fol. 1842] indicated, together with his name and the date.)

Q. 73. Mr. Pickard, if your assumption as to the form of calibration curve of the condenser were incorrect, what effect would that have on the curves of Exhibit 440 for Identification?

A. It would have this effect: it would change the shape of the curve but it would not change the wave length range—that is, the values given at the top and the bottom of these curves on the exhibit are fixed by the maximum and minimum values, and those in turn are taken from the SE 143 instructions, and regardless of the shape taken by the plates, whether they are semicircular, logarithmic, or otherwise, the range covered by any one of the connections and any particular set of inductance values would be the same as I have shown in my chart. The only thing that would be affected would be the shape of the curve lying in between the maximum and minimum values.

By the Commissioner:

Q. 74. In other words, it might change the characteristics of the curve but not its end points?

A. Not its end points and not its range, its ratio from maximum to minimum.

By Mr. Blackmar:

Q. 75. Referring also to plaintiff's Exhibit 440 for identification, it might appear that there was a gap be- [fol. 1843] tween something over 1,000 meters and something near 2,500 meters that is not covered by the receiver. Would you explain that, please?

A. Yes. For simplicity I have simply taken maximum and minimum values of the inductances actually in the SE 143. The inductance values are conrolled through the use of switches so that there are a number of inductance values lying between the maximum and minimum values which I have assumed. I did not for two reasons use the

intermediate values. In the first place, I did not know them. The instruction book does not give the intermediate values. I would have had to make some rather inaccurate assumptions on the subdivisoin of the inductance by the switch connections, but had I known the inductance values for each switch point I could have easily made the chart a rather large family of curves, one curve corresponding to each subdivision of the inductance then it could be seen that there was no gap; in fact, there would be a considerable overlappping; for example: starting from the bottom up, that the first or series connection with the minimum inductance of 81 microhenries, which runs only to 422 meters, would overlap by quite a number of meters the next highest point—that is, the next switch point, giving a higher value than 81 microhenries, and so on up the scale. Actually, the overlappping would amount to probably 20 degrees on the condenser scale—that is, the wave length [fol. 1844] obtained at, say, 150 or 160 degrees on the condenser scale on one inductance point setting would be the minimum wave length obtained from a zero setting on the condenser on the next inductance setting and so on.

Q. 76. Then may this exhibit be interpreted to mean that with the series connection the range of the receiver is from somewhere around 200 meters?

A. 340, let us say.

Q. 77. Is that the minimum?

A. That is the minimum.

Q. 78. 340 meters up to slightly over 3,000 meters?

A. Yes, that is correct.

Q. 79. With the parallel connection the range of the receiver is from 700 or 800 meters, in that neighborhood?

A. 677.

Q. 80. Up to about 8700 meters?

A. Yes, that is correct.

Q. 81. And that in both instances that range can be continuously covered from one end to the other?

A. Yes.

Mr. Black: I offer in evidence Plaintiff's Exhibits 440 and 441 for Identification. (Received and marked.)

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Q. 82. What other paper have you there?

A. I have here available my work sheet—that is, my computations based on the assumed calibration curve of

the condenser and the SE 143 instruction book values for inductance and antenna.

[fol. 1845] Q. 83. And that is available to defendant's counsel?

A. That is available to anyone.

Q. 84. In Plaintiff's Exhibit 413-B, which is the instruction book for the SE 143 receiver, I note the following statement with reference to connecting the primary variable condenser in parallel with the primary inductance, namely "This combination is very efficient on waves longer than 3,000 meters. On short waves it is not as efficient as the series capacity." Do you agree with that statement? Do you wish to see the statement?

A. With certain reservations, yes. That is, as a general matter, I think I have already said that this matter of efficiency is a complex one. It depends upon a great many factors, both in and outside the receiver, which vary in different installations. But in general I agree with that statement given in the instruction book.

Q. 85. Bearing in mind that the statement was made with respect to the SE 143, are you in a position to agree or not to agree with it?

A. So far as anything in the receiver is concerned, yes. The only reservation which I would have would be with respect to the characteristics of the antenna, which, of course, are not specified, other than by way of capacity.

Q. 86. Noting that the instruction book for that receiver states that the secondary circuit has a wave length range from 250 to 6800 meters, does Plaintiff's Exhibit 440 indicate that the parallel connection is available to cover [fol. 1846] the same range as the secondary covers?

A. It certainly shows this: that in the absence of the parallel connection the wave length range of the secondary as specified could not be covered. That is, with series connection alone you would fall far short of covering the secondary range. So if the receiver were to be used for wave lengths over the entire secondary range it would be necessary to use parallel tuning as well as series tuning.

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[fol. 1847] (Tr. 702)

Q. 87. Referring to Plaintiff's Exhibit 411, which is the instruction book for the type CN 239 receiver, I note that it states that "the secondary circuit has nominal wave

length range of 1,000 to 10,000 meters. The actual range is from about 1,000 to 15,000 meters." Were you familiar with this and similar types of receivers?

A. Yes. And also familiar with that use of nominal and actual wave length range.

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Q. 90. The instruction book just referred to also states that the antenna circuit has a wave length range of from 1,000 to 10,000 meters when used with an antenna of a certain capacity. How can the full actual range of the secondary circuit of that type of receiver be utilized?

A. Either by adding to the antenna capacity with a parallel connection of an antenna condenser or alternatively by adding loading coils to the antenna circuit.

Q. 91. Which arrangement would be preferable from an operating point of view?

A. From an operating point of view the parallel connection with the condenser would be preferable.

Q. 92. And from the point of view of efficiency and selectivity of the receiver which would be preferable?

A. The parallel connection would be in general always preferable. Higher selectivity would be obtained in practically all cases.

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(Tr. 705)

[fol. 1848] Q. 98. One of the advantages of the parallel condenser to which you have referred is the ability to receive long waves under certain conditions. During the period in which we are interested, 1910 to 1919, were there considered to be advantages in the use of long wave for communication as distinguished from the use of short waves?

A. Very distinctly so. This period was before the appreciation and use of short waves for long distance communication. It was a time when this art of radio communication was absolutely dominated by the laws laid down by Marconi, Austen, Cohen, and Alexanderson, which were to the effect that the greater the distance of communication the longer the wave length that should be used. The Austen-Cohen transmission formula was accepted in this period as gospel by all of the radio world so far as I know, and it showed very definitely indeed that if you wished to

[fol. 1849] work over distances of several thousand miles you had to use for optimum transmission wave lengths of the order of 10,000 meters or even more, and that is true today. That is, these laws were not superseded in any way by the introduction of short waves, merely that the short waves were not amenable to the laws of Marconi, Austen, Cohen and others. The laws themselves, however, held for the wave lengths then used.

Q. 99. Lest we get into any misunderstanding as to what "short waves" means, I think you have used it in the sense meaning well below 200 meters, haven't you, in this answer?

A. Yes. That is the division line which I have used through the years.

Q. 100. There are short wave receivers in this case which apparently go down to only about 300 meters. That is the distinction I want you to be sure to bear in mind.

A. In the modern sense, wave lengths of 200 meters or less are short wave lengths, and anything over that is not considered short today. In the period that we have been considering, however, wave lengths of 300, 400, 500 meters were considered short.

Q. 101. I think I interrupted your answer, Mr. Pickard.

A. Well, the laws which I have mentioned being universally accepted at that time and the facts so far as they [fol. 1850] were then known for wave lengths from, say, 300 or 400 meters up to 25,000 meters obeying these laws so far as our measurements showed, it came about as a matter of course that the longer waves were used for the longer distances. Both theory and practice seemed to be in full agreement. Furthermore, the longer waves at that time provided, from an engineering standpoint, an easier application of power. We did not know in the early days how to get any large amount of power into a radio circuit of any sort or how to get it out as radiation at the higher frequency or shorter wave lengths, and certain types of transmitters were actually limited in their performance to the longer waves where any considerable amount of power was required.

The arc transmitter was in that class, invented by Poulson in 1903 or thereabouts.

It was soon found in practice that the amount of power, or better, perhaps, the efficiency of the arc, rapidly fell off as the wave length became shortened. So the arc fell into a division in the transition period between spark and con-

tinuous wave, in which certain transmitters operated from a few hundred meters up to approximately 3,000 meters and arc transmitters took over the work from 3,000 meters up to in some cases 25,000 meters, and all of the transmissions over the longer distances, such, for example, as over the North Atlantic between this country and Europe, was delegated to wave lengths of well over 3,000 meters, and most [fol. 1851] of it was accomplished by arc transmitters.

Q. 102. And as far as regular communication at transatlantic distances was concerned, approximately what wave lengths were in use?

A. Wave lengths in order of 10,000 meters; and when I say "in order of 10,000 meters" I mean waves ranging from something under 10,000 meters to 15,000 or in some cases 20,000 meters.

I recall very clearly indeed, because I made frequent measurements of wave lengths of transmitters in this country and abroad in this period, that the wave lengths in use were such as I have just stated. Nauen, for example, one of the stations that was frequently received in this country and used by us as a sort of test station in this period, operated with approximately 12,000 meters. Other stations in France operated at around 10,000 meters, and the Lafayette station at, I think, approximately 20,000 meters.

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(Tr. 906)

Cross-examination.

By Mr. Edwards:

X Q. 381. As I recall your testimony, it was to the effect that the essential feature of novelty of Claim 16 is the parallel variable condenser and primary coil of the coupling transformer. Am I correct in that?

A. That is correct. That is, the novelty is the use of [fol. 1852] a parallel capacity or condenser across a portion—or, as it has been interpreted later, all of the primary or antenna circuit inductance.

X Q. 382. And it is upon that basis that your estimate of the value to be attributed to Claim 16 is based, is it not?

A. Yes.

X Q. 383. To be somewhat more specific, would you agree that the essential feature of novelty is making the parallel condenser a variable condenser as distinguished from having a condenser in parallel with the coil?

A. No. I should say that the question of variability was not primarily concerned for the reason that tuning could be accomplished by varying the inductance and leaving the condenser fixed.

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(Tr. 911)

A. As a matter of fact, when the number of turns in the coils are changed, the distributed capacity is not in general changed. If I might make that clear, and briefly, what we have in a coil is a distributed capacity consisting of a series of capacities or small condensers existing between adjacent turns. If the coil has 100 turns, there are therefore 100 turning capacities in series between the whole 100 turns in the coil. If now, by switch or other arrangement, we cut down the number to, say, 50, we have changed the series of small condensers to one-half the former value and we [fol. 1853] have increased that part of the capacity two-fold. We have also changed the distributed capacity due to the condensers by the capacity not merely from turn to turn but from turns that are separated by several turns from each other. It is really a very complicated matter, and it often, as a matter of measurement, comes down to this: that the distributed capacity of a coil remains approximately the same with a considerable change in the number of turns, although the general effect is to increase the distributed capacity when we decrease the number of turns.

X Q. 393. Let us, for simplicity, assume that the coil is a fixed coil. Do you agree that one skilled in the art could adjust the coil so as to have the proper number of turns and proper distributed capacity so as to tune the antenna to any given frequency?

The Commissioner: One skilled in the art as of what time?

Mr. Edwards: As of 1910.

A. Yes. As of 1910 one skilled in the art would know and would be able to adjust the inductance of the coil by changing the number of turns or in other ways, and he would also

have known that he was changing, to some extent, the distributed capacity of the coil.

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[fol. 1854] (Tr. 912)

X Q. 394. And as a result he would be able to tune the antenna to any given frequency?

A. As the result principally of the change of inductance of the coil, yes. Within the limits of the inductance range of the coil and the capacity of the antenna and secondarily the distributed capacity of the coil, he would be able to change within those limits the tune of the circuit.

X Q. 395. And one skilled in the art at that date could have devised a series of coils which could be used one at a time to give a selected frequency, a selected number of frequencies to which the antenna would be tuned, could he not?

A. Yes, that is quite correct.

X Q. 396. If he did use such a fixed coil he would of necessity have a capacity in parallel with the primary coil of the coupling transformer, would he not?

A. He would have such a capacity—that is, distributed capacity of the coil.

X Q. 397. And that would be in parallel with the inductance, would it not?

A. In a general sense, yes; bearing in mind, however, that distributed capacity differs materially from a lumped capacity such as is given by a condenser for the very reason that it is distributed over the entire coil, and not concen-[fol. 1855] trated across the terminals of the coil. There is quite a difference there.

X Q. 398. But would it not be the fact that the designer would have tuned his antenna to a given frequency by the use of a capacity in parallel with the primary of his coupling transformer?

A. Theoretically, yes; practically, no; because the distributed capacity is in general very, very small as compared with the capacity of any antenna which would be used.

X Q. 399. In point of fact, such coils were used to tune the antenna, were they not?

A. Principally through their inductance they were used to tune. I do not know of any deliberate use that was made of the distributed capacity of the coil.

By the Commissioner:

X Q. 400. How were the usual tuning inductance coils made at that time?

A. The usual type of coil was a single-layer coil for the shorter wave lengths; and referring now to such a receiver as the IP 76 and considering the coupling coil—that is, in Exhibit 87 the coil marked J′—that consisted of a single layer of 100 turns of wire wound on a form approximately $3\frac{1}{2}$ or 4 inches in diameter and about the same length.

[fol. 1856] X Q. 401. Were the turns spaced or close together?

A. The turns were slightly spaced apart. The forms were generally threaded—that is, a worm screw thread was cut in the hard rubber and later bakelite and the wire wound in that. Actually, the turns were separated by something less than the wire diameter, and in all cases the turns were separated by the thickness of the insulation—or, rather, by the thickness of the insulation on a single conductor. That would be regardless of whether the form were threaded or not.

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[fol. 1857] (Tr. 916)

X Q. 404. So it was advantageous, then, to space the coils?

A. Yes.

X Q. 405. In order to decrease the distributed capacity?

A. Yes. There were really three reasons for it. First, the uniformity of inductance of a series of coils made up for different receivers; second, the matter of losses, which were due to the matter of spacing. The closer the spacing the greater the loss.

By Mr. Edwards:

X Q. 406. Is it not true that the bringing of coils closer together reduced the capacity area of the coil and spreading it apart tended to increase the area, up to a certain limit?

A. No. The capacity between any pair of conductors is a simple function of the area exposed by each conductor to the other and the separation—that is, if we have two conductors which may be wire turns 1/100th of an inch apart, the capacity may be one-tenth of a microfarad. If we double that spacing and place them two one-hundredths

of an inch apart, approximately, the capacity between those two wires has been cut in half. I say "approximately" because there are fringe or edge effects which are not amenable to a simple formula.

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[fol. 1858] (Tr. 920)

By the Commissioner:

X Q. 421. Let us assume a primary circuit comprising an antenna and a tuning inductance inserted between the antenna and ground. Let us now make a hypothetical assumption, which I understand can be only one—namely, that the tuning inductance has no distributed capacity whatever but is still capable of inductive adjustment or change of inductance. Would that be capable of tuning to different frequencies?

A. Yes, your Honor; that would be capable of tuning to different frequencies, and would be, as a matter of fact, the ideal case, the one toward which every constructor has striven, but not quite attained.

X Q. 422. And that would have both capacity and the inductance?

A. Yes. It would have the capacity of the antenna and the pure inductance without any distributed capacity of the coil, and it would be capable of tuning to any desired wave length within the range of the inductance variation given by the coil.

X Q. 423. Let us assume a second primary receiving circuit also having an antenna connected through ground by means of a tuning inductance but this time the tuning inductance possessing its distributed capacity. That is, as I understand it, also capable of being tuned to different [fol. 1859] frequencies?

A. Yes. That is the actual case. That is the practical case, because, of course, all coils have some distributed capacity.

X Q. 424. I think you have already answered the third question in my mind; but for the sake of the record I will ask it: which of those two systems, the first or second one, which I referred to, is the more efficient?

A. The first is the more efficient, your Honor, for the reason that distributed capacity in the coil, just because it is distributed, increases the losses in the coil, and there-

fore impairs the efficiency, and, therefore, the selectivity, of the system. It is, however, a secondary effect, and I would hate to leave this matter without just one sentence on the magnitude of the effect. We have seen in some of the exhibits in this case, such as the instruction book for one of the receivers, SE 143, a specified capacity for an antenna, namely, .0008 microfarad. The distributed capacity of the coil on an ordinary receiver primary is less than one-hundredth of that value. Its effect, therefore, is very secondary, although it is present, and does have an effect.

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[fol. 1860] By the Commissioner: (Tr. 925)

X Q. 437. Did you know at that time, in 1910, whether distributed capacity in an inductance coil was a detriment or a benefit?

A. I knew, your Honor, very definitely, from measurements, that it was a detriment, and that was the subject of frequent discussion among engineers at that time. It abundantly appears in lectures on the art as of 1910-1914, particularly.

By Mr. Edwards:

X Q. 438. The detriment was in what respect?

A. Efficiency; because the effect of the distributed capacity, just because it is distributed, lowered efficiency. It introduced additional loss in the circuit and so affected not only the transfer of energy from coil to coil or from antenna system to space, but also, of course, the selectivity or the sharpness of tuning which could be accomplished.

X Q. 439. In view of your statement, would you now agree with the basis upon which value should be attributed to Claim 16 of the Marconi patent as the basis of distributed capacity as against a lump capacity or a variable capacity as against a fixed capacity?

A. I do not know that the matter of distributed capacity had entered my mind before in this connection, but I should say that the value of the apparatus and [fol. 1861] arrangement described and covered by Claim 16 of the Marconi patent lay primarily in parallel capacity tuning of a circuit and was in part independent of whether or no that capacity were variable; but admitting that variability of the condenser is a distinct advantage, that, of

course, makes the apparatus more valuable. I should have difficulty in answering the question as a whole, taking into account distributed capacity, because I am not aware of any system of radio telegraphy in which distributed capacity was used as a method of tuning.

[fol. 1862] By the Commissioner: (Tr. 929)

X Q. 441. Every antenna circuit or every primary circuit which has an antenna and a tuning inductance in connection to ground possesses inductance and possesses capacity, and that capacity is of two kinds or characteristics: first, the capacity of the antenna with respect to ground, and, second, the distributed capacity of the inductance coil: is that correct?

A. That is correct, your Honor.

X Q. 442. Second: am I correct in understanding that any distributed capacity whatsoever in the coil, in the inductance coil, is a detriment?

A. That is also correct, so far as it has an effect; and I mention that because its magnitude is so small as compared with the average antenna that for many purposes it can be neglected. So far as it is present at all, its effect is always detrimental. I know of no exception to that.

[fol. 1863] PICKARD—REDIRECT (Tr. 941)

R. D. Q. 471. Will you please look at the Marconi patent 763772 and point out whether or not it discloses that the parallel condenser h of Figure 2 is a variable condenser?

A. I find that the parallel condenser h is described on page 2, lines 83 to 86, as "preferably similar in construction and operation to the condenser e," and referring to the description of condenser e, which appears on page 1, lines 88 to 92, I find that this condenser e is "preferably one provided with two telescoping metallic tubes separated by a dielectric and arranged to readily vary the capacity by being slid upon each other." So as the condenser h is described by the Marconi patent as being similar in construction to the condenser e, it is a variable condenser.

R. D. Q. 472. Have you in mind what we have referred to as Groups I and II of the defendant's receivers in this case?

A. Yes.

R. D. Q. 473. And I believe that you have testified to familiarity with at least a number of types of those receivers?

A. Yes.

R. D. Q. 474. In so far as you are familiar with them are there wave length ranges within the stated ranges of those receivers that could be covered only by the parallel condenser connection?

[fol. 1864] A. That is correct. The secondary tuning range in those receivers is such that with the coils supplied in the primary circuit tuning over the entire range could be accomplished only by the use of parallel tuning.

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R. D. Q. 476. To one skilled in the art as of 1910 or thereabouts, did the term "condenser" mean distributed capacity as well as capacity lumped in a piece of apparatus?

A. No. The term "condenser" as of that time meant a discrete piece of apparatus,—that is, a lumped capacity. Whenever we referred to other capacities, such as those existing in coils and in various systems of wiring, we did not use the word "condenser," but referred to them as "distributed capacity."

R. D. Q. 477. You have spoken of the distributed capacity in parallel with the coil,—that is, the distributed capacity of the coil,—as decreasing efficiency; is that correct?

A. That is correct.

R. D. Q. 478. Does a condenser connected in parallel with the coil also decrease efficiency?

A. No, it does not.

[fol. 1865] R. D. Q. 479. Will you please explain that?

A. There are two reasons: one reason is that distributed capacity is not a capacity connected across the terminals of the coil. It corresponds really to an infinite series of small condensers connected across various and all portions of the coil; secondly, the dielectric involved in distributed capacity effects is, of course, the insulation, whatever it might be, of the coil itself. In the case of a receiving coil, this insulation is a more or less imperfect dielectric, such, for example, as silk or cotton impregnated, very often, with shellac or a similar material; whereas in the case of the normally used variable condenser, the dielectric is air, a very perfect dielectric, in which there

is no loss at ordinary receiving circuit potentials; so for two reasons the loss is much greater in the case of the distributed capacity than it is in the case of a lumped capacity connected across the coil. I might add to that that any ordinary variable air condenser of practically any period in the art was a far better device from the standpoint of losses than any form of coil which could be devised. The condénser first and last was a very efficient piece of apparatus, the coil relatively much less efficient.
[fol. 1866] R. D. Q. 480. So within your knowledge has the coil or coils of an antenna circuit been designed for the purpose of tuning that circuit by means of the distributed capacity of the coil?

A. No, I know of no case in which that has been done.

R. D. Q. 481. From your experience would it be practical to do that?

A. No, because it would be physically impossible to cover the normal desired wave length ranges, owing to the small amount of capacity existing as distributed capacity in any practicable size of coil. It is very, very small as compared with the antenna capacity, generally of the order of less than one per cent. of the antenna capacity.

R. D. Q. 482. I do not know whether this question is too speculative or not, but, if so, you can tell me: if an antenna circuit were designed to be tuned with a series of coils having the same inductance but different capacities and those coils were of reasonable size, how broad a range could be covered?

A. The size of the coil, as I think I have indicated before, is rather definitely limited. We cannot have indefinitely large coils, and therefore indefinitely large distributed capacities; because finally the coil shrinks to a single turn of wire if it gets large enough, and then, when that turn [fol. 1867] attains a sufficient diameter, the inductance increases beyond the desired value.

Within any practical range of coils—and by that I mean coils ranging, say, from an inch or two in diameter up to a foot or two in diameter, or something of that sort, in which the cubic contents or volume increased over quite a large range indeed, the distributed capacities would range from a few millionths of a microfarad—one or two millionths of à microfarad, for the smaller coils,—up to perhaps three, four, or even five times that value, and the effect upon wave length tuning of any circuit would be quite

closely the square root of that. That is at the most; and assuming, as I have assumed several times before, an antenna capacity of .0008 microfarad, or, in the more frequently used units (micro-microfarads, or millionths of a microfarad) of 800 micro-microfarads, we would then have a circuit capacity ranging from, say, 803 micro-microfarads to, say, 815 micro-microfarads, and that is a change in capacity of 12 parts in 800, and that is one and one-half per cent. The change in wave length would be quite closely the square root of the capacity change, or approximately .75 per cent.; that is, less than one per cent.

For example, if with the smallest coil the circuit was tuned to 3,000 meters, it could be changed by going through the larger coil to not over 3030 meters.

[fol. 1868] R. D. Q. 483. In one question on cross-examination there was reference to the difference between a condenser and distributed capacity as depending upon the structure of the condenser and the structure of the coil.

In a practical receiver, and referring to the antenna circuit, still, are there differences in results between utilizing a condenser for tuning and utilizing a distributed capacity for tuning?

A. Yes. Entirely aside from the very considerable difference in range or wave length range which might be accomplished by the two methods, there is a difference in efficiency. The efficiency—that is, the selectivity of the circuit—would be greater with the condenser than with the use, so far as it could be made to function, of the distributed capacity as a tuning element.

R. D. Q. 484. In the receivers of those periods it was not unusual, was it, to provide different coils for tuning purposes?

A. Yes; either different coils or coils with taps of one form or another, which is more or less the same thing.

R. D. Q. 485. And what was the electrical purpose of providing either one of those arrangements?

A. To subdivide the inductance into steps so that by the use of some other variable element, such, for example, as [fol. 1869] a variable condenser in series or in parallel with the circuit, one could continuously vary the tune of the circuit—that is, by infinitely small steps over an appreciable range. Then the number of steps would be so proportioned that a slightly overlapping series of wave ranges would be

obtained. That was the object of the subdivision. In some special cases, the inductances were subdivided quite finely as particularly in the IP 76 receivers.

R. D. Q. 486. What was the effect on the constants of the circuits of changing coils or changing connection to coils? What constant was affected primarily?

A. Primarily the inductance.

R. D. Q. 487. A change in the inductance had some effect on changes in the wave length, did it not?

A. Yes. The wave length was directly related to the change of inductance; or, rather, related as the square root of the change of inductance. It was still a direct relation, though.

R. D. Q. 488. And in any normal antenna circuit was the larger portion of the inductance of that circuit embodied in the coil?

A. Yes; with the exception of a very long wire antenna and a very small antenna loading inductance such as might be used at the very bottom of the wave length range of a receiver. In that case there would be an appreciable amount [fol. 1870] of the inductance in the circuit of the antenna. Usually by far the greater part of the inductance was concentrated in the inductance of the receiver.

[fol. 1871] LESTER L. JONES (Tr. 1264)

LESTER L. JONES, a witness produced on behalf of the defendant, having been first duly sworn by said Commissioner, was examined, and in answer to interrogatories, testified as follows:

Direct examination.

By Mr. Houghton:

Q. 1. Mr. Jones, will you state your full name, age, residence and occupation?

A. Lester L. Jones, 455 Summit Avenue, Oradell, New Jersey; age, 47; engineer and executive.

Q. 2. Were you ever in the United States Navy?

A. I was in the navy as a civilian employee.

Q. 3. When did this employment begin?

A. I first started to work in the Brooklyn Navy Yard in December, 1913, and continued at the Brooklyn Navy

Yard until the end of February, 1917, at which time I was transferred to the Washington Navy Yard, and I continued there until March or April, 1918.

Q. 4. During this entire period of employment by the navy, were you employed under your present name or under a former name, prior to its judicial change?

A. Yes, I was employed under the name of Lester L. Israel.

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[fol. 1872] (Tr. 1270)

Q. 19. Were you the Lester L. Israel whose name appeared on the instruction book prepared for the SE 143?

A. Yes, sir.

Q. 20. Were you in any way connected with the preparation of that book, other than signing it?

A. My best recollection now is that I wrote at least portions of the book. I may have written it all. I cannot remember definitely. I certainly read and checked everything before I signed my name to it. I remember that I had quite full discussions with everybody available as to what should go in and what should be left out.

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(Tr. 1274)

Q. 28. Referring again to the SE 143 and the statement in the instruction book regarding the possibility of placing the primary condenser parallel with the primary coil, what condition would this change meet, or what change of condition would this change of position meet?

A. That would be for an unusual or extraordinary use of the receiver. It would not be an arrangement for regular use of the receiver, because I clearly recollect that we put into the receiver everything that was required for the regular and ordinary use. I also recollect that the statements in the instruction book relating to the outside connections of switching apparatus for arranging the antenna [fol. 1873] tuning condenser in parallel with the coil were desired by Lieut.-Commander Eaton as being desirable for two purposes. More specifically, in the case of regular antennae, where it had been shot away, and it was necessary to operate with a single short wire. In this latter

case, the wave-length range of the receiver would have been seriously diminished, and it was desirable to have the operators know that it could be extended to a longer wave-length range on any kind of antenna, by using the parallel connection.

There is a statement there that this form of connection is not as efficient, and this was intended to discourage the use of such a connection in regular operation.

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(Tr. 1277)

Q. 32. Referring to Plaintiff's Exhibit 413-B in the instruction book for the SE 143, sheet 1, first paragraph below the figure 1, it states:

"This places the primary variable condenser in parallel with the primary inductance. This combination is very efficient on waves longer than 3000 meters; on short waves it is not as efficient as the series capacitive."

Do you agree with that statement?

A. Yes.

Q. 33. Will you explain why you agree with that statement?

[fol. 1874] (Tr. 1278

A. Yes. I agree with the statement for these reasons on the long waves, the paralleling of the condenser forms an efficient arrangement, because the radiation resistance, or total resistance, of the antenna on ships is usually very small. The distributed inductance of the antenna is also negligible. The coils of this receiver were of stranded wire, and were very efficient; that is, they had low resistance values on the long waves.

In consequence of all these things, the primary circuit with the tuning condenser arranged in parallel, would oscillate as a single resonance circuit, having low damping; and such a system would be easier to manipulate than a system in which the loading effect were obtained by a separate loading coil inserted in series with the antenna and the tuning condenser.

On the shorter wave-lengths, the parallel tuning arrangement would be less efficient and generally undesirable, for these reasons: the antenna system on short waves generally

has a high resistance. The parallel condenser arrangement would minimize the inductance necessary for tuning, and would give to the antenna a broad resonance curve, and make it less selective.

And also the usual long lengths of antenna lead-in wires, and the long antennae themselves, would have considerable distributed inductance, so that the parallel arrangement would create a set of coupled circuits in the antenna, which [fol. 1875] would not resonate to a single frequency at any one setting and would give rise to additional interference, due to coupling waves being generated in the antenna system.

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[fol. 1876] Jennings B. Dow (Lieut.-Commander, U. S. Navy, Washington, D. C.) a witness produced on behalf of the defendant, having been first duly sworn by said Commissioner, was examined, and in answer to interrogatories testified as follows:

Direct examination.

By Mr. Edwards:

Q. 1. You reside where?

A. I reside in Washington, D. C.

Q. 2. Will you state what training and experience you have had in the line of radio engineering?

A. I graduated from the U. S. Naval Academy early in June, 1919. From there I was ordered to sea duty as radio officer of the U. S. S. Montana, and in 1920 I was ordered to duty as Assistant Radio Officer of the U. S. S. Texas. In or about 1921 I was ordered to duty as radio officer of the U. S. S. California; and about 1923 I was ordered to duty on the staff of Vice-Admiral Wiley as a communications and radio officer.

During the above period, subsequent to my graduation from the Naval Academy, my duties involved the supervision of operation and maintenance of radio installations of the vessels which I mentioned; and, during the period of my office on the staff of the Vice-Admiral, Wiley, I was charged, in addition, with the supervision of the operation and maintenance of the radio installations of all battleships of the battle fleet.

[fol. 1877] Q. 2a. What types or classes of vessels were the Montana, the Texas, and the California?

A. The Montana was an armored cruiser. The Texas was a battleship, and the California was a battleship.

Q. 3. Now will you continue your answer?

A. About 1924 I was ordered to duty at the Post Graduate School at Annapolis for post graduate training in communications engineering. I devoted a year to this work and the following year I continued my duties in the graduate School of Engineering at Harvard University.

I hold the degrees of Bachelor of Science and Master of Science and Communications Engineering. Upon the completion of my studies at Harvard——

Q. 4. What year?

A. In the summer of 1926, I was ordered to duty on the staff of Admiral Williams, who was Commander-in-Chief of the Asiatic Fleet, where I served during the tenure of his office, as radio officer in the Asiatic Fleet, for a period serving both as communications and radio officer of the Asiatic Fleet.

Upon the detachment of Admiral Williams, I was ordered to duty as radio material officer of the Sixteenth Naval District, with headquarters at the Navy Yard Cavite, Philippine Islands.

[fol. 1878] Q. 5. When was that?

A. I completed eighteen months of duty as radio material officer at that station; and, in the summer of 1929, I was ordered back to the United States for duty in the Research and Design section of the Radio Division of the Bureau of Engineering, during which duty I was charged with the preparation of specifications for Navy radio equipment and with the administration of research and development problems at the Naval Research Laboratory.

I was detached from the Bureau of Engineering in the spring of 1932, and was ordered to duty as radio officer of the Radio Control Battleship Utah, as I had followed the development and design of the equipment for that vessel, as a part of my duty in the Bureau.

I served on the Utah in the capacity of radio officer until the fall of 1934, when I was ordered again to duty as assistant to the officer in charge of the Radio Division, Bureau of Engineering.

In June, 1938 I relieved the officer in charge of the Radio Division of the Bureau of Engineering, and I have served in that capacity up to the present time.

The Radio Division of the Bureau of Engineering, of which I am the head, is charged with the design, development, procurement, testing, and installation of all radio equipment used by the Naval Service, including equipment for ships, shore radio stations, and air-craft.

[fol. 1879] Q. 6. Have you read Patent No. 763,772, of June 28, 1904, to G. Marconi?

A. I have.

Q. 7. And do you believe that you understand the description of the construction and operation of the device therein shown?

A. I do.

Q. 8. I call your attention to Figure 2 of the patent and to the condenser marked "h" in that figure. This condenser, you will note, is in shunt with the primary coil J′ of the coupling transformer. Will you state if you find anything in the specification directing the reader to any special function of that condenser, and whether or not you find any such direction in the specification, will you explain how that condenser operates?

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(Tr. 1351)

A. Oscillations transmitted by the equipment of Figure 1 are intercepted by the antenna F′ of Figure 2, and, as stated, beginning with line 118 on page 2 of the specifications, the open circuit of the receiver of Figure 2, comprising the antenna F′, the loading coil G′, the condenser h, and the coil J′, are tuned so as to be responsive to the wave signal energy.

[fol. 1880] By the arrangement shown in Figure 2, energy is coupled from the primary coil J′ to the secondary coil J″ and causes the secondary circuit, including the detector, to respond to the received signal in such a way that it can be detected.

I have analyzed the specification and the various figures of the drawing and have come to the conclusion, based upon facts which I will mention, that the condenser h acts as a series condenser in the open circuit or antenna circuit of the receiver, and that the coil J′ is in fact a coupling coil. By that I mean that, from the matter which is disclosed

in the specification, the condenser H and the coil J′ are not made resonant to the received signal.

The Commissioner: By that do you mean that the condenser h does not have any function in tuning?

The Witness: The condenser h serves to tune the open circuit to the wavelength of the incoming signal. That is, the condenser h, in combination with the loading coil G′ and the capacity F′ of the antenna—that is, the capacity between the antenna and ground—is adjusted to resonate at the wavelength of the incoming signal.

When that adjustment is made, a maximum voltage exists across the condenser h, which causes the current to flow in the coil j′ and this is coupled to the secondary coil J″, [fol. 1881] so that the received signals can be detected.

In answering your question more completely, your Honor, I refer you to the tables on page 4 of the specification. The upper table discloses the adjustments of the equipment at the transmitting station for various tunes, which are labeled in the left-hand column.

[fol. 1882] Similarly, the table captioned "Receiving Station" shows various adjustments of the receiving system for the various tunes labeled in the left-hand column.

The tunes under the receiving station column correspond to those under the transmitting station column. It is possible to compare these tables readily, where only one variable is involved. That is, it would be difficult to compare tunes wherein two adjustments were changed.

However, referring to the table headed "Receiving Station" and to tunes 3 and 4, under column "h" is shown the value of the capacity h for tunes 3 and 4.

You will note that the condenser h was not changed in shifting from tune 3 to tune 4. It is important to note that tunes 3 to 4 have, point for point, widely-separated wave lengths; since, in the column g′ of the lower table, you will note that only 21 turns of inductions were used in the loading coil G′ of the receiver for tune 3; whereas 100 turns were used for tune 4.

It is important to note at this time that the transformer, or that the primary coil J′ of the transformer, comprising this coil and the secondary coil J″, was not changed. And, since neither the capacity of the condenser h nor the in- [fol. 1883] ductances of the primary coil J′ were changed, the circuit consisting of the condenser H and the primary coil J′, even if it were resonant, for example, to tune 3, could

not be resonant to tune 4, since the wave length was changed and yet the adjustments of this circuit were not changed.

That there is a material difference in the wave length for tunes 3 and 4 is also borne out by the fact that, in the table headed "Transmitting Station," for these tunes, Transformer D-D′ was the same in both cases; that is, the primary coil D of Figure 1 had the same inductance for both of these tunes.

However, the capacity of the condenser e in the case of tune 3 was 0.004112 microfarads. In the case of tune No. 4 it was over four times as great.

As the inductance was the same in both cases, and the capacity was four times as great in tune 4 as it was in tune 3, and since the wavelength varies as the square root of the product of the inductance times the capacity, then the wavelength in the case of tune No. 4 must have been substantially twice as great as in the case of tune No. 3.

The Commissioner: Looking at it from the practical point of view entirely, would you not have to adjust the condenser h in Figure 2 to receive the two different waves?

[fol. 1884] The Witness: That is not necessary, your Honor. In a resonance circuit which may be adjusted, from which it is desired to adjust to several wavelengths, either the capacitance or the inductance or both may be varied to change the resonant wavelength.

Marconi varies his resonance wavelength by changing his loading coil G′ rather than the condenser h. And this is borne out by one or more parts of the specification which I will read. Beginning with line 56 on page 1 of the specification, I quote the following:

[fol. 1885] "The system also requires, as essential elements thereof, the inclusion in the lines (at both stations) from the aerial conductor to the earth of variable inductances, and the use at both stations of means for varying or adjusting the inductance of the two circuits at each station to accord with each other. By this arrangement of apparatus, I am able to secure a perfect 'tuning' of the apparatus at a transmitting station and at one or more of a number of receiving stations."

That is, Marconi prefers to accomplish this result by the adjustment of his loading inductance G′ in his receiver of Figure 2.

The Commissioner: And he did not need to make the condenser h adjustable, then.

By Mr. Edwards:

Q. 9. Whether he did or not, will you look at the table on page 4 and say whether, as between tunes 3 and 4, there is any indication of variation of the condenser for those tunes.

A. In the lower table on page 4, you will note that tunes 3 and 4 which operated at wavelengths varying in the ratio of substantially 2 to 1, the condenser h was identical in both cases.

Q. 10. Had the same value?

A. Yes, had the same value—namely, 0.0046 microfarads in both cases.

[fol. 1886] In reading the specification, I find the statement, beginning with line 83 on page 2 of the specification:

"In a shunt around said primary J', I usually place a condenser h, preferably similar in construction and operation to the condenser e."

Now, he indicates that that condenser might be variable; but I can find, in no place in the specification, anything to indicate that he actually used that, that he actually varied that to change his tunes.

The Commissioner: What would you consider as his preferred form with respect to condenser h—a variable or non-variable condenser?

Q. 11. Do you find anything in the specification indicating that?

A. Yes. It is very difficult to look at this circuit now, in the light of what we have learned over the past forty years. For another reason, which I will explain, he had no reason for varying the value of condenser h in making the circuit operate as he described it.

The Commissioner: I do not believe that quite answers my question. I will ask the reporter to read it to you.

(The last question of the Commissioner was read by the reporter as above recorded.)

[fol. 1887] The Witness: I would say that the preferred form would be the variable condenser.

Q. 12. Do you find any reference in the specification to the condenser h or its functions, other than the matter on page 2, lines 83 to 86, and in the table on page 4?

The Witness: Will you read that to me, please?

(Q. 12 was read by the reporter as above recorded.)

A. I find no other reference to the condenser h. However, beginning with line 12 on page 3 of the specification, the following statement appears:

"The adjustment of the self-induction and capacity of any or all of the four circuits can be made in any convenient manner and employing various arrangements of apparatus, those shown and described herein being preferred."

By the Commissioner:

Q. 13. Does the sentence beginning on page 1, line 88, help you in any way in your answer?

A. In the beginning of line 88 appears a statement describing the condenser e, which reads:

"A condenser e, preferably one provided with two telescoping metallic tubes separated by a dielectric and arranged to readily vary the capacity by being slid upon each other, is included in one connection from the induction coil to the transformer winding d."

[fol. 1888] That, when used in conjunction with his statement beginning with line 83 on page 2 of the specification, namely, "In a shunt around said primary j', I usually place a condenser h, preferably similar in construction and operation to the condenser e," indicates a construction of the condenser h, although the references to the tuning of the circuit indicate that the actual tuning adjustment was made by the coil g' of the receiver.

Q. 14. I wonder why Marconi wanted an adjustable condenser there, then?

A. I can readily understand why he might want an adjustable condenser there, if he tuned his circuit, consisting of the condenser h and the coil j' to the resonant wavelength; but, for the reason which I pointed out, using the table on page 4, he does not appear to tune that circuit.

Now, in going over this patent, that fact was also confirmed fro . another angle, which I can go into at this time if you desire.

By Mr. Edwards:

Q. 15. Suppose, before you do go into that, that you let me ask you again: do you find any statement anywhere in the patent as to any specific function that the condenser h is to perform.

A. Only by implication from the matter appearing in lines 84 to 86 of page 2, and the matter beginning with lines 88 to 92 on page 1.

[fol. 1889] Now, if he uses his condenser for tuning, as suggested, in the manner—

Q. 16. Now, before you go into that, I would like an answer to my question: do you find any specific statement in the patent that that condenser performs any specific function?

A. I do not.

Q. 17. Now, then, it is a fact or not that what ever function may be performed by the condenser would depend upon the value of the condenser in any particular circuit?

A. That is correct.

Q. 18. Is it or is it not true that it is purely a matter of speculation at the present time as to what particular function may have been intended by that condenser?

A. It is. However, from studying this specification in particular in connection with what I pointed out in connection with the tables on page 4, and in looking at it also from another angle based upon what appears in the specification, I conclude that the condenser h was used as a part of the resonant system, consisting of the antenna f', the coil g', the condenser h, and the ground, as the open circuit of the receiver.

Q. 19. Yes. Now, will you explain in what manner you find confirmation of that belief?

A. I will try to make this as—since it involves consideration of several parts of the specification, I shall have to go slowly in covering this.

[fol. 1890] Q. 20. Yes. Well, take it slowly, step by step, in order to enable the Court to follow your line of reasoning.

A. Beginning with line 118 on page 2, Marconi states that:

"The capacity and self-induction of the four circuits—i. e., the primary and secondary circuits at the transmitting station and the primary and secondary circuits at any one of the receiving stations in a communicating system—are each and all to be so independently adjusted as to make the product of the self-induction multiples by the capacity the same in each case or multiplies of each other—that is to say, the electrical time periods of the four circuits are to be the same or octaves of each other."

This means, as I have stated before, that the primary circuit of the transmitter, the secondary or open circuit of the transmitter, the primary or open circuit of the receiver, and the secondary or closed circuit of the receiver, are all adjusted substantially to the same wave length.

If we examine the values of the condenser e, the primary coil d, in Figure 1, we can obtain certain information which can be used for comparison purposes, to determine the relative values of wave length to which these circuits are adjusted in both transmitter and receiver.

[fol. 1891] We have a further means of comparing the wave-lengths by the table on page 4. In order to be specific, I would like to compare the transmitting tune No. 3 with the adjustment or with the resonant wavelength to which the circuit h-j' in Figure 2 is adjusted.

Tune 3 in the upper table corresponds to tune 3 in the lower table, and it will be noted that the capacity of condenser e in figure 1 was 0.004112 microfarads. This is substantially the same capacity as that of condenser h in figure 2, which, as listed opposite tune No. 3 in the receiving station table, is 0.0046 microfarads.

The primary coil d of Figure 1, which, in conjunction with the condenser e, determines the resonance wavelength of the primary circuit, and, accordingly, the transmitted wavelength, is described beginning with line 36 on page 3, which states:

"A transformer in all essential respects similar to 1, but with a primary of 1.93 meters, and the core or block on which both primary and secondary are wound, is .3048 meters wide."

It will be noted at this time that the three transformers corresponding to those listed in column d-d' of the trans-

mitting station table, are described beginning with line 26 on page 3, through line 46.

The coil d thus consists of a piece of wire 1.93 meters in [fol. 1892] length, wound on a core block .3048 meters wide. This would represent roughly one and one-half turns of wire.

We have now a description of the primary coil d. We also know that the condensers e and h are of substantially the same capacity.

If we now can determine a relative value of the coil j′ in Figure 2, we can get a pretty good picture of the resonance wavelength of the two circuits, namely, the primary circuit in the transmitter, which determines the wavelength of the communication and the value of the resonant wavelength of circuit h-j′.

Four different kinds of induction coils or transformers were used or are described by Marconi for use in the receiver. The description of these begins with line 59 on page 3 of the specification and continues through line 127 on page 3.

Four different types of induction coils corresponding to the four different types listed in the second column from the left of the table on page 4, under receiving station, are described. His induction coil No. 2 which was used in type No. 3 is described beginning with line 85 on page 3.

[fol. 1893] This description states:

"The form of induction coil shown in Figure 6 has a primary of 100 turns of copper wire .037 centimeters in diameter, wound on a core j (2.9 centimeters in diameter).

It will be noted that the diameter of the core is 2.9 centimeters, so that the circumference of the length of one turn would be approximately 10 centimeters. A hundred turns of this wire would be 1,000 centimeters, or 10 meters. That is, the length of the wire used in the coil j′ is 10 meters, in contrast with 1.93 meters of wire used in coil d of the transmitter.

The inductance of coil j′ of the receiver can be said to be several times greater than that of the coil d in the transmitter; and, since the condensers e and h are substantially the same, what I have said is further proof that the circuit in the receiver, consisting of the coil h and the in-

ductance j′ was not tuned to the wavelength of the received signal.

This confirms the matter which I mentioned some time ago, this morning, in connection with tunes 3 and 4, wherein the wavelength of the transmitted signal was changed; yet neither condenser h nor the coil j′ were changed to make the circuit h-j′ resonant.

[fol. 1894] This, all of which I have said, leads me to conclude that the condenser h was in fact used merely as a series capacity in the antenna or open circuit of the receiver, and that the coil j′ is merely a coupling coil by which energy can be conducted from the primary or open circuit of the receiver to the second circuit.

* * * * * * *

(Tr. 1368)

Q. 28. Is it true or not that, in effect, the total distributive capacity is in series with the inductance of the various antenna and coils?

A. That is correct. I think perhaps your Honor's suggestion off the record that we take a recess was a good one. I see further complications ahead.

* * * * * * *

A. (Continuing:) In answering the previous question, in showing the operation of a circuit consisting of a condenser h, a coil such as j′, I would like to simplify the apparatus under consideration, to the extent of providing an antenna connected to a parallel circuit consisting of a condenser and an inductance coil, the other side of which circuit is connected to the earth.

I have sketched such a circuit, and will assume that the wave length of the transmitter wave is such that the condenser, when it is in its mid-position, would resonate the coil to the transmitted wavelength.

[fol. 1895] Under these conditions, and starting out with the condenser in its zero position——

The Commissioner: Can I interrupt you to ask what you mean by "resonate the coil"?

The Witness: By that I mean: when the capacity reactance of the condenser plus certain distributive capacities which may be lumped with that condenser, is equal to the inductive reactance of the coil, the circuit is said to be in resonance.

Starting out with the condenser in the zero position, the circuit would consist merely of the antenna, having in series connection with it to the earth the induction coil.

Q. 29. That is the coil j'?

A. The coil j'.

Q. 30. Would you not make your circuit more complete by putting in there your loading coil and your antenna?

Mr. Blackmar: As I understand it, the witness just said he simplified the circuit by omitting them.

The Commissioner: I think that is correct. The witness, Mr. Edwards, started to simplify the circuit, omitting the loading coil.

Mr. Edwards: Oh, yes; that is all right.

A. The circuit—since we assumed that the resonant wavelength corresponded to that of the circuit h-j', when [fol. 1896] the condenser was in its mid-position, very little current would go through the coil j'. However, all of the current which would be induced into the antenna system by the transmitting station would flow through the coil j'.

The Commissioner: That is, with the condenser at zero position?

The Witness: That is correct. And, the coil j' would present an inductive reactance to the incoming signal or to currents of the incoming signal wavelength. Now, as we begin to increase the capacity of the condenser h, the inductive reactance of the coil begins to be neutralized. At the same time, more and more current begins to flow through the condenser h. As the resonant wavelength of the transmitting station is approached, when the condenser is in the mid-position, the same current flows in the condenser as in the coil; and, when the coil and condenser reactances neutralize one another, the reactance of the circuit between the antenna and the ground becomes resistive, rather than capacitive or inductive.

At the resonant wave length, the maximum current possible under the conditions will flow in the circuit, consisting of the condenser h and the coil j'.

Mr. Edwards: Will you read that last sentence to me please?

(Portion of testimony read as above recorded.)

[fol. 1897] The Witness: As the capacity of the condenser is further increased beyond this resonance point, the current in the circuit, consisting of condenser h and capacity j′ decreases. However, more of the current flowing in the antenna circuit will divide to the capacity branch of the circuit; so that, when the capacity is at an infinite value, if we desire to go that far, none of the current in the antenna circuit would flow through the coil j′.

By the Commissioner:

Q. 31. Could you plot some kind of a rough curve showing what you have just stated, with respect to the current flowing in the coil, flowing current from the coil against the condenser position?

A. I think I can plot that; but two curves should be plotted, in order to illustrate what I have said. One of these currents should be the current which flows in the circuit, consisting of the condenser h, parallel with the coil j′; and the other current should be that which flows in the antenna lead or in the ground lead, which is the same thing. (The witness makes another drawing on the same sheet of paper.)

I have here a curve which shows the relative values of the currents in the antenna lead and in the resonant circuit h-j′, under the assumed conditions. The ordinates repre- [fol. 1898] sent current and the abscissas the condenser setting. 50 degrees condenser setting is assumed to be the mid-position or the resonance position of the condenser.

As the condenser is increased in setting from zero, the current in the circuit h-j′ rises with increasing rapidity, as the resonance position is reached, reaching a maximum value when the inductive reactance of the circuit is near to the capacity of reactance.

Mr. Blackmar: May I interrupt a moment? When you say the current is a maximum in the circuit, you mean the circulatory current in that circuit?

The Witness: Yes, that is right.

A. (Continuing:) Then, as the capacity of the condenser is increased beyond the resonant point, the current in the circuit h-j′ rapidly decreases.

With reference to the current flowing in the antenna lead, owing to the poor match of impedances between the antenna circuit and the circuit h-j′, the maximum value of current in the antenna lead will be considerably less than the maximum value of current in the circuit h-j′.

This current will be a minimum at the resonance frequency; and, at this point, the circuit consisting of the condenser h and the inductance j′ offers the maximum parallel impedance.

[fol. 1899] The Commissioner: What you mean by this is that, at that resonant point, the voltage was the maximum across the coil j′?

The Witness: That is correct. I would like to point out at this time, in order that there can be no misunderstanding, that what I have just stated in connection with the circuit which I have drawn is not the condition which exists in the corresponding circuit h-j′ of Figure 2 in the drawing, for the reason that the resonant frequency of the circuit h-j′ was, according to the specification, for the two reasons which I pointed out, much lower than the transmitting frequency. That is, the resonant wavelength of the circuit h-j′ was much greater than that of the primary circuit of the transmitter, consisting of the capacity e and inductance d, as this capacity and inductance determines the wave-length.

Mr. Edwards: I will offer in evidence the sketch, consisting of two figures made by the witness, and ask that it be marked Defendant's Exhibit JJ.

The Commissioner: It is so received.

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Q. 32. Can you make a corresponding sketch or sketches which will include the induction coil g′ in the antenna, and explain the action with that coil included, as shown in Figure 2 of the Marconi patent?

[fol. 1900] A. Under the particular conditions which were assumed, namely, that the condenser h and the coil j′ were resonant, the condenser h in its mid-position, there would be no substantial difference in the curves. That is, we are assuming, in connection with the question which was asked, that the circuit h-j′ was actually resonant to the transmitting frequency when the condenser was in its mid-position.

Q. 33. In this case how does the coil g' affect the operation (indicating Fig. 2 of the Marconi patent)?

A. The circuit of Figure 2, from the facts pointed out in connection with the tables on page 4 and in connection with the deduction which was readily made in connection with the condensers e and h, and the coils d and j', operates in an entirely different manner from the circuit which was made the subject of the previous question.

As explained, the condenser h of Figure 2 is used as a part of the resonant system, consisting of the antenna f', the coil g', the condenser h, and the earth. When this circuit is resonant to the incoming frequency, the maximum voltage exists across the condenser h.

The Commissioner: You are now talking about the disclosure of the patent?

The Witness: Oh, yes.

[fol. 1901] A. (Continuing:) The maximum voltage exists across the condenser h, and a maximum of current flows through the coil j'. However, the current which flows through the coil j' under these conditions is not nearly as great as would be the case where the condenser h and the coil j' were adjusted so as to provide a circuit which was resonant to the received frequency or wavelength.

Q. 34. Assuming that the values of g' and h in Figure 2 are so adjusted as to make the natural frequency of circuit f', g' and h and e resonant to the incoming frequency, what, then, would be the frequency of the circuit h-j'?

A. As I pointed out previously this morning, from the information contained in the specification, the condenser e and the condenser h are substantially of the same value for tune No. 3. However, the coil j' has a much greater inductance than the coil d, so that the resonant frequency of the circuit h-j' is much lower than the resonant frequency of the circuit consisting of the condenser e and the coil d.

Therefore, considering the circuit between the tap on the coil g' and the earth—that is, just the impedance of that branch of the circuit, the reactance would be capacitive, even in the presence of the coil j'.

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[fol. 1902] Q. 35. From the values of the coils as given in the specification, can you say approximately how much

lower the frequency in the circuit h-j′ would be than the frequency in the circuit f′-g′-h and a ground?

A. Because of the fact that the coils d and j′ are of different diameters, this comparison can be only approximate. That is, if these coils were of the same size, the same diameter, it would be possible to pretty closely approximate the ratio of inductance, from the fact that the inductance varies approximately directly as the square of the number of turns; that is, if these coils were of the same diameter, the coil d having one and a half turns, the square of that would be 2.25; and if the coil in j′ would have a hundred turns, the square of that would be 10,000. Under these conditions, the ratio of inductances would be as 10,000 is to 2.25.

However, no such ratio as that exists, since the coil d is probably larger than the coil j′.

Now, on the basis of the length of wire, a comparisón can be made. The coil d contains 1.93 meters of wire. The coil j′ contains 10 meters of wire.

If this wire were laid out straight, the inductance would be approximately in the ratio of length of the two wires, namely, about 5 to 1.

[fol. 1902] However, when a wire is coiled, provided it is not coiled back upon itself but wound in the form of a helix, the inductance is enormously increased, so that I would say that the ratio of the inductance of coil j′ to coil d would be at least 5 to 1—probably 10 to 1.

Since the condenser e and the condenser h, are for tune 3, of substantially the same capacity, and since the resonant wavelength of the circuit consisting of inductance capacity in parallel varies as the square of the product of the inductance and capacity; then, assuming that the minimum ratio of inductances which was deduced above—the resonant wavelength of the circuit h-j′ and the circuit e-d′ would be at least as the square root of 5 is to 1, and probably much greater, because of the fact that the coil j′ is a coil rather than a straight piece of wire.

By the Commissioner:

Q. 36. What spacing of turns are you making in your assumption?

A. The assumption which I made assumed merely that the wire was straight, laid out as a straight piece, and

that there were no turns, which is the most unfavorable condition in so far as the ratio of inductances is concerned.

[fol. 1904] By Mr. Edwards:

Q. 37. Unfavorable in just what respect?

A. Unfavorable in the point of obtaining a high ratio; that is, it is the condition under which the lowest ratio of inductances exists.

By the Commissioner:

Q. 38. I was just thinking—correct me if I am wrong—that, where you took a piece of wire and wound it in a helix—by the way, is that not what Marconi intended to do?

A. Yes.

Q. 39. Well, then, when it is wound in a helix, does not the spacing of the turns have a great deal to do with the induction?

A. Yes, it does. The closer the turns are together, the greater will be the inductance. However, it is difficult to compare the coil d with that of coil j, in so far as determining an accurate value of inductance ratio is concerned. My comparison gives the lowest ratio of those inductances which could exist, namely, I assumed, in determining that ratio of the square root of 5 to 1, that the coils j′ and d were merely straight pieces of wire.

By Mr. Edwards:

Q. 40. When you say "the ratio of inductances," do you refer to the ratio of performance as between the two circuits?

[fol. 1905] A. No. Going back over what I said, I pointed out that there was approximately 2 meters of wire in the coil d and 10 meters of wire in the coil j′. If this wire were laid out straight and the inductance were measured, the ratio of inductance would be roughly 5 to 1. If these same straight pieces of wire were associated with two identical condensers, or two similar condensers, such as e and h, then the resonant wavelength would vary as the square root of the ratio of the inductances, namely, as the square root of 5 is to 1.

Q. 41. I do not think you have left it quite clear. When you speak of the ratio of inductances, is that equivalent

to saying the ratio between the frequencies of these two circuits?

A. No. I tried to bring that out in my last statement: that the ratio of inductances is as 5 is to 1, but the ratio of wavelength is as the square root of 5 is to 1, and the ratio of frequencies would be inversely proportional to the ratio of wavelengths.

Q. 42. I understand that your calculations were based upon wire not wound into coils.

A. That is correct.

Q. 43. And upon that basis what, approximately, is your understanding of the frequency of the circuit h-j′ as compared with the circuit f′ g′ e′ under the condition that the circuit f′ g′ h′ E is tuned to the resonance of the incoming frequency?

[fol. 1906] A. Since we have been speaking of resonance wavelength, I will answer the question first in terms of ratio of wavelengths. The circuit f′ g′ h E is tuned to a wavelength which bears the ratio indicated above of one to the square root of 5, with respect to the wavelength to which the circuit h-j′ is resonant. In terms of frequency, the circuit f′ g′ hE would be resonant to a frequency which bears the ratio with respect to the resonant frequency of the circuit h j′, which is approximately as the square root of 5 is to one.

By the Commissioner:

Q. 44. Now, as I understand it, your whole answer is based upon the fundamental assumption that these coils are formed in straight lines and not in coils.

A. That is correct. Now, if the assumption is made that these lengths of wire are wound in the form of coils, then the ratio which I have indicated would be much larger.

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[fol. 1907] (Tr. 1381)

Q. 45. Commander Dow, referring to Figure 2 of the Marconi patent, am I right in understanding that, if the wavelength of the antenna circuit f′ g′ h and E is in resonance with the circuit e, d, and C of Figure 1, that the wavelength of the circuit h j′ in Figure 2 will be longer than that of the circuit f′ g′ h E?

A. That is correct, and that is borne out by the analysis which I made this morning in comparing the coils j′ and g

when used in conjunction with the two condensers e and h which had substantially the same capacity as is indicated in the table on page 4 for tune 3, and that is also borne out by the places in this table where it is possible to compare the condition of adjustment of the circuits for two different tunes, such as tunes 3 and 4. My estimate, based upon the analysis which I made, is that the resonant wavelength of the circuit h j′ is at least the square root of 5 times the resonant wavelength of the circuit e g C of Figure 1. The square root of 5 is something over 2.

Q. 46. Does the same ratio obtain if the circuit h j′ is tuned to a wavelength corresponding to the wavelength of circuit e g C of Figure 1?

A. If the circuit h j′ was tuned to the same wavelength, it would be possible to select a value of g′ which would make the theoretical resonant wavelength of the antenna circuit, consisting of f′ g′ h and E resonant to the same [fol. 1908] frequency as the circuit h j′. However, in doing that, the circuit h j′ would, in effect, introduce extremely high impedance, which would be in the nature of a resistance, because of the resonant condition, into the circuit between the antenna and the ground. Whether such a circuit could ever be made resonant, I do not know. I would have to investigate that.

Q. 47. Yes. Now, as I understand it, your basis of comparison was that the length of wire in the respective coils was a straight wire and not a coiled wire. Am I right in that?

A. That is correct.

Q. 48. Now, if you had used a coiled wire, what would have been the difference with regard to the ratio between the wavelengths of circuit h j′ and f′ g′ hE?

A. The ratio of the wavelength between h j′ and that of f′ g′ hE would be much greater. That is, the wavelength of the circuit h j′, instead of being roughly twice that of the circuit f′ g′ hE would be three, four, or perhaps five times as great.

By the Commissioner:

Q. 49. Is this last assumption of yours based on uniform spacing of the turns, or uniform diameter of the turns, or what?

[fol. 1909] A. Assuming, for instance, that the coil D is about one meter around—that is, it takes one meter of wire

to make one turn. That would mean that the wire on the coil j', if wound into a coil of the same diameter, would make about three turns to one in coil D, or to one-half in coil D, if the turns were spaced the same distance. Then the ratio of the inductance in coil j' to that in coil D would be approximately as 9 is to 2.25, or four times as great.

Now, the coil j' is wound into a much smaller space—that is, the wire of coil j' is wound in a much smaller space than that, so the ratio would be even greater than 4 to one.

I might amplify that a little for the purposes of clarification by stating that, if you have a piece of wire and wind it into a coil, the smaller you make the turns without approaching infinitesimal dimensions and the more turns you have in a given space, the greater will be the inductance for a given length of wire; provided, of course, that the wire is not wound in such a way that one turn cancels the other one, as by winding the wire back on itself.

Mr. Edwards: I will offer in evidence at this time copy of Patent No. 640,516, granted January 2, 1900, to M. I. Pupin, upon application filed May 28, 1895, as Defendant's Exhibit KK.

[fol. 1910] Mr. Blackmar: May I ask the purpose of that, Mr. Edwards?

Mr. Edwards: The purpose of this offer is to show the function of the condenser h in the Marconi patent, and the difference between that function and mode of operation and the function and mode of operation of corresponding condensers in the prior art and in the defendant's device.

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(Tr. 1387)

The Commissioner: Obviously, as I understand the offer of this patent of Pupin, it does not in any way attack the validity of Claim 16 of the Marconi patent in suit. As you state, Mr. Blackmar, that has been decided by the Court, and I do not recall just now what procedure was followed after the decision and prior to this accounting proceeding; but the defendant had at that time opportunity for a motion for a new trial and presentation of newly-discovered evidence and all those matters.

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The Commissioner: Oh, yes. They had all the customary opportunities of complete review of all the questions of infringement and also validity.

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[fol. 1911] (Tr. 1388)

The Commissioner: I do not believe that defendant's counsel at this late date could presume to introduce any prior art to go to the validity of Claim 16.

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I take the offer just as he stated it, that the Pupin patent is offered for the sole purpose of aiding the witness and the Commissioner and the Court in an understanding of how the condenser in the Marconi patent operates. That is my interpretation of this offer.

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(Tr. 1391)

The Commissioner: I will take it in evidence, in accordance with the understanding which I expressed for the record a few moments ago.

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Q. 50. Have you read the Pupin patent, 640,516?
A. I have.

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[fol. 1912] (Tr. 1392)

Q. 52. Will you refer to the matter on page 1 of the Pupin specification, especially beginning at line 9 and running down to line 20, and explain what that operation is; and, in doing so, you might refer to Figure 2 of the drawing of the Pupin patent.

A. Figure 2 of the Pupin patent shows a system that is responsive to alternating currents which are impressed at the point x.

Q. 53. If you will permit the interruption, you might refer to Figure 1 first, because that leads up to the other figures.

A. Pupin states broadly, beginning with line 9, that, if a condenser and a self-induction coil are connected in shunt, and an alternating current is impressed across such a circuit, the current in the shunt circuit flowing around the shunt circuit will be greater under certain conditions than

in the line itself. Such a circuit was illustrated in the sketch which I drew this morning, and curves were shown to indicate the conditions under which this phenomenon existed.

That is, when the frequency of the alternating current corresponded to that resonant frequency in which the shunt circuit is tuned, then the circulating current of the shunt circuit will be a maximum; and, concurrently and coinci- [fol. 1913] dentally, the current in the line feeding the shunt circuit will be considerably less. In fact, it is usually at the minimum value at that point.

By the Commissioner:

Q. 54. In your discussion this morning in connection with the circuit, and the curves which you drew, Defendant's Exhibit JJ, did you have anything in mind in connection with your testimony, other than what is termed by those skilled in the art as radio frequency?

A. That phenomenon is found regardless of the frequencies. It applies to all alternating currents.

Q. 55. It applies to what?

A. It applies to alternating currents of any frequencies.

Q. 56. Then your discussion this morning is not limited to phenomena such as are referred to as involving currents of radio frequency?

A. No, sir. It refers to any frequency.

Mr. Blackmar: I might point out to your Honor that antennas are involved in those circuits.

The Commissioner: That is a point I had in mind, that the sketch should involve antenna.

The Witness: Well, but the curves are applicable to the same circuit, whether a low frequency is impressed on the antenna or a high frequency.

[fol. 1914] A. (continued): Pupin further states, beginning with line 33, as one of the conditions to be met with in the adjustment of the circuit, that the capacity of the condenser and the self-induction of the shunt coil must be so related as to bring the shunt circuit in resonance with the line current; and, beginning with line 68, he states:

"By a 'resonance circuit' is meant one which has self-induction and capacity and therefore a natural period. When the self-induction and capacity are properly adjusted

so as to give it the period of the impressed electromotive force, the resonance circuit becomes resonant to this electromotive force."

Q. 57. In connection with Figure 2, Pupin states, beginning with line 89 on page 2:

"Referring now more particularly to Fig. 2 which represents the receiving end of the line and can be hitched onto the transmitting end shown in Fig. 1 instead of the receiving end shown in that figure, I show a resonance circuit H I and K L."

These are two resonance circuits in series, between the point x and the earth connection.

"The condensers H and K, as in Fig. 1, are in series in the line; but I control the frequency of H I by means of a circuit H′ I′ I″, which contains the secondary coil of the induction coil I I′ and the supplemental condenser H′ and a self-induction coil I′."

[fol. 1915] He adjusts this condenser h, or selects the condenser h, and inductance I of the primary coil of his transformer to be resonant to frequencies that he is interested in transferring to the secondary circuit.

He also adjusts the secondary circuit consisting of I′ and inductance I″ and the inductance of the solenoid of his responsive device, to neutralize the capacity of his condenser h′, so that the secondary circuit is also resonant.

By the Commissioner:

Q. 58. What is the character of the current impressed on x?

A. An alternating current. He does not state the frequency, as I recall; but, presumably, it is a relatively low frequency. That is, as far as I can gather from his disclosure. Perhaps it is some frequency between 25 cycles and 2,000 cycles per second.

Q. 59. Continuous?

A. No. He interrupts his current with a key. If you will refer to the left-hand part of Figure 1, which shows his alternators which generate the frequency, you will see that he has one alternator A and the other one B. And, in those circuits, he has a key; so that if, for example, alternator A has a frequency of say 100 cycles per second.

he impresses that on the line x g′ and, for the resonant frequencies, the circuit consisting of condenser h and the coil I is adjusted to resonate at this frequency of 100 cycles [fol. 1916] per second; and then the maximum current possible will flow in the primary coil of his transformer I-I′.

If he at the same time adjusts the inductance I′-I″, plus that of the solenoid in the responsive device M, so that the value of that capacity of the circuit is neutralized, so that that circuit also is resonant, then he will get a maximum transfer of current or of voltage to the secondary circuit.

Q. 60. What I had in mind when I made my inquiry with reference to the word "continuous," was the generators A and B themselves in Fig. 1, rather than a key. In other words, my inquiry went to what kind of current these generators produce.

A. I would say probably current in the nature of simple alternating current, very much as you would have—except with much lower frequencies—the same as you would have with a modern C W radio transmitter.

Q. 61. A continuous current?

A. Continuous current.

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By Mr. Edwards:

Q. 63. I believe you mentioned that Pupin might or would operate with frequencies up to in the neighborhood of 2000. Did you have in mind at that time the fact that Pupin mentions a frequency of 250 in the circuit, for example on page 2, about line 86, and perhaps in other places?

[fol. 1917] A. I did not at the time I made that statement, although I can see no difference in the operation of the circuits of Figure 1 and Figure 2, whether the frequency was 100 or 250 or 2000 cycles per second. Those are all within the range of audibility in which his responsive device M was undoubtedly operative.

Q. 64. What would be your opinion of a range of frequencies up into the neighborhood of 2,000?

A. Those are all audio frequencies, but could be used in such a system as that shown in Figures 1 and 2.

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(Tr. 1398)

Q. 66. What instruction does Pupin give in regard to securing the stated desired object of having greater current in the coil than in the condenser path?

A. I think I quoted from the specification beginning about line 33 on page 1, that the capacity of the condenser and the self-induction of the shunt coil must be so related as to bring the shunt circuit in resonance with the line current.

Q. 67. And the wavelength of the line current in this case would be that of the wire from the generator?

A. That is right. The wavelength would be equal to the velocity of light divided by the frequency. That would be the wavelength along the wire.

[fol. 1918] Q. 68. Well, from the fact that he mentions that it is in the multiplex telegraphy system, is it or is it not a fair inference that the frequency of the current is transmitted by wire from the wire x at the transmitter to the wire x at the receiver?

A. That is correct.

* * * * * * *

Mr. Edwards:

I will offer in evidence a copy of the Fessenden patent No. 706,735, granted August 12, 1902, upon application filed December 15, 1899. I may say that this patent is already in evidence as one of the plaintiff's exhibits. It is part of Complainant's Exhibit 171, and appears on pages 90 to 97 of that exhibit.

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(Tr. 1401)

It is offered in justification of what the defendant has been doing. I am offering it at this time as a matter of convenience, in order to have in this record on accounting the prior art, such as Pupin and Fessenden, together.

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But here I offer it, not to show invalidity but as showing justification for the defendant's use.

I ask that this be marked Defendant's Exhibit LL.

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By Mr. Edwards:

Q. 69. Commander Dow, will you look at Fessenden Patent 706,735 and explain the operation of that device, particularly with reference to the function of the condenser 19 in Figure 1 of the patent?

A. Fessenden shows, in the right-hand sketch of Figure 1, a transmitting station comprising antenna conductor 5 and 5-A, a ground connection down in the lower right-hand corner of the figure, which is not marked, an induction coil 1, a primary which is connected through battery 2 to the key 3 for interrupting the flow of current: a spark gap, 4-4, and a condenser 18 across the spark gap.

In the left-hand figure he shows a receiving system for an aerial conductor 6, which is connected through a shunt circuit consisting of the inductance 7, which consists of two coils with a galvanometer and a condenser 19.

He states in his specification, as shown in Figure 1, beginning with line 101 on page 2 of the specification:

"As shown in Fig. 1, a condenser 19 may be connected in shunt with the field coil or coils 7 for the purpose of obtaining as large a current as possible in the field coil 7, as this increase in current will give a greater torque to the ring 8."

He further states, in continuation, beginning with line 25 on page 3 of the specification:

[fol. 1920] "This shunt circuit must be tuned to the receiving conductor; otherwise, the oscillations produced by it will have no action upon the wave-responsive device at the receiving station. This shunt circuit, by virtue of its capacity, stores up an additional amount of energy, and, when a spark passes across the gap, since the sending conductor can radiate energy at a given rate, it must continue to radiate for a longer time, in order to dissipate this additional stored-up energy."

In the matter between the two quoted portions that I read, he explains, among other things, how, when this shunt circuit is made resonant to the incoming frequency, the current flowing around the shunt circuit is greater than in

the antenna, just as I explained in connection with the sketch which I drew this morning.

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(Tr. 1408)

Q. 72. How does the transmitter portion of Figure 1 of the Fessenden patent compare with the transmitter Figure 1 of the Marconi patent in its function and mode of operation?

A. Marconi employs two circuits in his transmitter, with a rather loose coupling between them. The transmitter of Fessenden is what may be called a direct-coupled or very closely-coupled system, in that his primary circuit or oscillation-generating circuit is directly connected to the aerial conductor or antenna. Marconi also has the coil g' [fol. 1921] in his antenna, whereas Fessenden has not. Marconi uses a loading coil. The wave emitted by the circuit shown by Fessenden would, according to all experience that I have had with spark transmitters, be a highly damped one. The wave emitted by the Marconi circuit would be classed as a damped wave, but would not be nearly so highly damped. In so far as frequency is concerned, they might have the same frequency. Both would be approximately radio frequencies. Both Fessenden and Marconi receivers would be receiving the radio waves.

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(Tr. 1410)

By the Commissioner:

Q. 78. Let us go back to radio waves in connection with Fessenden. You say that Fessenden would transmit waves that would be highly damped. What do you mean by that?

A. I mean by that that the individual wave trains which were caused by the discharge of the condenser 18 would have—or would die out in the course of a few, a very few, high-frequency cycles.

Q. 79. Would that have any effect on resonance?

A. It would, to this extent: that the waves generated by Fessenden's apparatus would take up a wider band of frequencies in the spectrum than would be taken up by Marconi's apparatus. This Marconi apparatus was more selective.

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[fol. 1922] The Commissioner: Perhaps we can save some time there, if the witness will tell us whether the Marconi transmitter is what has been referred to here as a tuned transmitter.

A. Marconi's system, comprising his transmitter and receiver, is capable of much sharper tuning than that of Fessenden.

By the Commissioner:

Q. 81. Would you call Marconi's transmitter a tuned transmitter?

A. Both Marconi's and Fessenden's are tuned transmitters.

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(Tr. 1415)

By Mr. Edwards:

Q. 97. Now come to the receiver portion of the Fessenden patent. How does the coil 8, which you term the galvanometer coil, compare, in its function and operation, with the secondary circuit shown in Figure 2 of the Marconi patent?

A. The coil 8 of the Fessenden patent is not tuned by a secondary condenser, as in the case of the coils j″ of Figure 2 of the Marconi patent. Had Fessenden tuned this coil 8 in the manner taught by Pupin, he probably would have gotten a greater effect than he did with the arrangement shown by Fessenden.

[fol. 1923] Q. 98. Now, tell us what happens when current is induced in the coil 8 of the Fessenden receiver.

A. Well, due to the magnetic field set up by the primary coil and the magnetic field set up by the induced current in the secondary coil, the coil 8, a torque is produced, and the coil 8 tends to turn, giving an indication of the incoming signal.

Q. 99. How does that operation compare with Marconi's Figure 2, so far as detection goes?

A. Well, Marconi employed another form of detector. Marconi used an entirely different form of wave responsive device. He used a detector and a receiver. His detector in Figure 2 is the item indicated by T.

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(Tr. 1418)

Q. 104. Yesterday, Commander Dow, you were referring to the comparative operation of the Fessenden and Marconi transmitters. Can you make a simple sketch showing the form of wave trains created in the charging circuit, and the form as emitted from the antenna in each of the transmitters?

A. I have drawn such a sketch.

In the circuit of Fessenden, the right-hand circuit in Figure 1 of the drawings, the antenna is directly connected to the charging circuit; so that the wave trains in the antenna would be the same as those in the charging circuit. [fol. 1924] By "charging circuit" I mean the circuit of the spark gap and the condenser 18. In the circuit of Marconi, Figure 1, where a loose coupled arrangement is used rather than a direct coupling arrangement as in the case of Fessenden, the closed circuit is as I have indicated in my sketch.

When the spark gap breaks down, by virtue of the coupling between Marconi's coil d–d′, the currents are induced in the open circuit, and very shortly following the instant of the breaking down of the gap, the current begins to build up in the open circuit of Marconi and reaches a maximum; and then, during the series of oscillations, which are in general of the same character as those of Fessenden, the oscillations continue in the open circuit, even for a short period of time after the oscillations die out in the closed circuit.

From the sketch I have drawn, showing the action in Fessenden and that for Marconi, the general character of the oscillations in the two circuits or in the circuits of Fessenden and Marconi, are very much the same.

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(Tr. 1421-2)

Q. 112-3 Is it now and has it always been necessary to employ some device for the purpose of translating these electromagnetic or electrostatic waves into sound waves?

A. It is and has.

[fol. 1925] Q. 114. What is such a device usually known as?

A. Such a device is usually known as a detector.

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The device shown in the Fessenden Patent 706,735 is only for translating the electromagnetic waves into the movement of a mirror which can be seen or sensed visually rather than orally.

Q. 116. Now, specifically, just how is that done in the device shown in the receiver of Figure 1?

A. As I explained yesterday in my testimony, the electromagnetic waves propagated from the transmitting station set up a flow of current between the antenna 6 and the earth connection, which is shown but not identified by a reference character.

Between the earth connection and the antenna 6 is a resonance circuit consisting of the condenser 19 and two coils 7. This circuit is made resonant to the radio frequency of the incoming signal. Interposed between the two coils 7 is a secondary coil 8, which is suspended by a string; the current flowing from the coils 7 sets up by induction a flow of current in the coil 8.

As a result of the magnetic fields due to the currents in the coils 7 and the coil 8, the ring 8 is caused to turn, which causes the mirror 9 to move, and causes a light beam to move on the scale shown.

[fol. 1926] Q. 117. What was the function and mode of operation of the condenser 19 in this receiver shown in Figure 1?

A. The condenser 19 is employed to cause the circuit, including the coil 7 and the condenser 19, to be resonant to the frequency of the incoming oscillations.

Q. 118. Then, when it is resonant, what happens?

A. When it is resonant, the maximum current flows in the coils 7 and the maximum effect upon the secondary coil 8 is produced. When this condition exists, as explained in Fessenden, with the matter beginning with line 102 on page 2 and continuing through to line 13 on page 3, the current in the resonance circuit is much greater than the current in the antenna circuit.

Q. 119. To what wave length is the circuit 19–7 made resonant?

A. That is, as I explained, made resonant to the radio frequency of the incoming signal.

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(Tr. 1425)

Mr. Edwards: I will offer in evidence the sketch made by the witness, shewing the wave trains of the transmitters.

according to the disclosure of Marconi, and I will ask that this be marked Defendant's Exhibit MM.

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(Tr. 1494)

Q. 271. If the loading coil corresponding to the wavelength of both the normal and highest wavelength of one of these instruments, as given in the instruction books, but lower than the highest assigned wavelength of the secondary [fol. 1927] of the instrument, were used, what would be the relative advantages or disadvantages of using such a coil, as compared with placing the condenser in shunt with the primary of the inductances in the circuit?

A. To answer that question from an engineering point of view would require a knowledge of the detailed construction of the receiver and the coils—that is, the resistances of the coils enter into it. However, the fact that it became unconventional to connect the primary condenser in parallel with the primary inductance in receivers of this general design which followed the period under consideration, leads me to believe that it was at least as efficient, if not more efficient, to employ the external loading coil, instead of connecting the primary condenser in parallel.

This is also confirmed, in a measure, by the fact that if it were more efficient to have a primary condenser connected across the antenna inductance system, it would have been used, not only in the last two positions of the switch of some of these receivers, but in the other positions also.

Q. 272. Take the SE 143, for example. If the proper-sized loading coil were substituted to give a wavelength range up to, say, 6,800 meters, which, as I recall it, is the wavelength range for the secondary in that instrument, [fol. 1928] what would be the relative advantages of the load coil and the parallel connection, with respect to selectivity or efficiency?

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A. I am unable to answer that question without going into the actual design of the receivers, since so many factors are involved in determining the answer to such a question. I might add that the fact that external binding posts were provided for a loading coil, would indicate to me that the use of an external loading coil was the preferred arrangement, and, possibly, the more efficient one.

Q. 273. Why would it be the preferred arrangement from the standpoint of operative advantage or increasing the efficiency or other qualities of the instrument?

A. If it had been intended by the designers of the receivers that the primary condenser be used, in preference to an external load coil for extending the frequency range or extending the wavelength range of the receiver, the designers would have provided two or more conveniently-arranged binding posts with suitable arrangements for rapidly making the connections of the condenser.

Q. 274. Granting all that, would the load coil serve just as well as the condenser in parallel, or would it have advantages, or disadvantages?

[fol. 1929] A. It is my opinion that the load coil would have been the much preferred arrangement.

Q. 275. For what reasons?

A. Because by the use of a loading coil the entire antenna circuit is made resonant. By that I mean that all of the capacity and inductance in the antenna circuit as a whole, including that in the antenna as well as in the coils and condensers of the circuit, are resonated to form a simple resonant circuit. Where a condenser is connected across a coil which provides only a part of the total inductance that is in the circuit, the resulting electrical structure is much more complex.

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A. (continued). In order to provide for an efficient transfer of energy from one point of the circuit to another part, there must be a proper match of impedances. The impedance of a circuit, consisting of a condenser and inductance in parallel—the impedance of a parallel resonant circuit consisting of an inductance and condenser in parallel, is very high. To properly and efficiently transfer energy to such a circuit, requires that the generator impedance be high also. By this I mean that if the impedance of the antenna circuit measured between the point of connection of the antenna to such a parallel circuit and the ground connection to such a parallel circuit is not high, the transfer of energy would be inefficient. It is like attempting [fol. 1930] to transfer energy from a storage battery, a high capacity storage battery, to a high resistance. To provide for a transfer of maximum power or energy from such a storage battery to a resistance would require that the re-

sistance of the battery be made substantially equal to that of the resistance.

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By the Commissioner:

Q. 277. I am afraid your answer is rather long, and I would think a little complex, myself. Let me see if I get the gist of your answer. As I understand it, it covers two basic things: first, am I correct in assuming that you stated that the electrical circuit involved in the parallel connection of the condenser was much more "complex" than the series connection?

A. Much more complex electrically.

Q. 278. Not physically?

A. Just electrically.

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By Mr. Edwards:

Q. 280. Could you make a diagram explaining that operation?

The Commissioner: Before you make a diagram, let us look at the physical question. It is no more complex physically, as far as elements are concerned?

The Witness: No.

[fol. 1931] By the Commissioner:

Q. 281. It costs no more to manufacture?

A. No. However, from the point of view of efficiency, determining the efficiency, it is much more complex in the case of the parallel circuit. I have prepared a sketch to indicate and or to show a comparison between a circuit in which all of the elements are in series, as in the case where a loading coil is used, and a circuit in which the parallel condenser is used.

Where a number of capacities and inductances are in series the circuit can be readily reduced to a simple one, consisting of a single inductance and a single capacity.

By Mr. Edwards:

Q. 282. Would you mind saying as shown in each one of your figures?

A. As shown in the right-hand upper figure labeled "Equivalent Simple Circuit." The capacity in this case is equal to the product of the two capacities, divided by their sum. The inductance is merely the sum of the several inductances, including that of the loading coils, coupling coils, and the antenna. Where an antenna is connected, or where a parallel resonant circuit is connected between an antenna and ground the case is not nearly so simple electrically and consists of two coupled circuits.
[fol. 1932] To determine the best values of inductance and capacity for the several portions of the circuit in order to permit the most efficient transfer of energy from one through the other, presents a rather complex electrical problem, but one which is amenable to solution.

By the Commissioner:

Q. 283. Before you go into all the detailed solution of this problem, is it a fact that, in your opinion, the parallel connection of the condenser is more complex electrically than the series connection?

A. That is correct.

Q. 284. And all of these sets involved in this accounting proceeding in which you are now testifying as an expert witness, called by the defendant, did use that parallel connection, did they not, even though it was more complex electrically?

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A. I know only that in the case of certain receivers switches were provided in the design of the receiver for employing a parallel connection of the condenser. In certain portions of the wavelength band in other receivers, no switches were provided, but mention was made in the instruction book that the condenser could be connected in parallel by making certain external connections, and in other receivers no mention was made in the instruction book regarding this use.
[fol. 1933] In all of these receivers, except those which I understand are grouped in Class I, external binding posts were provided for the specific purpose of inserting a loading coil for increasing the frequency range or increasing the wavelength range.

Q. 285. Then do I take your answer to mean that you do not really know whether these were used? I suppose

you really do not know whether the parallel connection was used. I believe that is the interpretation I have put on it. So suppose I reframe my question and ask you if in some of these receivers, at least, the sets were not capable of such parallel operation. Take the ones with the switch. What was the switch for?

A. In that case the switch was for the purpose of connecting the condenser in parallel.

Q. 286. Even though it possessed the more complex electrical characteristics?

A. That is correct.

Mr. Edwards: That would be true of Groups I and II. The switch was there and we did use it.

Q. 287. Somebody thought that was a good thing to have on those sets, didn't he?

A. It was, mechanically and electrically, a convenient arrangement, although, from the point of view of electrical efficiency of the circuit, it may not have been the most desirable one.

Q. 288. Why was it put on the sets?

A. Because of its mechanical and electrical convenience, probably.

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[fol. 1934] (Tr. 1502)

Mr. Edwards: I offer in evidence the sketch made by the witness, as Defendant's Exhibit PP.

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Dow—Cross

(Tr. 1550)

X Q. 452. That was my question, I think. Now I wish to refer you to Plaintiff's Exhibit 413-A. It is one of the instruction books for the SE 143 receiver. On sheet 2-A thereof this statement appears in reference to Figure 1: "This places the primary variable condenser in parallel with the primary inductance. This combination is very efficient on waves longer than 3,000 meters; on short waves, it is not as efficient as the series capacity." Do you agree with that statement?

A. I would take that statement as being correct, since the efficiency of the circuit is dependent upon the ratio,

or is dependent upon the design, which involves the ratio of capacity to inductance. For any given frequency, an optimum ratio of capacity to inductance exists.

X Q. 453. In view of that statement, may I ask the basis for a statement which you made in your direct examination today, namely, that the use of a loading coil was—and I think I am quoting you—"the much preferred arrangement" in the SE 143?

[fol. 1935] A. I think that the statement I made should be quoted more completely.

X Q. 454. Do you now think that the use of a loading coil in the primary circuit of the SE 143 was much preferred to the use of the parallel connection for an increase in the range of the primary circuit?

A. The use of a loading coil with an antenna series condenser would permit of an impedance match between the antenna and earth connections and the coupling circuit of the receiver. Where a parallel resonant circuit is used at resonance the impedance is extremely high, reaching a value of many thousands of ohms, ordinarily. Normally, the impedance of short antennas—that is, antennas that do not approach a quarter of a wavelength in length,—is relatively low, compared with that; therefore, if an antenna were connected to a parallel resonant circuit, there would be a mismatch of impedances. For that reason I would say that the use of a loading coil would be the more efficient arrangement.

X Q. 455. You think the use of a loading coil would be better in the SE 143 than the parallel connection for the purpose of extending the range of the primary circuit so that it would cover the same range as the secondary circuit; is that correct?

[fol. 1936] A. I can foresee engineering points involved, in an answer to that question in the manner in which it is framed.

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X Q. 457. What is the difficulty with the way it is framed?

A. In that the question refers specifically to the SE 143 receiver; and I am not familiar in detail, as I mentioned this morning, with the actual ratio of capacity to inductance, and the resistance of the coils of that receiver.

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(Tr. 1554)

X Q. 461. Let us get back to the question of which is the preferred arrangement, or which is the better. With respect to the SE 143, are you prepared to say which would be better, the use of the parallel connection of the antenna condenser, or the use of the loading coil?

A. Why, it would certainly be more convenient.

X Q. 462. I did not ask you that.

A. I should like to ask if your question is directed to a matter of efficiency, or convenience?

X Q. 463. Efficiency.

A. Then it would be necessary, to answer the question, to consider the circuit design.

X Q. 464. You cannot answer that with your present knowledge of the SE 143; is that correct?

A. That is correct.

[fol. 1937] (Tr. 1555)

X Q. 467. Perhaps it is a little involved. I will split it into two questions: does your statement that you cannot say which would be the more efficient,—the use of a loading coil or the use of the parallel condenser with the SE 143, because you are not sufficiently familiar with the circuit characteristics,—apply to the other receivers of Group III?

A. Yes. In amplification of that, I should like to say that for the reasons given above an impedance match does not exist when the parallel connection is used, which would favor using a loading coil, provided the proper ratio of inductance to capacity could be maintained. To answer the question completely, and based upon an engineering study of it, would require a detailed examination of the electrical constants of the resulting circuits.

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X Q. 468. Then I take it from what you have been saying that the design of a receiver is something that requires real engineering knowledge and skill?

A. That is correct.

X Q. 469. And to determine what arrangement of circuits may be most efficient with any particular receiver, requires either a comparatively complete knowledge of the constants of that receiver, or actual test of the receiver, does it not?

[fol. 1938] A. That is correct. For example, in the light

of existing knowledge all of these receivers that we are now considering would be classed as very poor receivers, for many reasons.

X Q. 470. That was not true in 1917, 1918, and 1919, was it?

A. That is correct.

X Q. 471. That is, they were high-class receivers, in those days?

A. That is correct.

X Q. 472. And for example with these various types of receivers it is possible to tune the primary circuit to a particular wavelength with various combinations of inductance and capacity. Isn't that ordinarily so?

A. That is correct.

X Q. 473. That is, you would increase the inductance and decrease the capacity and tune it the same way, wouldn't you?

A. That is correct. However, an optimum ratio of capacity to inductance exists for the most efficient operation.

X Q. 474. That is what I was coming to. Of those various arrangements, one is best, normally?

A. That is correct.

X Q. 475. And likewise when you increase the range of the primary circuit beyond its normal range with the series connection, whether it is better to do that by a loading [fol. 1939] coil or by a parallel condenser, depends on the constants of the receiver and the way it is constructed, doesn't it?

A. That is correct.

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[fol. 1940] (Tr. 1560)

X Q. 478. I show you a sketch. Does that fairly indicate the primary circuit of the SE 143, with a loading coil used to increase the range of that circuit, the sketch indicating in the dotted line the antenna capacity?

A. Yes; except that the antenna capacity you have placed in the ground lead rather than in the antenna lead.

X Q. 479. Yes, I see. I will correct that. Is the antenna condenser on the antenna side of the loading coil that we have spoken of?

A. It is.

X Q. 480. Now, does the corrected sketch that I show you, then, correctly represent the circuit that I have discussed?

A. Yes, it does. I might add that you have shown the antenna capacity in dotted lines, or you have shown the equivalent circuit of the antenna in dotted lines represented by a capacity only. That is satisfactory in this case, since the inductance of the antenna would be quite negligible compared to the other inductances in series.

X Q. 481. Would you mind drawing, to the right of that sketch, the equivalent circuit?

A. I will draw it.

X Q. 482. In the left-hand sketch, I will mark the——

A. I would like to add, here, that it is assumed that we are considering the operation of this circuit at a given frequency, and that the capacity of the antenna is assumed to be the effective capacity at that particular frequency.

[fol. 1941] X Q. 483. I will accept those assumptions. Now, I will mark on the left-hand sketch referenced characters for the various reactances: C for the antenna tuning condenser; L_L for the loading coil; L_R for the loading coil embodied in the receiver; L_C for the coupling coil; C_A for the antenna capacity. Will you please indicate on your sketch of the equivalent circuit at the right, the corresponding elements?

A. I am assuming further that the consideration of the—or the extent to which this circuit is to be considered, permits the ignoring of the resistances in the circuit.

X Q. 484. Have you marked your sketch as I requested?

A. I have.

X Q. 485. You stated that there was no series parallel switch on the diagram SE 143 that you have before you. Are you familiar with the method of connecting the antenna tuning condenser in parallel with the coils, as indicated in the beginning of the instruction book?

A. I am. The condenser is merely connected by a rearrangement of the circuits outside the front panel, to put the condenser in parallel with the inductances.

X Q. 486. Now, I have drawn on the same sheet at the lower left-hand corner another circuit diagram. Does that properly represent the primary circuit of the SE 143 re-[fol. 1942] ceiver when the antenna condenser is connected in parallel, as described in the instruction book? The sketch also indicating, in dotted lines, the capacity of the antenna.

A. This diagram shows the proper connection of the condenser across the inductance coils. However, in this case, the inductance of the antenna is not in series with

an inductance which would make the antenna inductance negligible at all frequencies. It would be more accurate, in making an analysis of such a circuit as this, to show in series with the effective antenna capacity an inductance representative of the antenna inductance.

X Q. 487. Can you state at what frequencies relative to the fundamental of the antenna itself, the inductance of the antenna would be negligible?

A. I cannot state that offhand. As I pointed out in connection with my sketch yesterday, it is very difficult to compare a relatively simple electrical circuit with one which can be shown analytically to be quite complex, without knowing the various electrical constants of the circuit.

X Q. 488. In your sketch at the right of the top which you have already made, you have neglected the distributive capacity of the coils, have you not?

A. Yes; also the resistance of the circuit is neglected.

[fol. 1943] X Q. 489. Does the resistance have any effect on the tuned frequency of the circuit?

A. Only a very slight effect, which can be considered negligible, I think for present purposes.

X Q. 490. Now, the distributive capacity of the coils extends, does it not, not only from each turn to each adjacent turn, but also from each turn to other turns in the same coil?

A. Yes, that is correct. But, in treating analytically circuits having coils with distributive capacities, it is the custom to consider the distributive capacity of the coil to be represented by a small capacity in parallel with the coil. The actual value of the distributive capacity will vary with the wavelength or frequency used, just as the effective capacity of the antenna varies with the wavelength or frequency used.

However, if the conditions that were assumed at the beginning of this discussion are made, namely, that we are considering the operation of these circuits at a given frequency, it is sufficient to consider a capacity as a given value for such a frequency.

X Q. 491. That is a convenience, rather than an accurate mathematical assumption, is it not?

[fol. 1944] A. No. It is accurate to consider the effective value of a capacity as an accurate value for any particular frequency.

X Q. 492. For any particular frequency, yes.

A. Yes.

X Q. 493. Supposing the frequency is changed. Then that capacity is changed, too, is it not?

A. Yes. It is accurate to substitute in this formula the actual value of the capacity.

X Q. 494. Then, as to the inductance of the antenna itself, does that increase or decrease as the wavelength increases?

A. I have not reviewed that particular thing for some years; but I am quite sure that the inductance of the antenna can be considered as constant for any particular frequency. That is, when the capacity changes, when the effective value of the capacity changes, that change is due to the change in the inductance of the antenna.

X Q. 495. As the wavelength which you are considering increases, is it not true that the inductance of the antenna becomes of less and less importance, due to the fact that the other inductances in the circuit are increasing?

A. That is correct.

X Q. 496. Now, the parallel connection in the antenna is used on the longer wavelengths, is it not, normally, rather than the shorter?

[fol. 1945] A. Yes, that is correct.

X Q. 497. For any particular receiver?

A. That is correct. That is, as it was used in these receivers that are under discussion.

X Q. 498. That is all I am interested in for the moment. In those cases it was on the longer wavelengths that the parallel connection was used?

A. Yes, that is right.

X Q. 499. And, on those wavelengths, the antenna inductance was smaller in comparison with the other inductances in the receiver than on the shorter wavelengths, was it not?

A. That is correct.

X Q. 500. Have you any idea of what the magnitude of the antenna inductance might be, for antennas such as the SE 143's were used with?

A. I cannot, offhand, indicate the value of that. It is quite small.

X Q. 501. Quite small?

A. Yes.

X Q. 502. It does not amount, for example, to as much as the inductance of the coupling coil which I have denominated L_c in the antenna circuit, does it?

A. I would say that it should be somewhat smaller than that.

[fol. 1946] X Q. 503. And, considering the reactance of the antenna at the longer wavelengths, it would be capacitive, would it not?

A. Yes, it would.

X Q. 504. Now, will you draw, at the lower right of the figure, please, the equivalent circuit of the lower left-hand figure, and mark the element with reference characters corresponding to those in the left-hand figure, please?

A. I have so drawn the circuit.

X Q. 505. I think you have added an additional reference character there which does not appear in the left-hand circuit. What is that?

A. I have added a small inductance, representative of the inductance of the antenna.

X Q. 506. And how have you marked it?

A. I have marked that L_a.

X Q. 507. Now, I will ask you to disregard the inductance of the antenna and draw an equivalent circuit, disregarding that—whether or not you agree that it should be disregarded. Have you done so?

A. I have.

X Q. 508. Will you add to it the reference characters I applied to the lower left-hand figure?

A. I have.

Mr. Edwards: Read that question.

[fol. 1947] (X Q. 508 was read by the reporter as above recorded.)

Mr. Edwards: I suggest that you make some indication to show that that is not one that follows the rest of the drawings; so, that, later on, we will not be offering this sketch as something with which the witness agrees.

Mr. Blackmar: Yes. I will now mark the five sketches on this paper as follows:

Figure I being the primary circuit of the SE 143 receiver, using a loading coil to increase the range of the circuit.

Figure II being the equivalent circuit thereof, as drawn by the witness.

Figure III is the circuit diagram of the primary circuit of the SE 143 receiver, with a condenser connected in parallel to increase the range of the circuit.

Figure IV is the equivalent circuit thereof, as just drawn by the witness.

Figure V is the equivalent circuit thereof, as drawn by the witness at my request, disregarding the antenna inductance.

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[fol. 1948] (Tr. 1571)

Mr. Blackmar: I ask that the sheet of sketches be marked as Plaintiff's Exhibit 453 for Indentification.

(The paper just offered was by the reporter marked as indicated, together with his name and the date.)

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(Tr. 1573)

Q. 513. Does what you have just said or should what you have just said been taken to mean that, in your opinion, in the SE 143 receiver, the parallel connection of the antenna condenser is not so good a way of increasing the wavelength range of the antenna circuit as the use of a loading coil?

A. I am unable to answer that question, as I have explained, for the reasons, as I have explained several times previously—without knowing the electrical constants of the circuit of the antenna, without knowing the frequency under consideration, it is not possible to answer such a question; and, to answer it even with a knowledge of those constants, would require an extended analysis.

X Q. 514. I just want to make sure that what you said before would not be misunderstood also—what you said particularly as to the different impedances of the antenna and parallel circuits.

[fol. 1949] We are not to draw the inference, from what you said as to that, that the parallel connection would not be as good in the SE 143 receiver as the loading coil, are we?

A. No.

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(Tr. 1584)

X Q. 553. Have you a copy of the Marconi patent?

A. I have.

X Q. 554. That is Patent No. 763,772?

A. Yes.

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(Tr. 1585)

X Q. 557. What is it in the antenna circuit that is tuned to the received wavelength?

A. The antenna f, the coil g′, the condenser h, and the earth capacity e. The coil j′ would obviously have some effect on the actual adjustment of that circuit.

X Q. 558. I am not sure, then, that that answers my question, in view of that last reservation and modification. Is there anything in the antenna circuit, or any part of the antenna circuit, that is tuned to the received wavelength?

[fol. 1950] A. The circuit as a whole, in whatever complexity it exists, is adjusted presumably so that a resonant condition exists. Now, that resonant condition is affected mainly by certain structural parts of the circuit; and, of course, it is also affected by anything that is coupled to or attached to the circuit, to a greater or lesser degree.

X Q. 559. Do you mean that the circuit as a whole is tuned to the received wavelength?

A. Broadly, I think that is a re-sonable answer to make, since anything which is attached to or coupled to the circuit enters into the adjustment or tune for the resonant condition.

X Q. 560. In other words, then, as I understand you, it is the circuit made up of the antenna f′, the loading coil g′, the condenser h, and the coil j′, and the earth connection?

A. Plus——

X Q. 561. Plus any effects that may be brought into that circuit from the secondary circuit?

A. That is correct.

X Q. 562. That, as a whole, is tuned to the received wavelength?

A. That is correct.

[fol. 1951] X Q. 563. Then, if I understood your direct testimony to be to the effect that it was the circuit f′ g′, h,

and e, that was tuned to the received wavelength, I misunderstood you. Is that so?

A. There is a question of degree there—that is, the elements that you have just recited mainly determine the resonant condition. Now, there are contributing causes which alter that resonant condition, to a greater or less degree.

X Q. 564. Then, to repeat, if I understood you to say that the circuit which was tuned to the received wavelength was solely determined by f′, g′, h and e, I misunderstood you?

A. You. did. I made no such statement.

X Q. 565. And you do not want your testimony interpreted as meaning that, do you?

A. That is correct.

X Q. 566. What is correct, that you do not want it so interpreted?

A. That is correct.

X Q. 567. In other words, if the value of j′ is changed and the circuit is not changed, there will have to be a readjustment of the circuit?

A. Slightly.

[fol. 1952] X Q. 568. Depending upon the amount of change in j′?

A. Yes. The answer to that question should not be misunderstood, in as much as, from the analysis which I made of that circuit, in the light of what is stated in the specification, the effect of j′ to me, from those analyses, appears to be only a very slight one—that is, the inductance j′ could be increased enormously without seriously affecting the resonant condition. However, if it was materially reduced, it might affect the resonant condition very much.

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(Tr. 1589)

X Q. 572. In other words, to analyze a circuit of this kind, as well as to analyze the antenna circuits of receivers like the SE 143, you really have to know the constants of the individual elements of the circuits, do you not?

A. That is correct.

X Q. 573. Did you ever attempt to duplicate Figure 2 of the Marconi patent, in so far, at least, as the primary cir-

cuit is concerned, for any of the tunes mentioned in the tables on page 4 of the patent?

A. Well, I have not, no.

(Tr. 1597)

[fol. 1953] X Q. 596. Now, if the condenser h, in figure 2 of the patent, is variable, then is it not correct to say that a variation of the capacity of that condenser enables the electrical periodicity of tuning of the open circuit of the receiver to be altered?

A. That is correct.

X Q. 597. I hand you Defendant's Exhibit JJ, comprising sketches made by you in the course of your direct examination; I think you made some assumption as to the relation between the position of the condenser plates and the resonance with the incoming signal; is that correct?

* * * * * * *

A. Yes, the assumption was made that the condenser was about in its mid position.

X Q. 598. That the condenser was in its mid position?

A. The circuit was resonant.

X Q. 599. And that was what circuit?

A. The circuit consisting of the condenser h and the coil j' was resonant.

X Q. 600. Regardless of the value of the antenna capacity?

A. Yes. This curve was drawn to show the relation of the current in the closed circuit to that in the antenna circuit. And, since the antenna circuit is a very high impedance circuit, as compared to the series impedence of the [fol. 1954] condenser h, and the coils j', the current in the circuit, consisting of the condenser h and the coils j', would be much greater than that in the antenna.

X Q. 601. And that sketch shows, does it not, the greatest current in the coil j' when the circuit composed of the condenser h and the coil j' is tuned to the received wavelength?

A. That is correct.

X Q. 602. In using such a circuit in a receiving antenna, where would the condenser be set to secure the loudest signal in an associated circuit with a detector in it?

A. That would be set—it would have to be adjusted by some meter or by the ear to such a position that the largest measured current or voltage is in that coil.

X Q. 603. Supposing that an adjustment would be made by ear, which is the usual way, is it not, in a receiver?

A. Yes.

X Q. 604. Would not the condenser be set midway to receive the loudest signal.

A. It would be. Well, of course, the assumption was made that, at that particular point, the largest current was flowing in that circuit. That was the basis for drawing the curve.

[fol. 1955] X Q. 605. Although the assumption was that, at that point, the circuit composed of h and j', in and of itself, was tuned to the received wavelength?

A. The actual resonant frequency to which the condenser in the mid-position would respond, would be influenced by the antenna.

X Q. 606. Then, may I repeat the question: if the operator tunes that circuit, considering that as the primary circuit of the receiver, to receive the loudest signal, would the condenser be set at the mid-point?

A. I think we are thinking about two different things here.

X Q. 607. I would be glad to know what the trouble is.

A. This curve was drawn under the assumption that, for the condenser in the mid-position, the maximum circulating current or voltage in that circuit——

* * * * * * *

X Q. 609. First tell us what the assumption is, Commander Dow.

A. The assumption is made that the circuit as a whole, including the effect of the antenna capacity, is adjusted so that the greatest current will circulate in the circuit h-j', when the condenser is in the mid-position. And this curve was drawn merely to show that the relation between the current of the antenna and the current in the wheel circuit h-j'—that is, that the current in the wheel circuit was [fol. 1956] much larger than the current in the antenna lead.

Now, this condenser h and the coil j'—are any condenser h and any coil j', provided this adjustment is made. And the result is based upon making the adjustment under the assumed condition.

* * * * * * *

This is the resonant condition of the circuit as a whole.

X Q. 610. Then the circuit h-j', of itself, and without considering the effect of the antenna, is not resonant to the received wavelength?

A. That is correct.

X Q. 611. Then I take it that your statement, which appears at page 1370 of the record, that "We assume that the resonant wavelength corresponded to that of the circuit h-j', when the condenser was in its mid-position," must be qualified, to a certain extent?

* * * * * * *

A. I do not understand why.

X Q. 612. Because, as I understand you now, the resonant wavelength corresponds to that of the entire antenna circuit, when the condenser is in its mid-position?

[fol. 1957] A. Well, here we go into that question of the relative constants; or, rather, the relative value of various constants. If you will be explicit, and tell me what the capacity of the antenna is, what the inductance of the antenna is, what the capacity of condenser h is, and what the value of the inductance of coil j' is, I think I can more readily answer. I may answer this question in terms of the picture that is in my mind, as to the relative values of things.

X Q. 613. What is that picture?

A. That the antenna has some effect on the resonance frequency; but that it is principally determined by the value of the condenser h and the coil j'.

X Q. 614. But, nevertheless, it does have some effect?

A. Yes, that is true.

X Q. 615. And, to that extent, the assumption, as I read it to you from the record, must be qualified?

A. Yes, to that extent.

X Q. 616. To that extent?

A. I do not recall at this time exactly what led up in the direct testimony to this figure, and I do not like to say something, make answers to present questions, on the basis of those questions as I hear them and attempt to read them in the light of something that was said before, with which I am not familiar. I have not even read the record in this case.

[fol. 1958] X Q. 617. Then the entire circuit shown in Ex-

hibit JJ is resonant to the received wavelength, with the condenser at its mid-point; is that right?

A. Yes.

X Q. 618. And it is at that point where the maximum current flows in the coil j'?

A. Yes, that is correct.

X Q. 619. Can we generalize that statement and say that, in a receiver having a primary and secondary circuit, to secure the best signal, the antenna circuit as a whole should be tuned to the received wavelength?

A. Under this condition here, the maximum current——

X Q. 620. And "this condition here" is what? The record does not show.

A. Assuming this structure shown in the antenna circuit of Figure 2 of the Marconi patent. When the antenna circuit as a whole is tuned to the resonant frequency, the maximum current will flow in the coil j'.

However, that is not the maximum that is obtainable, if certain elements of the circuit are—or if the tune of certain elements of the circuit are modified, as, for example, by adjusting the capacity of the condenser h and the coil j' to have a broader value. I do not know whether that is clear or not.

[fol. 1959] X Q. 621. Well, even then, the antenna circuit as a whole would still be tuned to the received wavelength, would it not?

A. Yes, that is correct.

X Q. 622. I think my general statement, then, applies to that, does it not?

A. The maximum current, but not a max-max current.

X Q. 623. When the maximum current is received, then the antenna circuit as a whole is in tune with the received wavelength, is it not?

A. That is right. But that is not the maximum current that is obtainable by better adjustment of the circuit.

X Q. 624. Yes; but even a better adjustment would still leave the antenna circuit as a whole tuned to the received wavelength?

A. Yes, that is correct.

X Q. 625. So that I still think my statement is correct.

A. I think so.

[fol. 1960] (Tr. 1605)

X Q. 628. I may have misqucted your testimony slightly. What you said, according to the record, at page 1372, is that: "As the condenser is increased in setting, from zero, the current in the circuit hj′ rises with increasing rapidity as the resonance position is reached, reaching a maximum value when the inductive reactance of same is equal to the capacitive reactance."

A. Yes.

X Q. 629. Is not the reason that the current in the coil j′ is smaller when the condenser is nearer zero because the antenna under those conditions is de-tuned—that the circuit as a whole under those conditions is detuned?

A. Yes, that is correct.

X Q. 630. And, likewise, the reason why the current in the coil j′ again decreases after the mid-point is passed, is because the antenna circuit as a whole is de-tuned, is it not?

A. Yes, that is correct.

(Tr. 1607)

[fol. 1961] X Q. 631. Commander Dow, will you refer again to Defendant's Exhibit JJ; have you that before you?

A. I have that before me.

X Q. 632. I think you testified, in connection with that exhibit that, if a loading coil such as g′ were added to that circuit, the action of the circuit would be substantially the same; is that correct?

A. Yes.

X Q. 633. And adding the loading coil would involve a change of the position of the condenser, in order to secure the maximum current in coil j′, would it not, assuming that the wavelength was unchanged?

A. It would involve changing the tuning of the condenser h, to some extent, the amount of which would have to be determined by the relative value of the various capacities and of the inductances in the circuit.

X Q. 634. Including that the coil g′?

A. That is correct.

[fol. 1962] (Tr. 1613)

X Q. 664. Have you a copy of the Pupin patent, Defendant's Exhibit KK?

A. I have a copy of the Pupin patent, 640,516.

* * * * * * *

X Q. 665. Commander Dow, what is the highest frequency mentioned in this Pupin patent?

A. 250 cycles is the highest frequency that I find specifically mentioned in the specification.

X Q. 666. Is that considered a radio frequency?

* * * * * * *

The Witness: Such frequencies are not ordinarily considered in the category of radio frequencies, however.

X Q. 667. Did you ever see a radio transmitter designed to transmit frequencies of 250 cycles?

A. I never have.

X Q. 668. Did you ever see a radio receiver designed to receive frequencies of 250 cycles?

* * * * * * *

The Witness: Not a conventional type of radio receiver.

X Q. 669. Why do you use the word "conventional" in that answer?

A. Because there are in use at the present time—or there is in use at the present time, receiving apparatus for receiving frequencies of the order of 500 cycles.

[fol. 1963] X Q. 670. The question was with respect to 250 cycles.

A. With respect to 250 cycles, I have not.

X Q. 671. What wavelength corresponds to 250 cycles?

* * * * * * *

A. It would be 1,200,000 meters wavelength.

X Q. 673. Now, I presume, in studying the Pupin patent, that you notice that on page 1, line 37, and following, he says that iron should be avoided in the coils of this circuit. Have you ever constructed a coil which, with a suitable condenser, would resonate at a frequency of 250 cycles.

A. I have never actually determined or made such a determination. However, I have constructed many coils which, with a suitable condenser, would resonate at such frequencies, coils without the use of an iron core. Coils used in the SE 1000 amplifiers, by a relatively small condenser connected across their terminals, would be resonant at such a frequency.

X Q. 674. Those have iron in the core?

A. Those are air-core.

X Q. 675. Air-core?

A. Air-core. They were computed at 1,000 cycles, without the use of a condenser.

[fol. 1964] X Q. 676. Referring to Figure 1 of the Pupin patent, what are the elements of his resonance circuits at the receiving end?

A. The condenser h, the coil j, and the inductance of a solenoid which forms a part of the responsive device M; also the condenser K, the inductance L, and the solenoid which forms a part of the responsive device N.

X Q. 677. What are the devices M and N?

A. They are electrodynamic devices, which contain a magnet and an armature, which is constrained in its motion by what appears to be a spring.

X Q. 678. Similar to a telegraph sounder?

A. Of that order, yes.

X Q. 679. Would the presence of the coil of these devices M and N have any effect upon the resonance curve of the circuits in which they are connected?

A. Yes, I would say that they would have an effect upon it.

X Q. 680. With normal devices of that character, would they have any effect on the sharpness of tuning of the circuit?

A. Yes, they are inductance coils, and all inductance coils introduce a certain amount of resistance into the circuit, so that, depending upon the amount of resistance introduced into the circuit by their presence they would have an [fol. 1965] effect upon the resonance curve of the circuit.

X Q. 681. Does that mean that, the greater the resistance of those coils, the less sharply resonant the circuit would be made?

A. That is correct.

X Q. 682. Does such a coil normally have an iron core?

A. From the nature of the nomenclature—or, rather, from the nature of the structure shown, I would assume that these coils had an iron core.

X Q. 683. And the reference to them at line 123 on page 2 of the patent as "Electromagnets" tends to bear that out, does it not?

A. Yes, that is correct.

X Q. 684. And is it not true that, with a coil of that type, the resistance would normally be considerably higher than with an air-core coil?

A. Yes. There is nothing here to indicate the relative values of resistance of the solenoid plates, and the resistance of the inductances I and L. At these frequencies, it is quite possible that the resistances of the inductances I and L were fairly high.

X Q. 685. And that, again, would tend to reduce the sharpness of the tuning of the circuit, would it not?

A. Yes, that is correct.

[fol. 1966] X Q. 686. Can you state whether or not, as of either 1895 or 1900, there would have been any problems involved in designing the circuit H-I-M, so that it would have a sharp resonance characteristic?

A. I cannot state. I cannot give a very specific reply to that question, because I am not entirely familiar with the literature on the particular subject under discussion at this time, which existed at the time in question. However, I note, beginning with line 97 on page 1 of the specification, the statement: "Electromotive forces of different frequencies, say of 150 and 250 vibrations per second." That is a 2-to-1 ratio in wavelength. If those frequencies were used, the resonance characteristics of the circuits might not have been so bad.

X Q. 687. Now, will you refer to Figure 2 of the Pupin patent. As I understand it, there are primary and secondary circuits there; is that correct?

A. That is my understanding.

X Q. 688. Which is the primary circuit of the left-hand of the two groups of circuits?

A. The circuits H-I I understand to be the primary circuit.

[fol. 1967] X Q. 689. How is that circuit tuned?

A. That circuit is tuned substantially to resonance, according to ~~my~~ understanding of this patent.

X Q. 690. By means of what element or elements?

A. Capacity H is made resonant with the inductance of the coil I, plus the reflected inductances of the secondary circuit.

X Q. 691. Now, will you please look at page 2 of the patent, and particularly lines 95 to 100, and state whether that induces you to change your answer in any way.

A. It does not lead me to change my answer.

X Q. 692. Does not the patent there say that he tunes the circuit H-I by means of the circuit H′-I′-I?

A. That is correct. However, he states, beginning with line 94: "Referring now more particularly to Fig. 2, which represents the receiving end of the line and can be hitched onto the transmitting end shown in Fig. 1, instead of the receiving end shown in that figure, I show a resonance circuit H-I and K-L", which I mentioned.

He states further: "But I control the frequency of H-I by means of a circsuit H′-I′-I".

In giving my answer, I stated that the capacity H was made resonant with the inductance I, plus the reflected inductances of the secondary circuit.

[fol. 1968] X Q. 693. And, according to the patentee, it is the reflected inductance, or whatever may be reflected from the secondary circuit, that tunes the primary circuit, is it not?

A. That is correct.

* * * * * * *

(Tr. 1629)

X Q. 694. Have you got a copy of the Fessenden patent before you?

A. I have.

X Q. 695. I think you referred in your direct examination primarily to Figure 1; is that correct?

A. That is correct.

X Q. 696. You said that the antenna circuit of the transmitter of that patent was tuned. What are the tuning elements of the transmitter antenna circuit?

A. He has a capacity body, 5-A, a conducter 5, a condenser 18, and the ground lead, which is not identified by reference character. That I interpret to be or understand to be the tuned element or tuned system.

X Q. 697. Is there any lumped inductance in that antenna circuit?

A. There is none.

[fol. 1969] (Tr. 1630)

X Q. 704. Well, as compared with Figure 1 of the Marconi patent, the spark would be much sharper under the Marconi patent, would it not?

A. That is correct.

X Q. 705. In the receiver, Figure 1, of the Fessenden patent, how many tuned circuits are there?

A. Well, he shows a resonant circuit, consisting of the condenser 19 and the two coils 7. Now, the inductance of the

antenna and the capacity of the antenna are lumped into that and form a part of the whole resonant system that he has.

X Q. 706. That is, in your opinion, the antenna circuit is a tuned circuit; is that correct?

A. Yes. I would place that as a tuned circuit.

X Q. 707. Is there any other tuned circuit in the receiver?

A. There is none.

[fol. 1970] (Tr. 1631)

X Q. 709. Now, I understood you to testify that there was tuning in the receiver circuit. To what is that circuit made resonant?

A. The circuit—I understand the specification to disclose that the condenser 19 is selected to be of such value that the maximum current flows in the inductances 7.

X Q. 710. The maximum current at some particular wavelength?

A. Yes; at the wavelength of the transmitting station.

X Q. 711. That is, the wavelength as determined by the capacity of area of 5-A, wire 5, and the condenser 19 and the ground connection?

A. That is correct.

X Q. 712. Now, I notice that Fessenden says, on page 3, at line 25, that the shunt circuit of the transmitter must be tuned to the receiving conductor. In the first place, what is the shunt circuit of the transmitter?

A. I will now read out of the specification something that I have not observed before, in that apparently the wavelength of the oscillations generated in Fessenden's transmitter is determined to a substantial extent by the inductance of the secondary coil of his transformer and the condenser 18. He states: "It is preferred to place a shunt [fol. 1971] circuit containing a condenser across the terminals of the induction coil at the sending station, for the purpose of maintaining sustained radiation. This shunt circuit must be tuned to the receiving conductor."

X Q. 713. What kind of an instrument is that which is marked "I" in Figure 1?

A. An induction coil. This would indicate to me that he was using relatively long wavelengths, since the inductance of the secondary of his coil must have been very great.

X Q. 714. What do you mean by "relatively long", in this instance?

A. Well, as distinguished from waves of the order of 100 to 200 meters that we have assumed he was using, from the structure of his circuit, which consisted merely of the vertical conductor of the capacity 5-A and the condenser 18, in series to ground.

X Q. 715. An induction coil is normally a pretty large coil, is it not?

A. Yes.

X Q. 716. Would it not, in practice, be so large that it probably would not act as an inductance at all on a transmitter frequency?

A. I do not know that I would say that. I do not have any justification for saying that.

[fol. 1972] By the Commissioner:

X Q. 717. Has that an iron core?

A. Yes.

X Q. 718. Would it not act more as a choke-winding, than anything else?

A. Well, that would depend largely upon the construction, the exact construction, of such a secondary coil, whether it was close to the iron core or whether it was quite some distance away from it. It would depend upon the size of the wire.

By Mr. Blackmar:

X Q. 719. Are there any instructions in the patent as to how that coil should be constructed?

A. It states that the apparatus employed at the sending station may be similar to that now in use for the generation of electromagnetic waves and consists of an induction coil 1, having its primary coil in circuit with a generator 2, said circuit having a make-and-break mechanism 3 included therein.

X Q. 720. Would you know what kind of coil would be "similar to that now in use for the generation of electromagnetic waves"?

A. Well, I have used induction coils of various sizes and designs. I have no information at hand to indicate the exact construction of the coils which he used.

[fol. 1973] By the Commissioner:

X Q. 721. The coil that he uses here might be termed the conventional spark coil, might it not?

A. It might, yes, sir.

XQ. 722. Are you sufficiently familiar with radio of the early days to give an estimate of the length of spark that would be necessary to the practice of the Fessenden invention here?

A. I would say that the Fessenden spark, with a length from an eighth of an inch to half an inch or one inch, was probably used. At least, I would not expect him to use a longer spark than that, with the condenser across the secondary.

XQ. 723. Well, without the condenser across the secondary, the free length of a spark obtained from the secondary terminals would be much longer?

A. Yes.

XQ. 724. On that assumption, how long a spark would you think would be employed? What I am getting at is this—correct me if I am wrong—the spark coils of these early times were rated as a one-half inch spark, or one-inch spark, or 2-inch spark, and so forth.

A. Well, I used a spark coil for radio, about 1910, that had a spark length of about an inch and a half on the open circuit and about a quarter of an inch when the condenser [fol. 1974] was connected across it. I have also seen spark coils at around that time that were able to break down gaps up to—used for radio—that were able to break down gaps up to 4 or 5 inches, when the condenser was not across the secondary; and something in the order of about a quarter of an inch to an inch when the condenser was used.

XQ. 725. Have you any idea of the number of turns on the secondary winding for an inch length of spark on convential equipment?

A. That is ordinarily very great.

XQ. 726. Would 50,000 turns be about right for each inch of spark? Now, do not take my statement as correct. I may be wrong. But that is an impression that I had. That figure comes to my mind because, in those early days, I built some spark coils myself. It is a good many years ago, however.

A. If he had 100 turns on his primary coil and used 50 volts for his battery and had 50,000 turns on the secondary coil, he would get something of the order—and I am using transformer theory on this—something of the order of 25,000 volts, between spheres of a centimeter in diameter.

That would be equivalent to a spark gap of about a centimeter.

[fol. 1975] X Q. 727. My estimate, if anything, then, is perhaps conservative?

A. Yes. Of course, it is very difficult to look at a sketch of this kind and say exactly what goes on in the circuit. Now, with these 50,000 turns in the secondary, or whatever he had, there was an enormous distributive capacity, which always enters into the picture, and concerning which I have no knowledge. I can only take this statement which Mr. Blackmar pointed out, to indicate that the secondary induction coil did play a part in determining the wavelength of his circuit.

By Mr. Blackmar:

X Q. 728. Of course, there is a possibility that his theory of how it operated was incorrect?

A. That is correct; as I understand sometimes is the case.

X Q. 729. And, from your examination of Figure 1, and your general familiarity with induction coils, as they were used to generate radio frequency, do you not think that probably your first answer as to what determined the frequency of that circuit, was more correct than Fessenden's theory of what determined that circuit?

A. Well, that is a little difficult to answer, because Fessenden worked on those, and I have not.

[fol. 1976] X Q. 730. You assume that he worked on it, but you do not know. Many patent applications are drawn on paper, as I think you know, without having them tried out. But, leaving aside what you think Fessenden may have found: if a circuit, such as the transmitter in Figure 1 of that patent, was utilized with an induction coil of an ordinary type, what would determine the transmitted frequency or transmitted waves?

A. Well, upon this statement that you have pointed out——

X Q. 731. I am asking you from your own experience.

A. From my own experience, it would be determined very little by the secondary of the induction coil, and principally by the elements which I have pointed out.

X Q. 732. That is, by 5, 5-A, and 18, and the ground connection?

A. That is correct.

X Q. 733. And, on that basis, would it be fair to say that you are doubtful as to the accuracy of Fessenden's statement as to what determined the wavelength of the signal?

A. Well, as I said before, I am not familiar with the detailed construction of his coil. He may have had an induction coil there that had very few turns on his primary. He may have been using very high primary current. He could maintain a fairly high ratio of turns without, perhaps, as [fol. 1977] many secondary turns as was conventional on coils with which I am familiar.

By the Commissioner:

X Q. 734. In the absence of any specific specifications by Fessenden of the type or character of coil to be used, can you answer the question with reference to what you would consider as the conventional spark coil, as of that time?

A. I would say that, from what I understand of the conventional spark coils of the period between—the period proceding 1910—that a wavelength would be determined by the elements which I detailed before Mr. Blackmar pointed out this statement of Fessenden.

By Mr. Blackmar:

X Q. 735. Rather than by what Fessenden determines the shunt circuit to be?

A. Yes.

X Q. 736. Now, he says at the same place that the shunt circuit must be tuned to the receiving conductor. What do you understand he means by "the receiving conductor"?

A. Well, I understand that he means the receiving system as a whole.

X Q. 737. As a whole?

A. Yes; that is, he sent out waves, transmitted waves, of such a length that his receiving system responded best to it.

[fol. 1978] X Q. 738. And he meant, did he not, the receiving system as a whole, rather than the circuit made up of the coils 7 and 7 and the condenser 19?

A. Yes, the receiving system as a whole includes those elements.

X Q. 739. I understand your direct testimony to be that it was the secondary 7 and 7 and 19 and the wires 6——

A. I think you will find, if you will refer back to my direct testimony, that I stated that the capacity of the element 6,

which is the receiving conductor, was considered, in that answer, to be lumped with the capacity 19.

X Q. 740. So that what is tuned to the received wavelength, if anything, is not only the circuit 7-7-19, but anything associated with it.

A. Yes.

X Q. 741. That might affect the tuned frequency?

A. That is correct.

X Q. 742. Now, will you please point out where, in the patent, Fessenden says that the receiving conductor as a circuit or receiving conductor, as he calls it, is tuned to the received wavelength? I would like you to be specific, because I will be interested to see whether that statement is in the application as filed.

[fol. 1979] A. Fessenden states, beginning with line 101 on page 2 of the specification: "As shown in Fig. 1, a condenser 19 may be connected in shunt with the field coil or coils 7, for the purpose of obtaining as large a current as possible, in the field coil 7, as this increase in current will give a greater torque to the ring 8."

X Q. 743. Does that, to you, of itself, direct you to tune the receiving conductor?

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(Tr. 1641)

The Witness: I interpret that to mean that he selected such a value of capacity there that his receiving system as a whole was resonant to the wave that he was interested in.

X Q. 745. Now you may complete your answer to the previous question.

A. Fessenden continues, beginning with line 106, in a rather long explanation, showing the results which he got, indicating a phenomenon similar to that which I pointed out in one of my sketches of yesterday, wherein the current in the antenna circuit was larger than the current in the receiving conductor.

[fol. 1980] X Q. 746. Is there anywhere in that rather long explanation where there is a specific direction to tune the receiving conductor?

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A. No specific mention is made of it. However, all the explanation which he gives is that which would apply to a resonant circuit, or a shunt circuit connected to the terminals of a generator or source of alternating electromotive force.

X Q. 747. That is, according to your opinion, he describes the results which would be obtained by tuning; is that what you mean?

A. Yes, that is correct. That is, by having a resonant circuit which is responsive to the incoming oscillations.

X Q. 748. In the receiver of Fig. 1, as I understand it, there is no tuned secondary circuit, is there?

A. There is not.

X Q. 749. Will you refer to Figure 2 of the patent, please? Is a secondary circuit shown there?

A. I find no tuned circuit in Figure 2.

X Q. 750. Is there a secondary circuit, however?

A. Oh, yes, there is a secondary circuit.

[fol. 1981] X Q. 751. But there is no shunt condenser there in the primary or secondary circuit, either, is there?

A. No, there is not.

X Q. 752. In fact, in that figure, no means are shown for varying or tuning either of the circuits?

A. There are none shown. And I find no mention of such a structure in that portion of the specification relating to Figure 2.

X Q. 753. In any other figures of the specification of the Fessenden patent, do you find a secondary circuit? That is, in Figures 3, 4, or 5?

A. I interpret Figures 3, 4, and 5 as showing various arrangements for transforming electromagnetic waves into recordable action.

X Q. 754. Do you find any secondary circuits in any of them?

A. I do not.

[fol. 1982] (Tr. 1647)

X Q. 769. I understood you to say that the circuits of a receiver constructed according to the Marconi patent would be more selective than a receiver made in accordance with Figure 1 of the Fessenden patent?

A. I would say so, yes.

X Q. 770. And that would be also true of the defendant's receivers, would it not—that they were more selective than the receivers shown in the Fessenden patent?

A. That is true.

[fol. 1983] (Tr. 1650)

Mr. Blackmar: I think possibly if he hears them he will bear them in mind.

My first question is the one just asked—what is there which is common to the Pupin patent and to the defendant's receivers which is not also present in the circuit of plaintiff's Exhibit 454 for Identification?

The second question will be: What is there that is common to the Fessenden patent and the defendant's receivers which you do not find in Plaintiff's Exhibit 454 for Identification?

And the third question will be: what is there that is common to the Pupin patent and to the defendant's receivers which you do not find in Plaintiff's Exhibit 455 for Identification.

The fourth question is: What is there that is common to the Fessenden patent and to the defendant's receivers which you do not find in Plaintiff's Exhibit 455 for Identification.

[fol. 1984] (Tr. 1652)

X Q. 781. I believe there are four questions on the record.

(The first question referred to, as recorded on page 1650, minutes of July 18, was read by the reporter).

A. My answer to this question applies to those of defendant's structures in which a condenser is shown connected in parallel with the primary inductance system. With respect to what appears in the Pupin patent and defendant's structures which does not appear in the sketch marked Plaintiff's Exhibit 454 for Identification, I find no detector in the sketch.

X Q. 782. Then, am I correct in understanding that, in so far as radio frequency circuits are concerned, there is nothing which is common to Pupin and to the defendant's receivers which you have mentioned, that is not also present in Plaintiff's Exhibit 454 for Identification?

A. That is correct.

X Q. 783. Now, will you listen to the second question which I put on the record yesterday afternoon.

(The second question referred to, as recorded on page 1650, minutes of July 18, was read by the reporter.)

A. My answer to this question applies also only to those of defendant's structures in which the condenser is shown in parallel with the primary inductance system. I find no [fol. 1985] detector in the sketch marked Plaintiff's Exhibit 454 for Identification.

X Q. 784. Then am I correct in understanding that, if the question had been confined to the radio frequency circuits, your answer would have been that you find nothing else common to Fessenden and to the defendant's receiver that you described, which is not also in Plaintiff's Exhibit 454 for Identification?

A. That is correct.

X Q. 785. Now will you listen to the third question, please?

(The third question referred to, as recorded on pages 1650 and 1651, minutes of July 18, was read by the reporter.)

A. My answer to this question also applies to those of defendant's structures in which a condenser is shown in parallel with the primary inductance system. I find no detector in the sketch marked Plaintiff's Exhibit 455 for Identification.

X Q. 786. Then am I correct in understanding that, if the question had been limited to radio frequency circuits, you would find nothing which is common to Pupin and to the defendant's receivers which you have mentioned, which is not also in Plaintiff's Exhibit 455 for Identification.

A. That is correct.

[fol. 1986] X Q. 787. Now, will you listen to question No. 4 which I asked yesterday afternoon.

(The fourth question referred to, as recorded on page 1651, minutes of July 18, was read by the reporter.)

A. My answer to this question applies to those of defendant's structure in which a condenser is shown in parallel with the primary inductance system. I find no detector in the sketch marked Plaintiff's Exhibit 455 for Identification.

X Q. 788. And again am I correct in understanding that,

if the question had been limited to the radio frequency circuits, you find nothing which is common to the Fessenden and to the defendant's receiver, which you have mentioned, which is not also in Plaintiff's Exhibit 45 for Identification?

A. That is correct.

X Q. 789. Now, in limiting your answers to the defendant's receivers in which the parallel connection is shown, would your answer be the same if they also applied to the so-called Group III type receivers, when the connections were so made at the time that the antenna condenser was connected in parallel?

A. The answers would reply.

[fol. 1987] (Tr. 1665)

X Q. 824. Now, in connection with Defendant's Exhibits NN and OO, and referring to the figures showing the parallel connection with the antenna condenser, you pointed out, as I understood it, a similarity between those circuits and the receiver circuit of Figure 1 of the Fessenden patent, in that, in all cases, there was more than one coil in parallel with the condenser. Do you remember so testifying?

A. I do.

X Q. 825. Why, in your opinion, did Fessenden show two coils marked 7 in Figure 1?

A. These coils 7 are on either side of his secondary coil 8, and he desired to place his secondary coil at a point in the field of maximum electromagnetic intensity.

X Q. 826. Is there any such function in the use of two or more coils in any of the receivers of the type of SE 950 or SE 1012?

A. No. Fessenden's detector device was incorporated as a part of his coil system, and there was no need to provide such an arrangement in connection with the use of the detectors which are employed in the receivers under consideration.

[fol. 1988] X Q. 827. Then, when you referred to this similarity, it was merely a similarity in that each used more than one coil; is that correct?

A. That is correct.

X Q. 828. Not a functional similarity?

A. You are correct.

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(Tr. 1668)

X Q. 841. Now, is it not correct that an inductance coil in a radio circuit may act either as an inductance or as a capacity, depending upon the wavelength applied to it?

A. That is correct; depending upon the design of the coil and the wavelength.

X Q. 842. And the same is true of the condenser, is it not?

A. At frequencies corresponding to wavelengths of one meter, certain capacities might act as inductances.

X Q. 843. And, in analyzing a radio frequency circuit to determine the effect of the elements on the current in the circuit, that is ordinarily done by considering the reactance of it, is it not?

A. The reactance or resistance.

X Q. 844. And if you know the reactance it does not make any particular difference whether the instrument is a coil or a condenser, does it, as far as analyzing the operation of the circuit is concerned?

* * * * * * *

A. That is correct.

* * * * * * *

[fol. 1989] (Tr. 1673)

Mr. Blackmar: With Mr. Edwards's permission I offer in evidence as Plaintiff's Exhibit 453 the sheet of sketches which has been marked Plaintiff's Exhibit 453 for Identification.

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(Tr. 1674)

[fol. 1990] Mr. Blackmar: I also offer in evidence as Plaintiff's Exhibit 454 a diagram which has previously been marked Plaintiff's Exhibit 454 for Identification; and I call the Commissioner's attention to the fact that it is a partial copy of Plaintiff's Exhibit 79, showing the Telefunken type of receiver, which the Court of Claims held infringes Claim 16 of the Marconi patent, which appears opposite page 180, of Volume 1, of the printed record in the main case.

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Mr. Blackmar: I also offer in evidence as Plaintiff's Exhibit 455 the diagram which has heretofore been marked

Plaintiff's Exhibit 455 for Identification, and I call the Commissioner's attention to the fact that it is a partial copy of Plaintiff's Exhibit 95, appearing opposite page 183 of Volume I of the printed record herein, which is representative of the Kilbourne and Clark types of apparatus, which the Court of Claims also held infringed Claim 16 of the Marconi patent.

GEORGE H. CLARK

(Tr. 1930)

[fol. 1991] 147 Q. With this receiver did you ever compare its operation, utilizing the series position of the condenser, with its position utilizing the parallel connection of that condenser?

A. Yes, I have made a test many times.

148. Q. Have you any contemporaneous records of such tests?

A. Yes.

149. Q. Will you describe briefly what the records are, not their contents, but what they are?

A. The record that I have with me is a record of tests made during a trip which I made on the U. S. S. Arkansas in December of 1912 from Key West to Colon. The tests were made either on the ship at Key West or en route to Colon.

150. Q. Have you a record of the test made by you December 19, 1912?

A. Yes, I have. I have the original log and I have the general report covering the whole test.

151. Q. Referring to the original log, where was that test made?

A. This was made with the receiver at the naval station at Key West, Fla., on December 19, 1912.

152. Q. What is the original document that you have before you?

A. This is the original log that I made at the time of the test.

153. Q. And the test to which you have referred appears on page what of the log?

[fol. 1992] A. It appears on page 12 of the log, the page which is numbered 12.

154. Q. Have you any independent recollection of that test?

A. Yes, I have. I have a memory clearly.

155. Q. What was the result of the test?

A. I found that with the shunt connection the signals were approximately 5 times as loud as with the series connection on the primary condenser.

156. Q. What type signals were you receiving and from where?

A. Arc signals from the transmitter at Arlington.

157. Q. And this page 12 is a contemporaneous record of that test, is that right?

A. Yes, made at the time.

158. Q. Now, referring to that, will you please point out what that shows with respect to a comparison?

A. The circuit of each test is shown schematically at the left.

On the first test and second test I used the parallel condenser in the primary, No. 1 test having two coils in the loading coils and No. 2 test having three coils. I obtained a shunt across the telephone for a unit signal of 15 ohms in the first test and a unit signal of 7 ohms in the second test, the second one being approximately twice as loud as the first and being the better of the two for the shunt connection. I have a star and arrow marked on the side of the diagram which means I called attention to that one as it would be the one which I would select in making a later [fol. 1993] report. It was the one of the two to be considered as standard.

159 Q. When were the star and arrow put there?

A. At the time or shortly before my report was written.

The third was with the series condenser. There I obtained a shunt of 50 ohms across the telephone for unit audibility. So with a shunt of 7 ohms for the parallel connection in the condenser as against 50 ohms for the series connection in the condenser it shows that the signals were approximately 5 times as loud for the parallel connection.

Mr. Blackmar: I offer in evidence a photostat copy of page 12 of the log book, which is the page to which the witness has just been referring, defendant making no objection to the use of the photostat copy, as I understand it.

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(Tr. 1933)

Mr. Blackmar: I ask that it be marked Plaintiff's Exhibit No. 462.

* * * * * * *

(Tr. 1934)

162. Q. You said something about taking that receiver on a trip with you from Key West.

A. Yes.

[fol. 1994] 163 Q. What ship?

A. On the U. S. S. Arkansas.

164 Q. Did you use that receiver and test it while you were on shipboard?

A. My orders were to compare the arc and spark transmitters at Arlington at the same time, during day and night, for strength of signal from each.

165 Q. Did you subsequently make a report of your observations?

A. Yes, I did. I have a copy here dated December 29, 1912.

166 Q. What did you do with the original?

A. The original was turned into the Navy Department as my report.

167 Q. Does that report contain any direct comparison between the use of the series and the use of the parallel connections?

A. Yes, it does.

168 Q. Where in the report?

A. Paragraph 2 on page 1. The first statement is made that it was the Federal Telegraph receiver and that said receiver had variable and fixed condensers in both primary and secondary circuits, and that the primary capacity was capable of use either in series or in parallel with the primary inductance.

On page 2, halfway down the page, is the following: [fol. 1995] "In general, signals were best obtained with the entire primary inductance, (3 coils), no loading coil, parallel connected primary condenser, No. 2 coil secondary inductance, and the maximum obtainable stopping condenser."

[fol. 1996] LYNDE PHELPS WHEELER produced, sworn, and examined as a witness on behalf of the defendant.

(Tr. 2102)

1 Q. Will you please state your full name, age, residence, and occupation.

A. My name is Lynde Phelps Wheeler. I live in 2317 20th Street, N. W., Washington, and I am Chief of the Technical Information Division of the Engineering Department of the Federal Communications Commission.

2 Q. And your age, Doctor Wheeler.

A. I have got to stop and calculate. I am 65 years old.

3 Q. Do you have any personal interest in the outcome of this suit, Doctor Wheeler?

A. No, none whatever.

4 Q. Will you briefly outline your education and experience in connection with radio matters.

A. I received my bachelor's and doctor's degrees at Yale University, the first, in 1894, the second, in 1902. From the time of my graduation in '94 until 1926, I taught electrical engineering subjects and mostly physics, at Yale University. In 1926, I went to the Naval Research Laboratory, here in the District, as assistant superintendent of the radio division and superintendent of the consulting division where I stayed until 1933. For the next three years, I was a consulting physicist in private practice, and in 1936 I went to my present position. During my teaching at Yale I initiated and organized the instruction in high-frequency phenomena and in vacuum-tube theory. In 1917, I was one of [fol. 1997] the organizers of the Signal Corps School for training officer candidates, which was established at Yale, and I taught in that school. From 1919 to 1926, I was in charge of a considerable portion of the work of instruction with the officers of the Army and Navy who were sent to Yale University for post-graduate work in communication engineering. I have taught courses in radio engineering at the Bureau of Standards and at the graduate school of the Department of Agriculture. At the Naval Research Laboratory, my duties were largely directing research on various phases of the radio art. While in the naval service and also since I have been with the Commission, I have been active in the preparatory work of this Government for international conventions on radio and communication sub-

jects. I am a fellow of the American Physical Society and of the Institute of Radio Engineers. I have served on several committees of the Institute, and I am at present the chairman of the standards committee of the Institute, and also a member of the board of editors of the proceedings of the Institute.

5 Q. In the course of your experience, have you had extensive practice in the making of mathematical calculations concerning radio parts and circuits?

A. I have done a fair amount of that sort of work.

6 Q. Are you familiar with the Marconi patent No. 763,-772, involved in this suit?

A. I have read it a number of times with considerable care. I think I can say I am familiar with it.

[fol. 1998] 7 Q. With reference to Fig. 2 of the patent, can you state, briefly, whether the specification of the Marconi patent discloses data necessary to enable calculations to determine with good accuracy the resonance frequencies of the receiver tunes which are summarized in the tables on page 4 of the patent specification?

A. As I read the specification, there is sufficient data for getting approximate values, with one exception: There is not sufficient data given to determine coefficient of coupling between the primary and the secondary circuits; otherwise, there is sufficient data.

8 Q. With reference to the secondary circuit of Fig. 2, do you find that that circuit is a circuit of high impedance or a circuit of low impedance?

A. A circuit of high impedance.

9 Q. Does that have any bearing on the answer you gave to the last question?

A. It justifies the assumption that the coupling is very loose between the two circuits, in which case the higher powers of the coefficient of coupling which occur in the general theory of coupled circuits, which is here applicable, can be made, and very approximate calculations as to the resonance frequency of the primary circuit can be determined.

10 Q. In connection with your study of this Marconi patent, have you calculated the value of the effective inductance of the antenna of Fig. 2 for tunes 1, 2, 3, and 4?

A. I have.

[fol. 1999] 11 Q. And the capacity of the antenna of Fig. 2 for the same tunes?

A. I have.

12 Q. And have you calculated the inductance of the loading coil g′ for tunes 1, 2, 3, and 4?

A. I have.

13 Q. And the inductance of the coupling coil j′?

A. I have.

14 Q. Did you find the data given sufficient to give close results in the calculations of these circuit constants or circuit values?

A. Depending on the meaning of the word "close", yes.

By Commissioner Gordon:

15 Q. Suppose you define that, so that this question may not be left open.

A. That, as to the coils, is sufficient to determine their inductances. The data with respect to the antenna are, I should say, insufficient for very accurate determination of the antenna constants, but sufficient to give an approximation which will be valid for practical purposes.

By Mr. Houghton:

16 Q. And from the calculations which you have just mentioned, have you determined the values of resonance frequency or wave-length indicated for the antenna circuit of Fig. 2?

A. I have.

[fol. 2000] 17 Q. For each of the conditions of the receiver, under tunes Nos. 1, 2, 3, and 4, in the table on page 4?

A. I have.

* * * * * * *

(Tr. 2106)

18 Q. Will you briefly explain what your determinations are with respect to the operation of Fig. 2 for the respective tunes indicated in the table on page 4.

Mr. Blackmar: Operation in what respect, Mr. Houghton?

The Witness: You took the question out of my mouth.

By Mr. Houghton:

19 Q. Operation with respect to resonance frequencies of response of the overall antenna circuit and any subsidiary

elements of the circuit which you have found it desirable to consider.

A. That is rather a large question. I think I may answer it best by explaining the method by which I arrived at my results. I first determined the equivalent inductance and capacity of the antenna as given from the specifications, by formulae as given in the circular, 74, of the Bureau of Standards, pages 73 and 75.

* * * * * * *

[fol. 2001] (Tr. 2107)

22 Q. Now, will you continue with your explanation.

A. After determining the approximate antenna constants from the height and diameter of the antenna wire as given in the specification, and having computed, as previously testified, the inductances of the coils, I applied the general theory of coupled circuits, assuming that the coupling is so small that its square can be neglected in determining the values of the resonant frequencies or wave-lengths of the primary circuit. Do you wish me to give the values that I found for that, or any more detail as to the method of calculation?

23 Q. I believe it will be instructive to continue, and give the values as you think necessary to explain the determination of the resonance frequencies of response of the antenna circuit in the several tunes.

A. Corresponding to the tune labeled "No. 1", under "Receiving-Station", in the table on page 4, I find that the wave-length response is approximately 206 meters; that corresponding to tune No. 2, approximately 303 meters; that corresponding to tune No. 3, approximately 199 meters; and that corresponding to tune No. 4, 364 meters. I calculated, in addition, since, in this patent for tune No. 3, the table reads, "up to 21 turns".

24 Q. Of what coil?

A. Of the coil g′; two additional values, one where there were no turns in g′, and one where there were 10 turns [fol. 2002] in g′. Where there were no turns, the resonant wave-length turns out to be 142, approximately; for 10 turns, 166, approximately.

25 Q. Of the tunes Nos. 1 to 6, set forth in the table for the receiving station, on page 4 of the Marconi patent, the table states that condenser h is omitted in tunes 1, 2, 5,

and 6. Under those conditions, what type of circuit is the antenna circuit, for tunes 1, 2, 5, and 6?

A. It is a series-tuned circuit, tuned by varying the inductance g'.

* * * * * * *

(Tr. 2109)

27 Q. What sort of resonance determines the resonant wavelength of response of the antenna circuit in tunes 1, 2, 5, and 6, where condenser h is omitted?

A. It is a case of what is technically known as "series resonance".

28 Q. With reference to tunes 3 and 4 of the receiving-station table, on page 4 of the patent, did you make calculations to determine what sort of resonance affects the tuning of the antenna circuit of Fig. 2 for those tunes?

A. As previously testified, I calculated the resonant wave-lengths or frequencies for cases 3 and 4. I then calculated the resonance frequencies having simple series circuit con- [fol. 2003] sisting of the antenna, coil g', and the condenser h, alone; that is, omitting the primary of the transformer j'. These wave-lengths thus calculated I find to be substantially the same as those previously calculated with j' in the circuit; and hence I draw the conclusion that the tuning of the antenna circuit with j' in the circuit is essentially simple series tuning.

* * * * * * *

(Tr. 2110)

A. (Continuing:) Just in the calculations with j' in the circuit, to which I testified some moments ago, I think it should be pointed out that there are, as it is a simple coupled circuit, two resonant frequencies of response, as there always are in a case of coupled circuits. I have read into the record only one of those two frequencies for tunes 3 and 4. The second frequency, which is essentially that of the circuit including j' and h alone, remains constant, for all of the tunes which I have calculated, to within 13 parts in 1,600, at a value of about 1,640 meters. This wave-length or frequency is not the frequency to which the whole antenna is tuned, and signals arriving of that frequency, 1,640 meters, or at that wave-length, would not be readily passed on to the detector, because of the relatively high

impedance of the primary circuit for that frequency; whereas signals arriving of the frequencies previously testified to would be so passed on.

* * * * * * *

[fol. 2004]

(Tr. 2111)

30 Q. I will ask the witness if I am not correct in assuming that, if the coil j′ is omitted from this circuit, there is no coupling existing to the closed receiving circuit. In other words, would not this answer of necessity assume an inoperative circuit?

A. If the coil j′ is physically omitted from the circuit, of course there is no coupling shown between primary and secondary of the oscillation transformer.

* * * * * * *

(Tr. 2112)

31 Q. Doctor Wheeler, you have just testified that with the coil j′ connected in the circuit, the resonance frequency of tuning of the overall antenna circuit is substantially the same as the resonance frequency of the series circuit comprising the antenna, the loading coil g′, the condenser h, and the earth connection E. Will you explain why this is so.

* * * * * * *

(Tr. 2113)

A. There are, as I see it, two reasons: one, the looseness of the coupling between the primary and secondary circuits; and, two, the relatively large value of the inductance of the primary of the transformer j′.

[fol. 2005] By Commissioner Gordon:

32 Q. Can you indicate, for the purpose of the record, upon what you base your "loose coupling"?

A. The reasons for the loose coupling, assuming a loose coupling?

33 Q. You did assume a loose coupling, I take it.

A. That there was loose coupling, yes. I have stated in evidence before that the coupling was so loose that the square of the value of the coefficient of the coupling was negligible. The reasons for assuming loose coupling here are, first, that the secondary circuit has a relatively high

resistance detector in series in it; second, that the reactance of the secondary coil j^2 is also high, the whole thing resulting in an impedance of the secondary which is very high. Now, the coefficient of coupling depends inversely upon the impedance, as it is taught in all the theory of coupled circuits; and hence, if that impedance is high, the coupling is small. From the data given, it is impossible to calculate the coefficient of coupling with any accuracy, but those considerations which I have just mentioned seem to me conclusive that the coupling must be very loose.

34 Q. Did Marconi suggest that coils j^2 be wound directly over the coils j'?

A. He shows four different forms, in Figs. 5, 6, 7, and 8, of that transformer. In three of them, viz., Figs. 5, 6, and 8, the secondary is wound over the primary. That in Fig. 7 [fol. 2006] shows the primary wound over the secondary.

35 Q. How about the magnetic lines of force in that type of winding?

A. Where the primary is wound over the secondary?

36 Q. No, where the two coils are, of the primary and the secondary, coincidental?

A. They are wound, one over the other. They are not actually coincidental in space. If they are right close, there, why, in the absence of magnetic materials in the coil, there is a considerable leakage of lines of force, to use a technical bit of slang. That is, all of those which originate in the primary do not cut through the secondary, so that the coupling is necessarily what is called "loose". A tight coupling would correspond to the majority of the lines of force from the primary, threading the secondary, much more than the majority.

37 Q. I do not know whether I understand your last answer. Do you mean that a tight coupling is exemplified by the fact that the primary and secondary circuits have in common a majority of the lines of force, and that a loose coupling does not have the majority but has a considerable leakage? Is that the sum and substance of your last answer?

A. Well, of course, coupling in any case is affected by the one coil, to be in the field of another coil. Now, you can put two coils far apart, and relatively few of the number of lines of force from the primary will go through the secondary.

[fol. 2007] 38 Q. And that is "loose coupling"?

A. That is "loose coupling".

39 Q. Now, if you have the two coils wound right over each other, can you get them any closer together, winding one coil directly over the other?

A. There is the thickness of the insulation between the two. It is difficult to get them actually in contact, of course.

40 Q. Oh, I know that.

A. Yes.

41 Q. You have got to follow the practical considerations, such as insulation, and something to wind them on; but I am just asking as a matter of curiosity if you can get coils, the primary and secondary coils, any closer together, or any tighter coupling, by winding one over the other.

A. Yes, it would be possible.

42 Q. How would you do that?

A. By interleaving the windings for the primary and the secondary, on the same form, winding the two together simultaneously.

43 Q. And that would be a tight coupling, in your opinion?

A. Not all of the lines would go through, in that case, even, unless we put some magnetic material in to concentrate the lines of force.

44 Q. I am trying to find out what you mean by a "tight coupling". You have exemplified what you mean by a "loose coupling"; that is, where the coils were separated; [fol. 2008] and in giving your answer, you held your hands wide apart. I think that is a good example of loose coupling. Now, I am trying to find what your opinion is of a tight coupling. I always thought that a tight coupling was where the coils were wound directly over each other, or coincidentally with each other.

A. Not necessarily so.

* * * * * * *

(Tr. 2117)

45 Q. Will you please explain how a loose coupling occurs in spite of the coils being wound with the secondary directly superimposed physically on the primary.

A. The matter of the coupling is a matter of the concentration of the lines of force in the magnetic field of a coil through another coil. Now, the lines of magnetic induction or magnetic force are closed lines; they run around.

That is, if you have a coil with current running in this direction, the lines of force are running actually through, this way, and around, and out through space, and back. Now, even if one is wound over another, there will be a considerable number of these lines of force wihch do not traverse the secondary, at all, but go out through space and come back, at the primary, around in that way; whereas, if we put magnetic material (iron) in the core, more of them are concentrated down in the space. The question of the degree of coupling is a matter which involves quantitatively the geometry and the nature of the materials in the neighborhood, both.

[fol. 2009] 46 Q. Do the impedance of the coils themselves and the impedance of the external circuits connected to the coils affect the coupling?

A. Yes.

47 Q. Will you please explain in what manner the presence of a high impedance in the secondary or closed circuit may affect the coupling between that circuit and the antenna circuit in Fig. 2.

A. In general, the effect on the impedance of a primary circuit, of the presence of the secondary, is to make it apparently have a higher resistance and a lower reactance, and the amount of that increase in resistance or decrease in reactance depends upon what is known as the "coefficient of coupling" between the two circuits. If that coefficient is small, the change in the apparent resistance and reactance of the primary is small. If the coefficient of coupling is large, the changes may be relatively large; and the measure of that change depends directly upon the frequency, directly upon the mutual inductance, and inversely upon the impedance of the secondary.

Commissioner Gordon: I do not believe you have answered the question. As I understood the question, it called for the effect of an impedance on the coupling; and in your answer, as I understand it, you have given the effect of coupling upon impedance. Suppose you read the question to the witness again, and let him answer it.

(47 Q. was read by the reporter.)

[fol. 2010] A. I think the last portion of my answer was relevant to that; that is, the coupling depends directly upon

the mutual inductance, directly upon the frequency, and inversely as the impedance of the secondary; so that if the secondary impedance is high,the coupling is low; and *vice versa.*

By Commissioner Gordon:

48 Q. In the last portion of your answer you used the word "coupling" in the ordinary sense of magnetic coupling of two circuits?

A. I have a little difficulty understanding just what you mean by the "ordinary sense of the word". "Coupling" is used in several senses.

49 Q. I am trying to find out in what sense you are using it. Suppose you define it.

A. As defined by the equation which I stated, in words?

50 Q. Oh, can you not define it more simply than by referring to a mathematical equation? It is a familiar term that is used again and again in radio. I do not believe it requires a mathematical formula to define it. However, if you feel that it does, why, all right.

A. I think it does, to be precise, quantitative about it.

[fol. 2011] 51 Q. Considering the antenna circuit shown in Fig. 2 of the Marconi patent, for tunes 3 and 4, on page 4 of the patent, how does the current in the circuit f'-g' divide bewteen the path through condenser h, and the path through coil j', for tunes 3 and 4?

A. They divide inversely as the impedances of the two branches.

52 Q. With an incoming signal of the resonant wavelength approximately 199 meters which you have determined for tune 3, would a large part or a small part of the antenna current pass by way of the coil j', or the condenser h?

[fol. 2012] A. If you want a quantitative answer, I would have to figure the two impedances.

53 Q. Speak qualitatively, if you can.

A. In general, the impedance of the condenser h at that frequency is less than that of the coil j', and therefore the greater current will flow through h.

* * * * * * *

(Tr. 2122)

55 Q. Now, Doctor Wheeler, referring to your calculations concerning the Marconi circuit of Fig. 2, will you

please explain the behavior of the antenna circuit of Marconi, Fig. 2, in tunes 3 and 4, assuming the values for the elements making up the circuit, which you have calculated.

A. From the calculations which I have made and my study of the circuit of Fig. 21, I am of the opinion that the antenna circuit behaves as a simple series tuned circuit, the primary coil j functioning simply as a coupling element and not as a tuning element; that is, the coil j' is the element for the transfer of the energy from the primary to the secondary, and does not figure appreciably in determining the resonant response of the primary to a received signal.

By Commissioner Gordon:

56 Q. I take it, however, that you do not mean that the antenna current, a portion of it, flows through j'?

A. A portion of it does, yes; a small portion.

[fol. 2013] 57 Q. What proportion?

A. I haven't the figures here. I could figure it out, in a little time.

58 Q. You must have figured it out beforehand, when you say "a small proportion". I am just trying to find out what you mean by that.

A. I was judging by the ratio of the reactances of the coil and the condenser at a given figure.

59 Q. Is it 50 per cent, or 25 per cent, or 30 per cent? What is it?

A. I cannot answer that quantitatively, immediately. I [fol. 2014] can answer it, in about five minutes, if I can take that time.

60 Q. No, I take it that you based your present answer to me on something that you had in mind. Now, what was that? You said, a small proportion. What do you mean by "a small proportion"?

A. I have not figured out what proportion.

61 Q. Then you did not know?

A. I can figure it, in five minutes' time.

62 Q. I am not asking you to figure it. I want to know what you meant when you gave in your answer the phraseology "a small proportion". I do not ask you to figure anything. I just want to know what your mental idea was of "a small proportion". You must have had something in mind, otherwise you could not have made that statement. Now, what was it that you had in mind?

A. I had no exact figures in mind, only I know it must be very much less.

63 Q. Less than what?

A. In the j' than in h, the current in the two.

64 Q. But you cannot tell us what it is, without figuring?

A. Not without figuring; no; not exactly.

Commissioner Gordon: All right, next question.

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[fol. 2015] (Tr. 2124)

66 Q. You stated that the coil j' acts as a means or coupling element for transferring the energy from the primary to the secondary circuit.

A. Yes.

67 Q. What is that energy? Is it potential across, an element of the primary circuit?

A. Energy is not potential, and I do not quite understand what you mean by that question. All of the energy that gets into the secondary gets into it by virtue of the action of the coil j'.

68 Q. Just how does that coil j' couple energy into the secondary—by what sort of coupling?

A. A very complicated sort of coupling, partially a coupling in the magnetic field, partially a coupling of the electrostatic field. I should judge that the electrostatic coupling was predominant over that of the magnetic field.

69 Q. Is there another name frequently applied to this electrostatic coupling?

A. It is sometimes called "capacity coupling".

70 Q. You stated that the antenna circuit of Fig. 2 for tune 3 of the Marconi patent and tune 4 behaves as a simple series tuned circuit, with j' functioning simply as a coupling element and not as a tuning element. Will you explain that in a little more detail.

A. The resonant frequency which I have calculated was [fol. 2016] on the basis of the ordinary coupled circuit theory, the resonance of the primary of the coupled circuit, frequencies to which it will respond in the presence of the secondary. Then, I also found that these frequencies, or rather, one of those frequencies, was substantially the same as for the simple series circuit, consisting of the antenna, coil g', and the condenser h, to ground; and hence, it seems to me conclusive that the resonance of that primary circuit

is accomplished without any substantial contribution from the coil j'. For instance, in the case of tune 3, with 21 turns in the coil g', the resonant frequency of the primary or resonant wave-length of the primary came out 199.2, and, with the same value of g' in the simple series circuit, came out 199.3 meters—substantially the same frequency; and hence I conclude that the coil j' affects the tuning of the primary circuit in only a very secondary degree.

By Commissioner Gordon:

71 Q. Is that true for all wave-lengths?

A. For the wave-lengths which I have computed, which vary from 142 to 364; not for all wave-lengths, no.

By Mr. Houghton:

72 Q. What values determined your making of calculations which led to these wave-length ranges, 142.3 meters to 364.1 meters?

A. The values of what?

73 Q. What values of elements of the circuit derived from the patent.

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[fol. 2017] (Tr. 2126)

A. I do not think I have testified to the actual values, numerical values, of the inductances. For the coil g' with the 21 turns, my computed inductance is 24 microHenrys. The computed value of the coil j' came out to be 154.7 microHenrys. That is, being computed by the formula given in circular 74 of the Bureau of Standards, page 252. The value of the condenser h is given in the tune, 4,600 micromicrofarads. The equivalent antenna inductance was computed to be 25 microHenrys, and the equivalent capacity of the antenna, 240 micromicrofarads.

By Mr. Houghton:

74 Q. Does the patent state any limit in number of turns for coil g'?

A. It says "up to 21 turns".

75 Q. That is for which tune?

A. That is for tune No. 3, receiver.

76 Q. Now, with respect to tune No. 4, does it state?

A. It specifies 100 turns for the coil g'.

77 Q. What is the total possible range, then, of turns for the coil g′, as disclosed by the patent?

A. From zero to 100.

78 Q. Do the wave-length ranges which you have calculated from 142.3 up to 364.1 bear any relation to the possible range of number of turns of the coil g′?

A. The values which resulted in the wave-length 142.3 assumes g′ zero; the values which resulted in 166.4 were for g′ having 10 turns; that resulting in 199.3 were, for g′, 21 turns; and that which resulted in the value 364.1, were for g′ having 100 turns.

[fol. 2018] 79 Q. Then the maxima and minima of the wave-length range to which you have just testified corresponds, does it not, to the maxima and minima number of turns of the coil g′, as disclosed in the patent?

A. It does, with this proviso—that h remains at 4,600 micromicrofarads.

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(Tr. 2128)

81 Q. If the condenser h for tunes 3 and 4 is to be given a value of 4,600 micromicrofarads, what utility is there in having the condenser constructed to be adjustable?

A. In so far as the operation of the equipment as specified in the table on page 4 goes, I can see no particular advantage in having the condenser h adjustable except for the purpose of attaining that particular value, 4,600 micro microfarads at all of the receiving stations. The question is possibly a bit difficult because one has to project himself back to some 30 or 40 years in the art to understand just what was intended in the specification and table.

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(Tr. 2129)

A. (Continuing:) At the present day, one would certainly not put in a variable condenser and then leave it fixed in only one value, but, at the time referred to, and with [fol. 2019] the type of construction of the condenser, making it adjustable for the purpose of getting one particular value might be the better way of obtaining that value than to attempt to make all condensers of a fixed value, questions of manufacturing tolerance, and so on, coming in. Of course, I cannot say that that was the reason, because I do not know; but, as far as the operation, as disclosed in those

tables, goes, there is no reason why that condenser h should not have been a fixed condenser.

By Mr. Houghton:

82 Q. Earlier in your testimony today, you stated that you assumed the coupling between the primary and the secondary to be so small that its square could be neglected. Did you make any computations to check the validity of such an assumption?

A. Yes.

83 Q. Will you briefly summarize what sort of calculation this was, and how it indicates the validity of the assumption.

A. I computed the inductance of the secondary, approximately—quite approximately—as disclosed in Fig. 8 of the patent, as being the form most amenable to calculation. This calculation, while it admittedly is not so accurate as those connected with the determination of the tuning, showed that the inductance was of the order of 1300 to 1400 microHenrys. From the number of turns in the construction given for the secondary used in tune 3, namely, that of Fig. 6, I judged that the inductance would be considerably greater than that shown in Fig. 8. Now, for the coil, as I figured it from Fig. 8, the inductive reactance of [fol. 2020] that coil would vary over the range of frequencies or wave-lengths 142 to 364 from 18,000 to 7,000 ohms, which is a tolerably high reactance. Consequently, this coupling coefficient for the alteration of the primary circuit resistance and reactance is bound to be small, because that coupling coefficient varies inversely as the impedance of the secondary. It having a high reactance in the coil j^2 and a relatively high resistance in the detector T, the two together will insure a very small coupling coefficient—coupling coefficient, Your Honor, for the transfer, not of energy but of apparent reactance and resistance from the secondary to the primary.

Commissioner Gordon: I do not understand what you mean by that last statement. I always thought that the coupling was for the transfer of energy.

The Witness: Your Honor, I probably owe you an apology for the form in which I have presented this. As you say, the ordinary meaning of the coupling coefficient is simply the ratio of the reactance which is common to the two circuits to the product of the reactances of the same nature in the primary and the secondary separately, the

square root of those. That is the ordinary one. Now then, any transformer behaves as if its resistance and reactance were different from the values that they would have if they were separated entirely. That is, if you measure the resistance and reactance of the primary of the transformer with the secondary taken over to China or anywhere else out of the way, you get certain values. You bring them together [fol. 2021] and measure again by identical methods the resistance and reactance of the primary, and you find them different: you will find the resistance increased and the reactance decreased by a certain fraction of the resistance and reactance of the secondary circuit. Now, I probably should not, in this company at least, have called this a "coupling coefficient", but what I meant by the coupling coefficient, in that sense, is the factor that determines what fraction of the secondary resistance and reactance is effectively transferred to the primary so as to alter its value, to what is found when they are together. It is a thing you find in all text books, and so on; and that factor, that transfer factor, varies directly as the "ordinary coefficient of coupling" if you want, and the frequency, and inversely as the impedance of the secondary.

By Commissioner Gordon:

84 Q. Does not your statement, after all, mean that the impedance of the primary and the secondary windings varies in accordance with the tightness or looseness of the coupling?

A. The amount of the change will be much less dependent on the tightness or looseness of the coupling than it is dependent upon the impedance of the secondary. The amount of the alteration of the constants of the primary coil, that is, the resistance and reactance, is much less dependent upon the coefficient of the coupling than it is upon the value of the impedance to the secondary when that impedance is large.

By Mr. Houghton:

85 Q. By the "coefficient of coupling" in that last sent- [fol. 2022] ence, did you mean the same thing that you meant when you used——

A. (Interposing:) Let us call them "coefficient of coupling, No. 1," and "coefficient of coupling, No. 2." No. 1

is the ordinary definition of coefficient of coupling. No. 2 is a coupling of the two circuits with respect to the alteration in the values of resistance and reactance which results from the juxtaposition of the two circuits.

By Commissioner Gordon:

86 Q. Or which result from the tightness or looseness of the coupling?

A. Well, no, not exactly, because this coefficient of coupling No. 2 is, as I have stated, less dependent upon coefficient of coupling, No. 1, than it is upon the magnitude of the secondary impedance.

87 Q. I am perfectly frank to say that I do not follow the use of two different terms or definitions of coefficient of coupling.

A. It may be inadvisable to have two terms, and call this second "baby" by another name than "coefficient of coupling." I think it would have been advisable. I regret somewhat that I introduced it. It is one of those things that is very simple to explain as an equation in mathematics and very difficult to put into words.

88 Q. Let us see if I understand this. Let us come down to what you referred to as a conventional coefficient of coupling. Just in the last few answers, you used the term "jux-
[fol. 2023] taposition of the primary and the secondary". When the primary and secondary are as closely related to each other as possible, isn't that known as a "tight" coupling?

A. You mean physically?

89 Q. Yes.

A. No, it may be, or it may not be.

90 Q. Then I wish you would define for me again what is meant by the conventional term as it is used, "loose coupling", and "tight coupling"; in other words, what those conventional terms mean as they are ordinarily used, and in simple language. The ordinary radio operator uses those terms, does he not?

A. Yes.

91 Q. And he does not possess any great wealth of mathematical knowledge, does he?

A. He may not, and usually does not.

92 Q. Then what do those terms mean to the ordinary radio man, not the highly skilled mathematician?

Mr. Houghton: In that connection, I believe it might be helpful, Doctor Wheeler, if you would enumerate the ways in which a coupling may be made looser or tighter, and then from that, explain the answer, requested by the Commissioner.

Commissioner Gordon: I suggest that he first give us definitions of these two phrases, "tight coupling" and "loose coupling", in the ordinary usage of the radio art, and then he can elucidate as to the various ways in which "tight coupling" and "loose coupling" are obtained.

A. Of course, tight coupling and loose coupling are two [fol. 2024] extreme cases of the coupling, and there are various types of coupling. The tight coupling will be one where the number that expresses it is of the order of five tenths or greater.

By Commissioner Gordon:

93 Q. Five tenths of what?

A. A pure number. Coefficient of coupling is a pure number.

94 Q. I did not ask you to define "coefficient of coupling".

A. That is, we will say, the mathematician's way of looking at it.

95 Q. I did not ask you for the mathematician's way of looking at it. I asked you for the ordinary, conventional use of that term, as used by the radio man and not by the mathematician.

A. Well, he means, probably, that he brings the primary and the secondary into close juxtaposition for tight coupling.

96 Q. That is "tight coupling" in the ordinary sense?

A. In his sense of the word; yes.

97 Q. What is "loose coupling"?

A. When he separates the coils, primary and secondary, to considerable distance.

By Mr. Houghton:

98 Q. What is the functional difference between such tight coupling and a loose coupling?

A. There is more than one difference. It affects the [fol. 2025] transfer of energy from one circuit to the other, and it also affects the apparent electrical constants, the resistance and reactance, of the circuits. In the case of the

loose coupling, there is less energy transferred and less effect of the primary upon the secondary, or vice versa. In the case of tight coupling, there is more energy transferred, and the apparent resistance and reactance of the two circuits are altered more.

By Commissioner Gordon:

99 Q. Then as I understand it, that energy is transferred electromagnetically and electrostatically?

A. Yes.

100 Q. And is it proper to say that that transference is a maximum when a tight coupling exists, or, not a maximum, but it approaches maximum?

A. There is a certain critical value of the coupling, known as "sufficient coupling", or, rather, there is a critical value above which the coupling is called "sufficient" and below which it is called "deficient" coupling, where the behavior of the two circuits is quite different with respect to energy transfer. As you increase the coupling beyond this critical dividing point into the region where the coupling is called "sufficient", you will increase the energy transfer according to one law, and where you are below that, the law of transfer is different.

101 Q. But, after all, if I have a primary coil in my right hand a secondary coil in my left hand, and my arms are [fol. 2026] extended as far as they can be apart, it would be proper to say that that would represent a very loose coupling with a minimum transfer of energy, would it not?

A. Yes.

102 Q. And then, as I brought my two hands together and as they approached each other, would it be proper to say that I was increasing the coupling and also increasing the transfer of energy?

A. In a general sense; yes.

103 Q. And, finally, when my two hands met, providing the coils were of the same diameter, and I could not get them any closer together than the meeting of my hands, in that case I would have the maximum coupling and the maximum transfer of energy, would I not?

A. The closest coupling that you could obtain without the use of magnetic material in the cores of the coils.

104 Q. That would be about the closest I could get, under those circumstances?

A. Yes.

105 Q. Now, if the coil in my left hand were smaller than the one in my right, so I could slip it inside, would that give me a still closer or a tighter coupling?

A. Yes.

106 Q. And that, then, would represent, under those conditions, with two concentric coils, the tightest coupling that I could get without the use of magnetic material?

[fol. 2027] A. Yes, and without actual interleaving of the two on the same coil form.

By Mr. Houghton:

107 Q. Doctor Wheeler, you have indicated by your last answers that one manner of obtaining tightness or looseness of coupling is by the physical space of separation of a primary and secondary coil. Are there other ways in which tightness or looseness of coupling of two coils may be controlled?

A. Do you mean between two coils already existent or in the design of coils to produce the greatest possible degree?

108 Q. I mean between two coils already existent, and ph. sically close together, bearing in mind that you have said looseness or tightness of coupling had reference to the transfer of energy from one coil to the other.

A. You could effect it by putting magnetic material inside the core. You could change the coupling that way, and increase it.

109 Q. That would increase the coupling? Suppose a high impedance is inserted into the circuit of one of the coils; a high impedance of the same nature as the impedance in the coil itself; does that affect the energy transfer between the two coils, which may otherwise be termed the closeness of coupling?

Commissioner Gordon: It rather seems to me that defendant's counsel has not only asked a question but has made a statement. The question, because of that statement, is dual.

[fol. 2028] Mr. Houghton: I will withdraw the "which may otherwise be termed", and so forth, for the present. Will you read the question as revised.

Commissioner Gordon: Rephrase the question, so that the witness has the question in its rephrased form.

(109 Q., minus the final clause, was read by the reporter.)

Commissioner Gordon: I suppose the question contemplates both electromagnetic and electrostatic transfer of energy, does it not?

Mr. Houghton: Impedance of the same nature as the impedance of the coil would be inductive impedance.

A. Yes; it would affect the transfer.

By Mr. Houghton:

110 Q. Is it correct to say that the transfer of energy between the two coils is an indication of the looseness or tightness of coupling between them?

A. In a very general sense, yes.

111 Q. It appears to me that the Commissioner is troubled by the fact that these coils, j′ and J², are physically very closely juxtaposed; and yet you indicate that for the coupling between the primary and secondary of Fig. 2, the coupling coefficient is small, and the coupling is therefore what might be termed "loose". Can you explain how the loose coupling exists, in Fig. 2, notwithstanding the close physical justaposition of coils j′ and j²?

[fol. 2029] A. It would be very easy to do it mathematically. The reason is that the coefficient of coupling for all quantitative purposes is defined as the ratio of the reactance which is common to the primary and secondary to the square root of the product of the reactances of the same kind in the primary and the secondary; therefore, if the reactance of the primary or secondary, one or both, is high, that ratio is low, independently of the physical juxtaposition or physical positions of the primary and secondary; because it is a matter of definition of what we mean by the coefficient of the coupling. That is the definition, as I quoted it, as it is given in the books.

By Commissioner Gordon:

112 Q. But you still maintain, do you not, that when two coils, the primary coil and the secondary coil, are juxtaposed, the electromagnetic transfer of energy and the electrostatic transfer of energy are maximum, which can only be exceeded when the coils themselves are interwoven or interlaced or something, that is correct?

A. That is about correct, sir; yes.

By Mr. Houghton:

113 Q. Is it correct to say that introduction of high impedance into the secondary circuit of Fig. 2 produces the same electrical effect between coils j′ and j² that would be produced by the physical separation of coils j′ and j²?

A. Yes, sir; that is so.

[fol. 2030] 114 Q. In other words, either physical separation or the introduction of high impedance values into the secondary are alternative ways of loosening the coupling, is that correct?

A. That is correct.

By Commissioner Gordon:

115 Q. Both electrostatically and electromagnetically, is that true?

A. The electrostatic—remember, in the definition, that is taken care of by specifying the kinds of reactance. You have got to have the reactance which is common, too, to the two circuits of the same kind as the reactance in each circuit, when you calculate your coefficient of coupling.

Commissioner Gordon: I think that can be almost answered Yes or No.

A. It is a little more complicated than that, Your Honor, I think, because the definition of the coefficient of coupling changes with the character of the transfer, whether it is electromagnetic or electrostatic.

By Commissioner Gordon:

116 Q. Tell me, then, Doctor Wheeler, when we have two coils in juxtaposition, and we do not move them apart, or we do not separate them, but we merely change the reactance or resistance, or something, in one of those lines, would there be the same exact number of lines of magnetic force transferred from the primary to the secondary, as before?

[fol. 2031] A. No, because the current is changed.

117 Q. I am talking about the current in the primary.

A. No. I say, the current is changed by that fact, so that there is not the same number of lines of force in the two cases.

118 Q. There is not the same number of interlinking lines of force?

A. No.

119 Q. Now, how about the electrostatic effect which is set up in the primary and affects the secondary? Is that the same, where we change the resistance or reactance of the secondary?

A. If the reactance that we change is inductive reactance, the number of lines of force will remain the same, but if it is capacity reactance in the secondary you could not say; we change, and it will make a difference.

120 Q. And by this last answer, you mean the electrostatic, is that right?

A. Of course, the use of the word "electrostatic" is somewhat a misnomer any way, because in this case we are dealing with changing charges, moving charges, all the time, and there is nothing static about it.

121 Q. It is a term that you have used?

A. Yes, it is used. I admit that scientists are not impeccable in their choice of language, always, it is true. The [fol. 2032] actual coupling in this case is a complicated thing, terribly complicated, and so far as I have been using it for these calculations it is not for a question of determining the energy transfer, at all, but only for determining the change produced in the electrical characteristics of the primary.

122 Q. Back in 1900, when Marconi applied for this patent, was he not more interested in the energy exchange than anything else?

A. I presume so. He was interested in that, and in tuning. According to this patent, one of the main features is to be able to tune the circuits so that only designated receiving stations can get the signal. And it is with respect to the tuning only that my calculations have been directed.

123 Q. Doctor Wheeler, before the noon recess, you stated that you could calculate the distribution or approximate distribution of currents in the antenna circuit of tune 3, as between the coil j′ and the condenser h. Have you now made such a calculation?

A. I have.

124 Q. And what does the calculation show with respect to the current distribution?

A. Assuming that the currents are inversely proportional to the reactances, the current in the coil j′ will be between one and two percent of that in the condenser h. This calculation is not exact, in that the true ratio of the

currents is the inverse ratio of the impedances. However, there is no data immediately available for determining the impedances, which means that I have neglected the resist- [fol. 2033] ances in each branch. Inclusion of the resistance of the coil j′ would tend to decrease the ratio of the current in j′ to that in h; and hence this ratio of one to two percent is in the nature of an upper limit to the ratio of the currents.

By Commissioner Gordon:

125 Q. Is that figure which you have given a figure which is just for the one wave-length or one set of wave-lengths, or does it apply generally?

A. No, only for the wave-length, 200 meters. It will change. I have not had time to figure it out for the whole range. I will change, but not enormously. It won't change enormously. I should guess it might go up to 10 percent at one extreme, as an outside. That is a guess.

By Mr. Houghton:

126 Q. When you were asked your opinion as to the relative value of the current distribution between the coil j′ [fol. 2034] and the condenser h, you stated that a much smaller amount of current would pass through the coil than through the condenser for tunes 3 and 4. Now, can you state without reference to the calculation which you have just made what theoretical basis you had for your previous statement with respect to the approximate distribution?

A. My knowledge that at these frequencies the capacity reactance of the condenser, as large as 4,600 micro-micro-farads, would be very much less than the inductive reactance of a coil of inductance of approximately 150 micro-henries. Without having calculated it out, I know it must be so, simply from experience, if you want to, with condensers and coils.

127 Q. Can you explain a little more in detail why you know it must be so?

A. I know that the capacity reactance is inversely proportional to the product of the capacity and the angular frequency, whereas the inductive reactance is proportional directly to the product of the inductance and the angular frequency, and with the magnitudes of the capacity and the inductance involved, I know that capacity reactance must

be much less than that of the inductive reactance. You may take it that the feel for the magnitudes of those things gets to be more or less instinctive after one has figured such things a good many times.

128 Q. Bearing in mind the current distribution in the primary circuit between the coil j′ and h, will the coupling [fol. 2035] of the secondary circuit of Fig. 2 to the coil j′ affect materially the current distribution to which you testified?

A. It would affect it somewhat, but not materially.

129 Q. You have testified that the secondary, j^2, of the transformer of Fig. 6 would have a very high inductive impedance. With that secondary coupled to the primary j′, does that impedance enable you to judge about what effect if any the coupling of that secondary would have on the distribution of current bewteen coil j′ and the condenser h?

A. Again, it is a matter of our old friend, the "coefficient of coupling", and that means in general that since, owing to the construction of the coil of Fig. 6, the coupling would probably be somewhat looser than in the case of that of Fig. 8, the effect of the reactance of the coil of Fig. 6 in changing the current distribution between the j′ and h would be less than in the case of the Fig. 8 transformer.

By Mr. Blackmar:

130 X Q. May I interrupt to ask which coefficient of coupling you were talking about, then?

A. As a matter of fact, it would be both that would be loosened, I believe.

[fol. 2036] By Mr. Houghton:

131 Q. Doctor Wheeler, do you have a tabulation of the resonance frequencies of the Marconi tunes as calculated by you in accordance with your previous testimony?

A. I have.

132 Q. Is this the tabulation?

A. That is.

Mr. Houghton: I ask that the tabulation be marked defendant's exhibit BBB, and offer it in evidence.

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(Tr. 2147)

133 Q. You have just indicated a correction in a legend on the copy of this tabulation. What was that? Will you

indicate it on the record, and also mark it on the tabulation.

A. Change the subheading of the column, numbered 4, by striking out the words "if" and "j' omitted", and substituting "of series circuit f'g'hE alone"; so that the caption reads: "W/L (wave-length) of series circuit f'g'hE alone."

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(Tr. 2149)

134 Q. Referring to the tabulation, defendant's exhibit BBB for identification, please refer to the first and third [fol. 2037] columns, the third column being headed "Overall W/L", meaning "overall wave-length". Will you please state if the values in the third column are those determined by your calculations for the respective tunes of the Marconi table on page 4 of the patent, as previously testified.

A. They are.

135 Q. And with respect to the fourth column, headed "Wave-length of series circuit f'g'hE, alone", are the values set forth opposite the three cases of tune 3 and tune 4 the values calculated for that series circuit by you, as previously testified to?

A. They are.

136 Q. And with respect to tunes 3 and 4, what do the figures set forth in the fifth column of the tabulation represent?

A. The wave-lengths of the second response point of the coupled circuit, consisting of the primary and secondary, as shown in Fig. 2.

137 Q. Are those the figures calculated by you for the three cases of tune 3 and the case of tune 4?

A. They are.

138 Q. And what do the figures set opposite them in the last column of the tabulation represent?

A. The resonant frequency of the circuit hj' by itself.

139 Q. Is there any statement in the patent with respect [fol. 2038] to the relation of the transmitter frequencies for the respective tunes, to the receiver resonance frequencies for those tunes?

A. The statement on page 4, immediately beneath the two tabulations, the one you refer to.

* * * * * * *

141 X Q. At what lines of that page?

A. At line 6 of page 4; and also in specification on page 2, beginning, line 118.

142 X Q. How about page 4, line 1?

A. For completeness, it might be well to make it "page 4, line 1".

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By Mr. Houghton:

143 Q. Referring to the tabulation, defendant's exhibit BBB for identification, will you explain what the several columns of figures on the tabulation mean, with respect to the tuning of the Marconi receiver antenna circuit?

A. You wish me to take the columns in order?

144 Q. Either taking the columns in order, or comparing the columns, with more particular reference to tunes 3 and 4.

A. In column 3 are given the computations of the resonant frequency of the primary circuit of Fig. 2 of the Marconi patent, from the data available in the patent specification as given in the tabulations on page 4, and the data as to coils, on page 3 of the patent. Column 4 gives the resonant [fol. 2039] wave-length of a simple series circuit consisting of the antenna, the loading coil, condenser, ground connection, with the values of the loading coil and the condenser as given for the corresponding cases of the Marconi primary circuit, Fig. 2

145 Q. The same antenna, also?

A. With the same antenna.

146 Q. What does the comparison of those columns show?

A. A comparison of those columns seems to me to afford the definite conclusion that the primary circuit of the Fig. 2 of the Marconi patent is essentially the same as that of a simple series inductance capacity circuit. Do you wish me to go on with the columns?

147 Q. Please.

A. In column 5 are given the values of the second resonant frequency of the primary circuit of the Marconi Fig. 2, which occurs in tunes 3 and 4, only, as only in those two cases will the transformer possess two frequencies. In the sixth column is given for comparison the wave-length of the circuit, consisting of the condenser h and the coil j', by itself, and the comparison of those last two columns

shows that the second coupling frequencies are substantially those of the circuit hj' by itself.

148 Q What does the fifth column show with respect to the effect on this second frequency of material changes in the number of turns of load coil g' included in the circuit?

[fol. 2040] A. It shows that the change, the effect, is very small.

149 Q. Doctor Wheeler, do you have all of the calculations, made to determine the results tabulated on defendant's exhibit BBB for identification, here and available for inspection by opposing counsel?

A. I have, with the exception of the figures in column 2.

150 Q. Are the figures in column 2 dictated by the statements referred to by you in the Marconi patent specification with respect to the relation between transmitter and receiver wave-lengths?

A. Yes, they are.

Mr. Houghton: I offer in evidence, as defendant's exhibit BBB, the tabulation just referred to by the witness, and entitled "Marconi Antenna Circuit Tunes".

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By Mr. Blackmar:

151 X Q. I would like to understand your answer with respect to column 2, which you said was dictated by something in the specification.

A. That all the circuits should have approximately the same tune, the four circuits.

152 X Q. In other words, the figures in column 2 were taken from column 3 rather than from anything directly in the patent, is that what you mean?

[fol. 2041] A. Except as given in the patent by the two quotations that I cited from it.

153 X Q. Well, those quotations are the ones which indicate that tune 1, for example, of the receiver, is to be of the same wave-length as the tune 1 of the transmitter?

A. Yes.

154 X Q. In other words, then, column 2 is arrived at by taking approximately the figures which appear in column 3?

A. Yes.

155 X Q. Rather than by independent calculation?

A. Yes, sir; that is correct.

156 X Q. Now, may I ask what the statements opposite the numbers 5 and 6 in column 1 mean.

A. In 5 and 6, the significant difference is in the form of the antenna, which has the zinc cylinder at the top, the different distance above the ground—not a difference, but a height—three meters, approximately, above the ground, in there. In that circuit it is. Otherwise, No. 5 is identical with No. 1, except for that antenna difference, and No. 6 is also identical with No. 1, and, as No. 1 is a simple series circuit, so also are tunes 5 and 6, with the exception that in 5 and 6 the condenser h is not included. The notations there are very compressed; there really should be a comma after the series.

[fol. 2042] Mr. Blackmar: I have no objection to your putting that in if you wish to, in the original.

The Witness: That is, it is a series tuning arrangement in those cases, as in tune 1. It differs from tune 1 in having H omitted.

By Mr. Blackmar:

157 X Q. Is h present in tune 1?

A. No. Oh, no, that's right. How did that get that way? The comma should be after the "only". If there is no objection, I would like to alter the thing, to put a comma after "only" in both those.

Mr. Blackmar: I have no objection, if you wish to do so in the original exhibit, if you think it will clarify it.

(The witness makes the correction or change referred to.)

* * * * * * *

(Tr. 2159)

(The tabulation referred to, marked Defendant's Exhibit BBB, and entitled "Marconi Antenna Circuit Tunes", is filed in connection with this case.)

* * * * * * *

(Tr. 2164)

164 Q. In your testimony yesterday with reference to tune 3 of the Marconi patent you indicated the general distribution of current of the antenna circuit between the coil j′ and the condenser h. With that current distribution, is the cur-

rent in the coil j' or the condenser h in such a condition that it could be said the current in each of those elements is considerably greater than the current flowing in the antenna?
[fol. 2043] A. If the received signal is in tune with the natural resonance frequency of the coil j' together with the condenser h, then the circulating current in that circuit may be much greater than that outside.

165 Q. Referring to your tabulation, defendant's exhibit BBB, with what is the received wave-length in resonance, in Fig. 2?

A. With the overall antenna resonance frequency, which is very different from that of the natural resonance frequency of the coil j' with the condenser h.

166 Q. In using the arrangement of Fig. 2 of the Marconi patent in accordance with tunes 3 and 4 of the table appearing on page 4 of the patent, is the antenna current of the received signal substantially equal to the current in the condenser h, or is the current in the condenser h much greater than the antenna current?

A. The antenna current will be very nearly equal to that in the condenser h.

167 Q. Referring again to the table, defendant's exhibit BBB, and to the disclosure of the Marconi patent, would you say that the transmitted and received wave-lengths of the Marconi disclosure are long wave-lengths or relatively short wave-lengths, speaking as of the period prior to 1919?

A. For the tunes calculated, they are relatively short wave-lengths. On the present international system of classifying, I think they would be called the "medium long", though my recollection is not exact on the frequency limits, under the present convention.
[fol. 2044] 168. Q. Do you find any disclosure in the Marconi patent comprising any teaching of the desirability of using a parallel resonant circuit for receiving wave-lengths longer than 3,000 meters?

A. I have not calculated all the possible tunes disclosed with the apparatus in the specifications; however, as far as I have gone, the answer to your question is No.

169 Q. Do you have the calculations here with respect to the results obtained contrary to those obtained by Mr. Picard with respect to the tuning range of the condenser of the SE-143 primary circuit?

A. I have.

170 Q. And they are available for inspection by opposing counsel?

A. They are.

Mr. Houghton: That is all. Direct examination closed.

Cross-examination.

By Mr. Blackmar:

171 X Q. You referred to not having calculated all the possible tunes of the Marconi patent. How many possible tunes do you consider that there are?

A. I haven't figured that out.

* * * * * * *

173 X Q. Isn't it true that in your opinion, within the disclosure of that patent, there might be an infinite number of tunes?

[fol. 2045] A. It depends upon the method of selecting turns in the load coil.

174 X Q. Do you understand that there is anything in the patent limiting it for example to the six so-called tunes of page 4?

A. No, not as the specification reads. In tune 3, on the "receiving-station", it is up to 21 turns, can be used. That would mean that you might have from zero to 21 turns, and if he can select one turn for a tune, that would make 21 tunes possible for the tune numbered "No. 3".

175 X Q. I think, Doctor Wheeler, my question was a little broader than that. In your opinion is there anything in the Marconi patent limiting it to the six tunes of page 4, including in those tunes any of the variations of tune 3 of the receiver?

A. The thing that will limit the ranges that you can get, of course, is the values of the coils and the condensers, and the particular way in which they are connected.

176 X Q. In your opinion, is the disclosure of the patent limited to the coils and condensers that are specifically described in the patent?

A. Of course, the statement is general that these things that are described in the specification are preferred forms.

[fol. 2046] 177 X Q. And that is also true, is it not, with respect to the tunes given in the tables on page 4?

A. I should judge so, in general.

178 X Q. They are referred to as "preferred adjustments", are they not?

A. I should think so, as a layman. I do not know. There may be some legal question of interpretation of the exact meaning of those phrases that I am not familiar with.

179 X Q. May I say this, Doctor Wheeler, that a patent is not addressed to a patent attorney, but to the man skilled in the art. In that sense I do not think you are a layman. Therefore, in construing the patent, it should be looked at by you as one skilled in the art; and from that point of view, would you in reading the disclosure of the patent consider that the only things contemplated by Marconi were those described by him?

* * * * * * *

[fol. 2047] (Tr. 2168)

A. The answer is No, that portions of the apparatus such as coils and condensers having equivalent electrical properties could be substituted for the actual elements as he specifies them.

By Mr. Blackmar:

180 X Q. What do you mean by "equivalent electrical properties"?

A. Similar inductances, self and mutual; similar capacities, and similar arrangement of those elements in the circuit.

181 X Q. Similar in value, you mean?

A. Similar arrangement of the elements; similar in value, yes.

182 X Q. Then as I understand your testimony, if a transmitter and receiver were utilized, even though they embodied the circuits shown in the patent, and the wave-length employed was say 3,000 meters, and that wave-length is not described in any of the tunes, then you would not be utilizing the disclosure of the patent; is that correct?

A. Strictly, I think that is what I would say now. I would not like to commit myself absolutely definitely on that point.

183 X Q. In 1904 were you familiar with radio phenomena?

A. Radio phenomena? Yes.

[fol. 2048] 184 X Q. If the Marconi patent had been brought to your attention in 1904, which is the year in which

it issued, and if you had read and studied the patent, am I to understand that the only thing that you would have been taught by it would be the use of the circuits shown in figures 1 and 2 for the wave-lengths covered by the various tunes on page 4?

A. That is all that I would definitely have been taught. I might have attempted, if I were curious at that time, to extend the same methods to other wave-length ranges, either longer or shorter.

185 X Q. And I assume then that you would have considered that a contribution by you to the radio art, is that right?

A. No.

186 X Q. Would you consider it based on Marconi's disclosure?

A. Yes.

187 X Q. And I also call your attention to paragraph on page 3, starting at line 12. Would that have suggested to you in 1904, if the patent had been shown to you, that variations or modifications could have been made in the arrangement shown in figures 1 and 2? And in the same connection, you might read the paragraph commencing on line 19 of page 4.

A. Yes.

188 X Q. Now, referring to your calculations which are embodied in defendant's exhibit BBB and in your testi- [fol. 2049] mony, will you please point out in the patent where you find the description of the receiving antenna, the constants of which you calculated.

A. I find on page 3, line 19, that the aerial conductors at all stations were of a given diameter, 7-strand copper wire. The transmitting conductor, the aerial conductor, is given for the transmitting station, the length of it is 36.576.

189 X Q. That is for the transmitting station?

A. For the transmitting station.

By Commissioner Gordon:

190 - Q. Where in the patent is this given?

A. On page 4, the second column of the "transmitting-station" table—36.576 meters of cable; and that is repeated all the way through.

By Mr. Blackmar:

191 X Q. With the exception of tunes 5 and 6?

A. Yes, which had a different kind, what we would call now "top loading" of particular kind. At the present moment I presume I may be accused of assuming that the antennas were the same at receiving and transmitting stations. I do not see any definite statement as to the receiving antenna.

192 X Q. In approaching your calculations and considering the patent did you consider the coil j′ as fixed or as variable?

A. As a fixed coil which had different values in different tunes.

[fol. 2050] 193 X Q. But as far as each individual tune was concerned you considered it as a fixed coil?

A. I did.

194 X Q. Incidentally, did you have available in 1904 formulas or information from which you could have made the calculations that resulted in the figures of defendant's exhibit BBB?

A. I do not know whether the formulae which I used this week in making the calculations existed in 1904, but formulae of some kind did exist at that time, and much previously.

195 X Q. That is, for computing the inductance of a multiple-layer coil, and things of that kind?

A. Yes, sir.

196 X Q. Were they more or less accurate than the formulas which you employed?

A. I used no formulae for multiple-layer coil.

197 X Q. In general were the formulas for calculating inductance that were available in 1904 more accurate or less accurate than those employed by you?

A. I do not know, without looking them up.

198 X Q. Then you are not sure whether you used the most accurate formulas now available in making your calculations, is that right?

A. No, I used formulas now available which I consider as good as any.

[fol. 2051] 199 X Q. I take it from a recent answer that you have not made calculations as to the wave-length of the secondary circuit of Fig. 2, is that correct?

A. Wave-length of the secondary circuit? No, I have not.

200 X Q. In Fig. 2 is the condenser h in shunt or parallel relation with the coil j'?

A. It is.

201 X Q. Do you consider the secondary circuit of Fig. 2 a series circuit or a parallel circuit?

A. As shown, the condenser h' is in parallel with coil j^2, or as pictured, in parallel with the coils j^2 and portions of the two g^2's.

202 X Q. In the first place, I think that the tables, page 4, will show that the condenser h' is not embodied in the arrangements of tunes 3 and 4; is that not correct?

A. That is correct.

203 X Q. My question was, as to whether you considered the circuit a parallel or a series circuit. I do not think that question has been answered. Will you please answer it.

A. With the condenser h' out of the circuit, the secondary circuit is a series circuit, including coil j^2, condenser j^3, portions of the coils g^2, and the detector.

204 X Q. And what type of detector is that?

A. According to the specification it may be a number of different types. From the drawing, I would say that a coherer is indicated.

[fol. 2052] 205 X Q. When you considered the impedance of the secondary circuit, you considered that a coherer was present, did you not?

A. I did.

206 X Q. What is the resistance of a coherer, roughly?

A. I do not know what it is, in magnitude. Before the particles cohere, it is some thousands of ohms. After it coheres, during that time, depending upon how many of the contacts do cohere, the resistance falls quite appreciably.

207 X Q. When you referred to high impedance in the secondary circuit, were you considering the coherer as decohered, or as cohered?

A. As cohered.

208 X Q. And can you give us any idea as to the resistance of the coherer in that condition, other than to say that it is less, or considerably less, than in the other condition?

A. I am trying to recall some experiments I made on that in 1897 or '8, and my recollection is not any too good as to exact values. I do recollect that the cohered resistance might vary over a very considerable range, from some hundreds to some thousands of ohms, the decohered resist-

ance being say of the order of—well, I cannot remember exactly, but say, the order of from 10,000 up to infinity.

209 X Q. I thought you gave us the figure of 3,000, a few minutes ago.

A. No, "a few thousand", I meant to say, some thousands—I do not know.

[fol. 2053] 210 X Q. If you followed the instructions of the Marconi patent and set up a transmitter and receiver in accordance with tune 4, for example, would you expect to have had the apparatus precisely in tune, if you had been building it say in 1904 and 1905?

A. You mean the transmitter and the receiver exactly in tune?

211 X Q. And each of the two circuits, in each of them.

A. I would have considered myself very lucky if they were.

212 X Q. Would there have been present in the apparatus means for bringing them into tune with each other?

A. Yes.

213 X Q. Now, taking the receiver, the condenser h could have been used for that purpose, could it not?

A. Yes; to a certain extent, yes.

214 X Q. h is a variable condenser, is it not?

A. It is an "adjustable" condenser, as he—well, from the point of view of 1904, yes.

215 X Q. Incidentally, in giving the constants which you had calculated, you did not mention the values of inductance for the coil g′ when it had 10 turns, or 45 turns, or 100 turns. Can you give us that information?

A. I can. This coil g′, I find for 10 turns the inductance to be 9.18 microHenrys; for 45 turns, 57.4 microHenrys; for 100 turns, 139 microHenrys. It is understood, of course [fol. 2054] that those values are not exact, to an indefinite number of decimal places. In general, I think they are correct to the last figure given from the formula. They are correct as far as the formula is concerned, to the last figure.

216 X Q. I am not sure what you mean by "as far as the formula is concerned."

A. That is, the formula, if you put the values of the radius, number of turns, and length of the coil, into the formula, the formula gives you a result which is good to that last place. The determination of the radius depends upon the thickness of any insulation as well as the diameter of the wire. The diameters of the wire are given appar-

ently from the values quoted in the specification, to the old British wire gauge, which is not our standard gauge, and therefore there is some uncertainty as to how thick their silk insulation, which he specifies, was, compared with modern silk insulation. I have taken the values from tables for the thickness of insulation available to me at the present time, so there are certain uncertainties in the data which went into the formula.

217 X Q. I notice the patentee specifies an interval of 2.28 millimeters between adjacent turns, on page 3, line 53. Did you take that as between insulation, or as between the wires?

A. Between the centers of the turns, the centers of the wire. Oh, wait a minute; I will have to correct that, I think. In the case referred to in line 53, page 3, there was no insulation.

[fol. 2055] 218 X Q. I thought you referred to silk insulation.

A. That would be for the coil j', but as to the inductance coils g and g', the diameter of the wire is given, the cylinder upon which they are wound, the coil form diameter is given, and in that case the 2.28 was taken as the distance between the outside of the wire's adjacent turns. There was no question of silk insulation in that coil g'.

219 X Q. I think you said a few minutes ago that if you built up the receiver of tune 4, for example, a final adjustment could be made with the condenser h to bring that into tune with the transmitter.

A. Yes.

220 X Q. Now, according to my notes you said yesterday that in so far as the operation of the receiver as specified in the table of tunes goes, you could see no particular advantage in adjusting h, except to attain the particular value specified in the table.

A. Yes.

221 X Q. Have you not now pointed out an advantage beyond that?

A. I have pointed out the same advantage.

222 X Q. In other words, to arrive at tune 4, you construe that as requiring exactly 4,600 micromicrofarads of capacity in the condenser h, is that what you mean?

A. If the antenna is as I have assumed in my calculations.

[fol. 2056] 223 X Q. You then do not take the figures

given in the tables, as guides, but as specific requirements, is that it?

A. As approximate specific requirements, yes.

224 X Q. Suppose you built the arrangement of tune 4; first, as I understand you, the coil j′ in that case and the coil g′ would each have a definite number of turns, is that right?

A. Yes.

225 X Q. And I think you said you would consider yourself very fortunate if you hit the right wave-length by constructing the circuit following the specific instructions of the patent, that is true, is it not?

A. Hit the exact wave-length? I would expect to hit somewhere near it.

226 X Q. What variable element or elements are there in the antenna circuit of the receiver of tune 4?

A. The coil g′, the condenser h, the two coils g^2 in the secondary.

227 X Q. I was speaking of the antenna circuit.

A. Of the antenna circuit, alone? We have coil g′ and the condenser h.

228 X Q. If the coil g′ were kept at 100 turns, then the only variable element would be the condenser h, is that right?

A. Correct.

[fol. 2057] 229 X Q. If you wished to have the circuit respond to the exact wave-length, would you expect to have to use exactly 0.0046 microfarads for the condenser h?

A. If the received wave-length were exactly 364.6 meters, you would have to be very close to .0046.

230 X Q. If you were also constructing a transmitter at the same time for tune 4, would you expect to have the transmitter and the antenna circuit of the receiver exactly in tune with each other, to start with?

A. No, not exactly in tune.

231 X Q. To tune them, you would have to make some adjustments in one or the other, would you not, probably?

A. I would.—That last?

232 X Q. "Probably" is the last word.

A. Probably, yes.

233 X Q. And as far as the receiver is concerned, that adjustment could be made by the condenser h, could it not?

A. It could.

234 X Q. And the condenser h would have utility for that purpose, would it not?

A. It would.

235 X Q. And the resultant capacity of condenser h would not, under those circumstances, necessarily be .0046 microfarads, would it?

A. Not exactly, no. It could however be exactly that if you adjusted the capacity E of the transmitter.

[fol. 2058] 236 X Q. And if you adjusted the capacity E of the transmitter, it might very well not amount to 16,849 micromicrofarads, might it?

A. Not exactly.

237 X Q. In other words, I think the probabilities would be, and I think you will agree with me, that when transmitter and receiver were in tune, either the condenser E or the condenser h would not have the exact value of table 4?

A. Might not.

238 X Q. And if the final adjustment were made at the receiver, then it might well be that the capacity of the condenser h would not be .0046 microfarads?

A. No, it would not be exactly that, though it would not be very far from it.

239 X Q. And then I repeat, is there not an advantage in being able to adjust the condenser h beyond the possibility of adjusting it to the exact value of tune 4?

A. The value of adjustment for condenser h is to adjust it to the exact value of the received wave-length, which may not be exactly .0046.

240 X Q. In making the calculations upon which the results of defendant's exhibit BBB are based, did you or did you not consider the distributed capacity of the various coils?

A. I did not.

241 X Q. And that would affect in one way or another the resultant wave-length, would it not?

[fol. 2059] A. It would have affected it, to a certain extent, yes.

242 X Q. Or, if the wave-length were not affected, it would affect the value to which the condenser h would be set, would it not?

A. It would.

243 X Q. As I understand you, the secondary circuit of Fig. 2 of the Marconi patent, when h′ was omitted, is a series circuit, is that right?

A. A circuit made up of the elements which I enumerated before, in series, yes.

244 X Q. And, as I also understand you from your direct testimony, that is a high-impedance circuit, is that right?

A. A relatively high impedance circuit, yes.

245 X Q. And is that so, whether or not that circuit is tuned to the same frequency as the primary circuit?

A. The impedance in the secondary will be different, depending upon whether it is tuned exactly or not.

246 X Q. The impedance of a series circuit varies with the impressed wave-length, does it not?

A. Yes, it does.

247 X Q. At what wave-length, as compared to the resonant wave-length, is the impedance lowest in a series circuit?

A. The impedance is lowest in a series circuit at the point of resonance.

[fol. 2060] 248 X Q. And when the secondary circuit of Fig. 2 is tuned to the same frequency as the primary circuit, and that is the received frequency or wave-length, then the impedance of the secondary circuit is at its lowest point, is it not?

A. It is.

249 X Q. What are the elements that give the secondary circuit the high impedance that you spoke of?

A. The resistance of the coil j^2. You are speaking now of tune 4?

250 X Q. Tune 3 or tune 4, yes.

A. And in the case of tune 4, the resistance of the two coils g^2, the resistance of the capacity j^3, the resistance of the detector T, the reactance of all of the coils mentioned, and the reactance of the condenser. There is one further factor that is relatively negligible, and that is the parallel circuit with the chokes c' and c^2, battery to the recorder or whatever it is that is indicated by "R" in the diagram.

251 X Q. Can you give me any idea of the magnitude of the reactive impedance of the elements, assuming as we are assuming that the circuit is tuned to the received wave-length—that is, the total reactive impedance? Doctor Wheeler, I am informed that I can make the question a little simpler by saying "reactance" instead of "reactive impedance", and that is what I mean.

A. I wasn't worrying about that; I knew what you meant. As I testified yesterday, the approximate, very approxi-

[fol. 2061] mate inductance of the coil j^2 in the construction disclosed in Fig. 8 is somewhere between 1300 and 1400 ohms, microHenrys. Its reactance would therefore be two times pi times the frequency times that value, which is somewhere between 13 and 14 hundred. The reactance of the condenser j^3 I can tell you nothing about, because the capacity of that condenser is not disclosed, as far as I have observed.

252 X Q. Maybe I can make a short-cut, Doctor Wheeler. At the resonance frequency, do not all of the reactances in the circuit cancel out in their net result?

A. The reactances cancel out at the resonance frequency, the sum of the reactances.

253 X Q. And therefore the algebraic sum of the reactances in that circuit is zero, is that right?

A. That is right. The impedance, however, does not vanish. You have still the resistive component.

254 X Q. Now, according to my notes, you stated yesterday that there were two reasons why you considered the antenna circuit in tunes 3 and 4 as a series circuit, and the first reason was, because of the looseness of coupling between the primary and secondary circuits. Do you remember so testifying?

A. Yes.

255 X Q. I would like you to explain that, please—that is, why the looseness of coupling between the two circuits leads you to consider the antenna circuit as a series circuit.

[fol. 2062] A. Because the looseness of the coupling reduces the reaction of the secondary upon the primary, and because the value of the primary inductance is such that it passes a very small amount of the total current; that is, expressed in a different manner, the computation of the resonant frequency of the antenna circuit as a whole, with the loose coupling, shows that the resulting resonant frequency is substantially the same as that of the series circuit at f'Ag'hE.

256 X Q. And that is your explanation, as I understand it, of why the looseness of coupling between the primary and secondary circuits leads you to consider the antenna circuit as a series circuit, is that correct?

A. That is correct.

257 X Q. And does it follow that, if the coupling were closer, you would not consider the antenna circuit as a series circuit?

A. If the coupling were close enough, and if the impedance of the secondary at resonance were low enough, then you could not consider the antenna circuit as equivalent to a simple series circuit. I should add also that the value of the inductance of the coil j′ affects the question of whether the antenna circuit can be considered as a simple series circuit.

258 X Q. That was the second reason you gave yesterday, I believe, the size of the coil j′?

A. Yes.

[fol. 2063] 259 X Q. And with the close coupling, would that be closer than the critical value, or can you not tell that?

A. I cannot answer that question quantitatively.

260 X Q. And if the coupling were close enough so that the antenna circuit is not a simple series circuit, what would it be—a parallel circuit?

A. No. The antenna circuit would be a circuit having a series element in series with a parallel element.

261 X Q. The overall antenna circuit would still be a series circuit, would it not?

A. In that sense of the word, yes.

262 X Q. In the arrangement of Fig. 2 of the patent, as I understand it, the antenna current divides between the condenser h and the coil j′ as it passes from antenna to ground, does it not?

A. That component of the antenna current which is of the frequency of the received signal, yes.

263 X Q. And that means that h and j′ are in parallel in the antenna circuit, does it not?

A. Yes.

264 X Q. The circuit of Fig. 2 of the Marconi patent for tunes 3 and 4, referring to the antenna circuit, as I understand you, is also resonant at the wave-length given in column 5 of the defendant's exhibit BBB, is it not?

A. Correct.

[fol. 2064] 265 X Q. That is, it has two resonance points?

A. Yes.

266 X Q. —namely, the wave-lengths given in the column 3?

A. Yes.

267 X Q. —and also those given in column 5?

A. Yes, that is correct.

268 X Q. Now, did I understand you to say that if the wave-lengths set forth in column 5 ,which are in the neighborhood of 1630 or 1640 meters were applied to the antenna circuit, that circuit would not respond well to that wavelength?

A. The primary circuit?

269 X Q. Yes.

A. No, the primary circuit would respond to that.

270 X Q. Then, if the secondary circuit were readjusted or rebuilt to respond to the same frequency, the receiver would be an operative receiver at those wave-lengths, would it not?

A. At the 1630-40?

271 X Q. Yes.

A. Yes, but not if the receiver were tuned to the 200-400.

272 X Q. That is, the secondary of the receiver?

A. If the secondary were tuned to this 200-400, it would not respond to the 1630-40.

273 X Q. And if you wanted to receive the 1630-40, the patent instructs you, does it not, to tune the secondary cir-[fol. 2065] cuit to that wave-length?

A. You would have to tune it to that, yes.

274 X Q. And that is taught in the patent, is it not? I refer you to line 118, at page 2 and following.

A. Yes, I guess that is a fair conclusion from that.

275 X Q. And under those circumstances, what type of antenna circuit would it be? Would it be a simple series circuit?

A. I have not figured that case, owing to lack of time, and I would prefer not to express an opinion until I had had opportunity to do that.

276 X Q. Wherein lies the difficulty there, Doctor Wheeler?

A. There is no difficulty.

227 X Q. Well, why are you unwilling to express an opinion? That is what I mean.

A. But I am loath to express an opinion where I have not figured out quantitatively the result.

278 X Q. In that arrangement, namely, receiving in the neighborhood of 1630 meters, with both primary and secondary tuned to that wave-length, can the effects of the coil

j' be disregarded, as you testified that they could be, for the wave-lengths, column 3 of the exhibit?

A. I cannot answer that definitely without carrying out the calculations.

279 X Q. Now, am I correct in understanding defendant's exhibit BBB to mean, as to column 3, that the wave- [fol. 2066] lengths given there are the resonant wave-lengths of the antenna circuit as a whole?

A. Yes.

280 X Q. And is the same true for column 5?

A. Yes.

281 X Q. That is, it has taken all elements of the antenna and associated elements into consideration?

A. What do you mean by "associated elements"?

282 X Q. Whatever elements might have any effect on that wave-length, or on the resonant wave-length of the antenna.

A. The answer is Yes.

283 X Q. Would relative positions of parts of the primary and secondary circuits have any effect?

A. Certainly.

284 X Q. And did you assume certain relative positions in making these calculations?

A. I assumed the position of the primary and secondary of the transformer j', j^2, as given in the specification. I assumed where the two coils g^2 were included in the secondary that they were so placed as to have no effect upon the primary circuit, and I assumed that the antenna structure was erected in position such that its constants were not affected by neighboring objects.

285 X Q. In your discussion of coupling coefficients and coupling effects between the primary and secondary cir- [fol. 2067] cuits, were you considering only the magnetic coupling, or both the magnetic and capacitive coupling?

A. I was considering both, though in neither case could I get a quantitative estimate as to the amount of the coupling from the disclosures in the patent.

286 X Q. Does the Bureau of Standards bulletin No. 74, to which I think you referred, give the formula or formulas for the coefficient of coupling, using that term in the sense which I think you called "unconventional", and in which you used it in your direct-examination?

A. I cannot answer off-hand. I do not remember. My impression is that it does not.

287 X Q. Does it, so far as you know, give any formulas that show that changes of the impedance of the secondary circuit have a greater effect on reflection of reactance and resistance into the primary than changes in the coupling have?

A. It gives formulas from which such a conclusion could be deduced.

288 X Q. Will you refer to them, please.

A. Page 49, the equation which is not numbered but is in the fourth line from the bottom of the page. I should amend that reply to say that that equation referred to is only one of the factors in this that I called yesterday the second kind of coefficient of coupling.

289 X Q. Do I understand you to mean that the formula [fol. 2068] to which you have referred shows that the changes in the impedance in the secondary circuit have a greater effect on reflection of reactance and resistance into the primary than changes in coupling have?

A. Not necessarily.

290 X Q. You mean I am not to understand that necessarily, or what does your answer mean?

A. That with proper values in that formula you can deduce a value for this coupling coefficient of which that formula is an expression, which will show a greater effect of the secondary reactance, of changes in the secondary reactance, than of changes in the mutual inductance between the two circuits.

291 X Q. The mutual inductance is not the only thing that enters into a coupling coefficient in the usual sense, is it?

A. No.

292 X Q. The inductance of each circuit enters into it, does it not?

A. If it is a case of magnetic coupling, yes.

293 X Q. Incidentally, are the primary and secondary circuits of Fig. 2 directly coupled?

A. Not what is known as "direct"—well, they are coupled by their magnetic field.

294 X Q. Yes, but is that what is ordinarily known as "direct coupling"?

[fol. 2069] A. It is so specified in the figure on that page.

295 X Q. Which figure?

A. Figure B on page 49.

296 X Q. And what does the legend under the figure show, with respect to figure B?

X. Oh, yes—"inductive coupling" they call it. That is my mistake, I beg pardon. "A" shows the direct coupling.

297 X Q. There is a figure there, showing direct coupling, is there not?

A. There is a figure. Figure A shows direct coupling, figure B, inductive coupling, and figure C, capacity coupling. I was misled for a moment, for the words "inductive coupling" were right under the figure as printed.

298 X Q. That is, you looked upon figure B as direct coupling for a moment, is that it?

A. Figure B was "direct coupling", as I read it for the moment, yes.

299 X Q. And you were willing to take that statement that it was a direct coupling?

A. No, I was not willing to take it. I simply said that this was what it said on that page, and I expected to clear it up in further answers by saying it was not what I understood by "direct coupling".

300 X Q. Is the coupling between the primary and secondary circuits of Fig. 2 of the Marconi patent what you [fol. 2070] understand by "direct coupling"?

A. No.

301 X Q. Which type of coupling is that?

A. Inductive coupling—what they call here "inductive coupling".

302 X Q. There is capacitive coupling present, is there not?

A. There is capacitive coupling present, and, in certain circumstances of the disclosure, that, I think will probably be the controlling coupling.

303 X Q. I think you said yesterday it was predominant under certain circumstances.

A. Yes.

304 X Q. I think you also said that capacitive coupling effects are quite complex, is that true?

A. That is correct.

305 X Q. Those capacity couplings between the circuits do enter into the coefficient of coupling between the circuits?

A. They do.

306 X Q. And they enter into both the conventional and the unconventional type of coefficient of coupling, as you have used those terms?

A. Yes.

307 X Q. Incidentally, the formula to which you referred, on page 49, is a formula relating to direct coupling, is it not?

A. Yes.

[fol. 2071] 308 X Q. The capacity coupling between the primary and the secondary circuits is directly affected by relative positions of elements in those circuits, is it not?

A. It is.

309 X Q. And in order to determine the actual capacity coupling between the circuits, you would have to know the relative positions of the parts, would you not?

A. I would.

310 X Q. As I understand your testimony with respect to the operation of the circuits of the Marconi patent, that is all based on calculations, is it not?

A. Yes.

311 X Q. That is, you have not set up the arrangement of Fig. 2, or any of the arrangements of Fig. 2, and observed their operation?

A. I have not done it, personally.

312 X Q. Referring to tune 3 of the tables of the patent, suppose that that circuit were set up with 21 turns in g′, and otherwise as described except that the capacity of the condenser h were in the neighborhood of 50 micromicrofarads; can you say whether or not the antenna circuit then would be a simple series circuit, or not?

A. It would be impossible to obtain a capacity of 50 micromicrofarads from the condenser as specified.

313 X Q. Supposing you used another condenser, then, [fol. 2072] and attained that capacity; on that basis, can you answer my question?

A. A condenser with the sam maximum value?

314 X Q. I do not see that that makes any difference, in view of the fact that, as such, it has a capacity of 50 micromicrofarads. You may take any maximum value you want.

A. If you take a small enough value of h, leaving j′ the same, g′ the same, there will be more difference between the resonance frequency as computed by the method of column 3 from that computed by the method for column 4, than in the cases of the tunes shown.

315 X Q. My question was, whether with a capacity for h of approximately 50 micromicrofarads, would the antenna circuit be a simple series circuit.

A. It would not show the same resonant frequency, exactly, as a simple series circuit, consisting of the antenna, the coil g′, and the small condenser h.

316 X Q. Does that mean that it is not a simple series circuit?

A. It is a matter of definition.

317 X Q. You have used the term, Doctor Wheeler. You must have had some definition in mind.

A. I say that this circuit, whatever you want to call it, which is a circuit consisting of a series element in series with a parallel, would not behave quite so closely, as far [fol. 2073] as the resonant frequency goes, to that of a simple series circuit, as if the condenser h were large.

318 X Q. Then I take it the border line between a simple series circuit and one which is not a simple series circuit is not a fixed or a definable border line, is that right?

A. Yes, it is a series circuit with respect to the resonant frequencies that it will exhibit, is what I have meant by saying the circuit is a simple series circuit.

319 X Q. Then let us go a little further and reduce the capacity of condenser h to zero: is it a simple series circuit, or not?

A. You mean, by reducing the capacity to zero, for practical purposes you remove the condenser h from the circuit?

320 X Q. Is that the electrical equivalent of reducing the capacity to zero?

A. Yes; then it would leave the series circuit, consisting of the antenna capacity in series with the coil g′ and j′ in series.

321 X Q. Is that a simple series circuit?

A. That would be a simple series circuit.

322 X Q. With respect to reflection of reactance or resistance effects from the secondary into the primary circuit, am I to understand that where the secondary circuit is a series circuit and the detector is a high-resistance detector, that fact alone means that there is necessarily loose [fol. 2074] coupling between the primary and the secondary circuits, regardless of other conditions?

A. No, it means there is loose coupling of the unconventional second kind that I mentioned.

323 X Q. There is loose coupling of the unconventional kind or the "new baby" type I think you called it yesterday?

A. Yes.

324 X Q. Regardless of the coefficient of coupling between the circuits, using that term in the conventional sense?

A. Yes.

325 X Q. That is, even if the primary and secondary circuits are very closely coupled, in the usual sense of that term?

A. A high-resistance secondary will then make the unconventional coupling coefficient relatively small.

326 X Q. In defendant's exhibit BBB, was there any reason for omitting the wave-lengths in column 2 for the first two arrangements of tune 3?

A. No reason that I recall at the present moment.

327 X Q. Did you calculate the transmitter wave-lengths on the basis of the transmitter circuits as described in the patent?

A. No, I did not.

328 X Q. That is, the approximate wave-lengths of the transmitters as set forth in column 2 of that exhibit are [fol. 2075] merely restatements of what appears in column 3, is that right?

A. Approximately so; yes.

329 X Q. And there is no other basis or source for the figures in column 2?

A. Well, with some rather wild guesses at the oscillation transformer dd', I would expect wave-lengths of the same order of magnitude at least as those given for the more accurately calculable resonance frequencies of the primary of the receiver.

330 X Q. As far as specific figures in column 2 are concerned, there is no basis for them except what appears in column 3, is that right?

A. Practically so.

331 X Q. I do not know what "practically so" means.

A. As I say, I was influenced from the general consideration that all of the circuits must be in tune according to the specification, and what rough guesses I could make about the primary circuit, that it seemed reasonable to suppose that the frequencies would be about there, but, perfectly true, there are no definite calculations back of those figures in column 2 that I have made.

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[fol. 2076] By Mr. Blackmar: (Tr. 2198)

332 X Q. Doctor Wheeler, you spoke of a simple series circuit, I think. You used that term?

A. I did.

333 X Q. What were you distinguishing from in that term?

A. A circuit in which the frequency-determining elements are in parallel with each other, with respect to the applied electromotive force.

334 X Q. Would that be a complex series circuit, or a parallel circuit, or what?

A. Would which be a complex?

335 X Q. The thing from which you were distinguishing.

A. I would make the distinction between a simple series and a simple parallel circuit; between a simple series circuit and the complex series circuit.

336 X Q. Now, referring to open antennas and radio receivers, 2-circuit receivers such as shown in the Marconi patent, do you ever have a simple parallel circuit in the antenna?

A. Not including the antenna itself.

337 X Q. I am talking of the whole circuit.

A. No.

338 X Q. Then it is either a simple series circuit or a complex series circuit, is that right?

A. Yes, that is right.

339 X Q. Referring to defendant's exhibit BBB, what is the nature of the antenna circuit of column 5? Is that a simple series circuit or a complex series circuit?

[fol. 2077] A. Complex.

340 X Q. And what is it that makes it so?

A. The presence of the simple series element, consisting of the antenna and loading coil in series with the parallel element, consisting of the coil j', condenser h.

341 X Q. The coil j' and condenser h taken alons, as I understand it, have a slightly longer resonant wavelength than the antenna circuit taken as a whole, is that correct—at least as to the cases of tune 3?

A. Yes, as respects tune 3, it has.

342 X Q. And in that case, for the wavelength of the entire antenna circuit, can the parallel circuit hj' be considered to act either as an inductance alone or as a capacity alone?

A. For tune 3?

343 X Q. Referring to tune 3, yes, and also referring, still, to columns 5 and 6 of the exhibit.

A. As far as a received frequency of approximately 200 meters goes——

344 X Q. (Interposing:) No, I think that is not what I was referring to. Assuming the received wave-length is that shown in column 5.

A. (Cont'd.:) If the received wave-length is approximately 1630-40), the resonant frequency of the antenna circuit would be the same to within a fraction of one percent, as with a simple series circuit consisting of the antenna, the coil g', and the condenser h, to ground. The frequency [fol. 2078] is very nearly the same as that of the parallel combination of hj', but the action of the circuit as a whole is barely distinguishable from that of the simple series circuit mentioned.

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346 X Q. I am not sure whether that answers my question. I will put it in a different way, Doctor Wheeler. The resonant wave-length of hj' taken alone is 1640 meters, is that right?

A. That is correct.

347 X Q. Now, assume that that is in a circuit: let hj' constitute one element of a circuit, and the wave-length of 1628 meters, to take case 1 of tune 3, is applied to that circuit: For that wave-length, does the combination hj' act as a capacity?

A. Very nearly so.

348 X Q. What does "very nearly so" mean?

A. By "very nearly so" I mean that the frequency response, resonant frequency, is what one would obtain with a capacity, alone, to within approximately 12 parts in 1600.

349 X Q. Does that mean that it does or that it does not act as though it were a capacity?

A. If that whole primary hj' combination were shut up in a box so that you could not see it, and if you applied external electromotive force and measured voltages and currents to the thing, and plotted a curve, voltage against [fol. 2079] current, you could not distinguish the result from that of a condenser alone to within the precision mentioned, 12 parts in 1600.

350 X Q. That is, if you applied to that box a wave-length of 1628 meters—and maintained it, constant?

A. Yes.

351 X Q. The indications would be that whatever was in that box had a capacity reactance, is that right?

A. Yes, that's it.

352 X Q. Now, the antenna circuit, column 5, as I understand it, consists of the antenna f′, the coil g′, at least in cases 2 and 3 of tune 3——

A. Yes.

353 X Q. —and whatever is in this box that we speak of, namely, h and j′, and the ground connection, is that right?

A. Yes.

354 X Q. I understood you to say a few minutes ago that that was not a simple series circuit but a complex series circuit, is that right?

A. If both j′ and h are in the box, parallel.

355 X Q. And if you cannot tell which is in the box, how can you tell whether it is a simple or a complex series circuit?

A. No way of telling.

356 X Q. The only way you can tell is by measuring what occurs when that wave-length is applied, isn't that right?

[fol. 2080] A. It would behave like a simple series circuit there, and you could not tell whether it was or was not a complex series circuit.

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358 X Q. As far as external behavior was concerned?

A. Yes.

By Commissioner Gordon:

359 Q. I do not just understand that last answer, and particularly with the idea of this whole thing being in a box and applying tests to it. If, for instance, you applied a constant electromotive force, would that, a test of that kind, indicate it was a simple series circuit?

A. No, you would have to vary some element in order to determine and plot a characteristic curve, like whatever was included between terminals on the box. You would have to vary their frequency or the voltage that is applied.

360 Q. But now if you had a condenser there alone in the box, a simple series circuit and condenser, and applied a

direct electromotive force to that, no current would flow, would it?

A. Applied a DC, no, no current would flow except for leakage current.

361 Q. But if you had this particular combination in a box and applied a DC current to it, what would happen?

A. Current would flow. You would know in that case [fol. 2081] by applying DC. I was considering only the application—I thought that was limited by the question to a signal voltage applied.

362 Q. I do not know what it was limited to, but you said if it was in a box you could not tell whether it was a simple series circuit or not, as I understood.

A. All predicated upon the assumption of an applied AC electromotive force.

By Mr. Blackmar:

363 X Q. Of the wave-length contained in column 5 of exhibit BBB, is that right?

A. Yes, that is right. That is the way I understood it. You are perfectly correct, Your Honor; if you will allow me to apply DC I will tell you whether there is a coil in there or not.

364 X Q. Incidentally, I understand you to say that in column 5 the wave-length of the entire antenna circuit is less than the resonant wave-length of hj′ in the three cases under tune 3, and is greater, in tune 4. Can you explain that?

A. All the differences between columns 5 and 6 are very small, a small fraction of one percent, and, to be frank, I do not know at the present moment what the cause of any difference of excess or defect over 1640 means. There may be some reason, but I do not see any at the present moment. [fol. 2082] 365 X Q. What bothers me, for example, in the case of Case 3 of Tune 3, is that you start with a resonant wave-length of 1640 meters for the parallel circuit hj′, and then you add a coil g′ and an antenna on the one end of it, and connect the other end to earth, and get a resultant wave-length which is shorter.

A. That is, now, the tune 3?

366 X Q. Yes.

A. Yes.

*   *   *   *   *   *   *

I am sorry, I cannot answer, right now, I would have to look at the calculations and run through them again to make sure if there were any physical reason for that discrepancy, or whether it was simply a matter of the numerical values being different in the different cases. I cannot answer it without going over the calculations.

368 X Q. I am not going to ask you to do that, but certainly if you add a coil g' with 21 turns in it, that alone will increase the wave-length, would it not, rather than decrease it?

A. That would increase the wave-length if it were not for the h. By itself, it would increase the wave-length, yes.

369 X Q. And added to a circuit hj', it would increase the wave-length, would it not?

A. Well,——

[fol. 2083] 370 X Q. I won't ask you to speculate on it, Doctor Wheeler.

A. I cannot give you the answer, right off. I am sorry. I wish I could. If I were quick enough, maybe I could, but I cannot, right now.

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(Tr. 2206)

378 X Q. Just one more subject, I think. Suppose that with the Marconi patent before you in 1904 you set up a transmitter and a receiver in accordance with tune 3 of the tables on page 4; and I will ask you to assume that as a matter of fact the wave-length transmitted by the transmitter was 135 meters; maybe something went wrong, or whatever the reason may have been. Now, you are located at the receiver. Can you tell me what you would do to try to receive that transmitted signal, with the patent in front of you and with all of its teachings before you?

A. I would first reduce the number of turns included in the coil g', if necessary, to zero. If that had not shortened the resonant wave-length of the antenna circuit sufficiently, I would then reduce the value of the capacity h.

379 X Q. And do you think from your calculations that by doing those two things you could receive a wave-length of 135 meters?

[fol. 2084] A. I think so. I could not tell, without a little computation, exactly, but I think so.

Mr. Blackmar: That is all.

Redirect examination.

By Mr. Houghton:

380 R. D. Q. Doctor Wheeler, in answer to a question on cross-examination, you stated that the thickness of the silk insulation on the wire used in winding coil j′ would affect the value of the inductance calculated for that coil. What can you say as to the extent of that effect?

A. I would say that that effect would be very small, probably less than one per cent.

381 R. D. Q. In answer to another question on cross-examination, you stated that the distributed capacity of coil j′ had been neglected in determining the overall resonant wave-lengths for tunes 3 and 4. What can you say as to the extent of the effect of considering the distributed capacity on the overall wave-length?

A. Distributed capacity of the coil j′ would be very small in comparison with the value 4600 micromicrofarads, for the capacity h, and as it would effectively be in parallel with h, it would increase that capacity by a small amount, which I should judge to be not as great as one per cent.

382 R. D. Q. Also, on cross-examination, you stated that increasing the closeness of coupling between the primary and secondary of the Fig. 2 of the Marconi patent, would [fol. 2085] affect the tuning of the primary circuit. What can you say as to the extent of that effect?

A. In my opinion that effect would also be very small. I cannot put any numerical estimate on it, however.

Mr. Houghton: That is all.

Recross-examination.

By Mr. Blackmar:

383 R. X Q. Besides the distributed capacity of j′, am I correct in understanding that you also neglected the distributed capacity of the coil g′?

A. I did.

384 R. X Q. And the distributed inductance of the antenna?

A. No.

385 R. X Q. That was taken into consideration?

A. That was taken into account.

386 R. X Q. In the case of tunes 1 and 2, where there is no condenser h, is the distributed capacity of j′ still negligible?

A. That depends upon your definition of "negligible". The effect will be greater. The distributed capacity of that—now, I am not saying that this is my guess as to the distributed capacity, but suppose it were 10 micromicrofarads, the capacity with which it is in series is 240, I have assumed, micromicrofarads, and the capacity of those in series would be less than 10. It would make considerable difference in that case, if we were to take it into account.

[fol. 2086] (Tr. 2263)

Louis Alan Hazeltine, a witness produced on behalf of the defendant, having been first duly sworn by said Commissioner, was examined, and in answer to interrogatories testified as follows:

Direct examination.

By Mr. Edwards:

Q. 1. Dr. Hazeltine, where do you reside?

A. I reside at Hoboken, New Jersey.

Q. 2. What is your occupation?

A. I am professor of physical mathematics at Stevens Institute of Technology.

Q. 3. Will you state what training and experience you have had in the development of the radio art, particularly with respect to radio transmitters and receivers, and also what experience you have had in connection with the licensing under patents in that art?

A. I graduated from Stevens Institute of Technology in 1906 with the degree of Mechanical Engineer. The following year I spent with the General Electric Company in Schenectady, gaining practical experience in electrical engineering lines. I then joined the electrical engineering department of Stevens and remained with that department until 1925, becoming head of the department, with the title "Professor of Electrical Engineering," in 1918.

I started the special study of radio in the winter of 1914-1915, following the work of E. H. Armstrong with [fol. 2087] the regenerative vacuum tube. My study at

that time culminated in a paper on the Oscillating Audion, as it was then called, read before the Institute of Radio Engineers, which contained the first general treatment of that subject of a mathematical sort.

During 1918 and 1919 I was connected with the Washington Navy Yard, having to do principally with the development and design of radio receiving apparatus. During that time I designed the receiver known at the SE 1420, which superseded prior navy receivers and went into wide use. Subsequently, I continued in consulting radio work and also in my own experiments in radio engineering, and I developed the neutrodyne radio receiver, containing features on which patents were issued to me, and that receiver went also into very wide use, this time use by the general public in broadcast reception.

I was a member of the three of the four National Radio Conferences called by the then Secretary of Commerce, Herbert Hoover, to advise the Department of Commerce in the regulation of radio broadcasting, which was then in its infancy.

I have served the Institute of Radio Engineers in various capacities, having been president and having been in several periods a member of the board of directors. I am a Fellow of the Institute of Radio Engineers, of the American [fol. 2088] Institute of Electrical Engineers, of the American Physical Society and of the American Association for the Advancement of Science.

I have received the honorary degree of Doctor of Science from Stevens and the degree of Master of Arts from Columbia.

In connection with the development of the neutrodyne receiver I had the experience of negotiating with manufacturers looking toward licensing them to manufacture these receivers under my patents, and I entered into an agreement with a group of manufacturers to license them for a royalty that was based on 5 per cent. and which included whatever patents I had or might secure in the radio receiver field and also the services that I might give them in the development of that field and in any defensive litigation.

[fol. 2089] (Tr. 2316)

X Q. 138. Then is it not true that in considering the action of a particular circuit you must consider not only the con-

stants of that circuit but the constants of any circuit which may be coupled to it?

A. Yes, in general.

X Q. 139. And if, for example, you have an arrangement such as the antenna circuit of the SE 950 with the condenser in parallel to the primary coils, the frequency or wave-length to which the antenna circuit will respond depends not only upon the inductance and capacity of that parallel circuit but also upon the antenna capacity and inductance and other leads that may be connected to it and the associated secondary circuit; is that correct?

A. Generally, but not always with equal weight in the receiver in which the primary and secondary are inductively tuned. It was on this type of receiver customary to work with a loose coupling, and under those conditions the effect of one circuit on the other is relatively slight as compared with a neutrodyne, which you were asking about previously, where the coupling is relatively close. The antenna capacity is important in determining the natural wavelength or frequency of the antenna circuit, but in such a receiver as the 950, in the ordinary use, the antenna inductance is generally not of much importance because it is quite low.

[fol. 2090] X Q. 140. But the antenna capacity does have a substantial effect on the natural frequency of the closed circuit?

A. Oh, yes.

X Q. 141. The parallel circuit?

A. Yes.

X Q. 142. And in Figure 2 of the Marconi patent is it not also true that the frequency, to which the parallel circuit hj', will thus respond, is materially affected by the presence of the coil g' and the antenna?

A. I should suppose so; yes.

[fol. 2091] (Tr. 2426)

X Q. 229. It was pointed out that your report, Exhibit JJJ, as supplemented by Exhibit KKK, contained no reference to a comparative test of the series and parallel connections. In the first place, when you were on the Salem, on the trip that is covered by the report, do you remember whether or not you made such a comparative test?

A. I do not recall at the present time whether I did or not.

X Q. 230. Do you recall whether or not you utilized the parallel connection of the antenna condenser?

A. A recent reading of my report makes me state that I did this, yes.

X Q. 231. And is that referred to in either Exhibit JJJ or the supplemental portion of Exhibit KKK?

A. I will have to look and see. The use of a condenser in parallel with the primary coupling coil in the case of the Telefunken receiver used in this test is shown in both Exhibit JJJ and Exhibit KKK in paragraph 17. In addition, the same is shown for the Federal Receiver used in that test in Exhibit JJJ.

X Q. 232. Was either of those receivers so arranged that the antenna condenser could have been connected in series, as well as in parallel?

A. Yes.

[fol. 2092] X Q. 233. Why did you use the parallel connection, rather than the series connection?

A. Because of the length of wave, I obtained better results. That was known before I went on board, and was continued as the optimum condition to use.

X Q. 234. And in making these tests, which are referred to, in which you used that connection, were you endeavoring to secure the strongest signal that you could secure?

A. Yes, I was.

X Q. 235. And in those tests, then, you used the parallel connection, rather than the series connection?

A. Yes.

[fol. 2093] (Tr. 2460)

Mr. Edwards: I offer in evidence as one exhibit copies of patents to DeForest No. 1507016, granted September 2, 1924, and 1507017, granted September 2, 1924.

(Received and marked Defendant's Exhibit UUU.)

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(Tr. 2461)

Mr. Edwards: I also offer in evidence Armstrong patent No. 1113149 granted October 6, 1914.

(Received and marked Defendant's Exhibit VVV.)

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[fol. 2094] (Transcript, page 2530)

Mr. Edwards: I also offer in evidence a photostat of a certified copy of the specification, claims, oath, and draw-

ings in the matter of the application of Reginald A. Fessenden, Serial No. 740,429, for Patent No. 706,735, granted August 12, 1902, to Reginald A. Fessenden.

Mr. Blackmar: In view of the fact that the patent, itself, is before Your Honor, I have no objection to this exhibit. In fact, I think it is necessary, to explain the patent.

Mr. Edwards: I ask that this be marked "Defendant's Exhibit CCCC."

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(Transcript, page 2541.)

Mr. Edwards: In view of the discussion, it is stipulated, subject to correction should error appear, that the La-Fayette station located at Croix d'Hins, near Bordeaux, France, was constructed by the United States Navy and completed and first commenced transmission, on August 21, 1920.

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[fol. 2095] GEORGE H. CLARK (Tr. 2582)

Q. 80. I have here three books, the first two of which are entitled "Manual of Wireless Telegraphy (Radio) for the Use of Naval Electricians," one dated 1913 and the other 1915; and also one entitled "Robison's Manual of Radio Telegraphy and Telephony—1918." Will you state what you know of your own personal knowledge of the distribution of these books in the Navy Department or throughout the Navy Department?

A. I saw one in every shore and ship station that I ever visited in the Navy.

Mr. Blackmar: I offer in evidence as Plaintiff's Exhibit 470 the title page and pages 134 and 137 of the 1913 Manual.

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(Tr. 2587)

Mr. Blackmar: I would like to add, to the offer on that, page 187.

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Mr. Blackmar: I also offer in evidence the title page and pages 134, 137, and 189, from the 1915 Manual, as Plaintiff's Exhibit 471.

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[fol. 2096] (Tr. 2588)

Mr. Blackmar: I also offer as Plaintiff's Exhibit 472 the title page and pages 142, 145, and 215 of "Robison's Manual of Radio Telegraphy and Telephony—1918."

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Q. 81. Mr. Clark, I note in each of Plaintiff's Exhibits, 470, 471, and 472, the following statement:

"The aerial circuit should be tuned with a variable condenser in series for short waves and in parallel (around inductance) for long waves (Fig. 92)."

As one familiar with the art in the period 1913 to 1918, what is your understanding of what is meant in that statement by short waves and by long waves?

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(Tr. 2589)

A. The terms "Short waves" and "long waves" refer respectively to the lower and upper ranges of the wave-length range of the receiver in each case. They do not refer to specific wave-length designations or values.

Q. 82. That is, it does not mean above and below a certain figure under all conditions?

A. No.

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[fol. 2097] Clarence Denton Tuska (Tr. 2612)

Clarence Denton Tuska, a witness produced on behalf of the plaintiff, having been first duly sworn by said Commissioner, was examined, and in answer to interrogatories testified as follows:

Direct examination.

By Mr. Blackmar:

Q. 1. Mr. Tuska, will you give your name to the reporter?

A. Clarence Denton Tuska.

Q. 2. And your residence?

A. 717 West Mt. Airy Avenue, Philadelphia, Pennsylvania.

Q. 3. And what is your present occupation?

A. I am employed by the Radio Corporation of America at Camden, New Jersey, where I am a member of the Patent Department.

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(Tr. 2613)

A. I became interested in radio about 1908 as an amateur. From about 1908 until about 1923 I had an amateur transmitting-receiving station, and in the latter part of that time owned and operated a broadcasting station. About 1915, with the late Hiram Percy Maxim, I founded the American Radio Relay League, which is as association of amateur radio operators. In connection with that League, I started a magazine, which is now known as QST. QST was the official organ of the American Radio Relay League, and in [fol. 2098] the early days it was necessary for me to act as editor and advertising solicitor and, in fact, all positions. In this capacity, I was placed in touch with many of the leading manufacturers of radio apparatus in the years 1915 and up to the beginning of the entry of the United States into the World War.

At the beginning of the war, I was first given a position as a civilian engineer in the Signal Corps of the U. S. Army. In the latter part of 1917 I was commissioned a second lieutenant in the army and was occupied with the establishment of a radio school at Ellington Field, Houston, Texas. I had complete charge of that radio school, and after it was operating and training radio operators and radio mechanics, I was sent to the University of Texas to revise the radio course of study which was being given there in training radio mechanics or radio operators for the Air Service.

I was assigned to Love Field, Dallas, Texas, where I had charge of the radio officers' course for a short time.

I was then sent to Camp McClellan at Anniston, Alabama, where I was detached under instructions to organize a school for the training of radio operators in the United States Field Artillery, so that those operators might co-operate with the Air Service observers in the direction of artillery gunfire.

[fol. 2099] I neglected to mention that prior to the entry of the United States into the World War I acted as a con-

sulting radio engineer for the A. C. Gilbert Company of New Haven, Connecticut, and I also——

Q. 7. What was the business of that company?

A. The A. C. Gilbert manufactured mostly toys; but at the time, Mr. Gilbert, who was president of the company, wanted to manufacture radio receivers of the amateur type. In connection with my first employment with Mr. Gilbert, I came in contact with patent matters and license matters as prevailed in the field to 1916.

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[fol. 2100] (Tr. 2617)

A. During the time when I first acted as a consulting engineer for the A. C. Gilbert Company, I was attending Trinity College in Hartford, Connecticut, where I obtained the degree of Bachelor of Science. At the expiration of the World War I organized the C. D. Tuska Company, for the purpose of manufacturing radio devices, and, at the same time, I continued—or renewed, rather—my relations with Mr. Gilbert. The renewed relations extended only over a few months, when my duties in the Tuska Company required all of my time.

The radio business of the Tuska Company grew rapidly as broadcasting came into the field. Since my duties included not only the design of apparatus, but also the management of the company, I became very much interested in patent matters, and filed a number of applications for U. S. Letters Patent.

Q. 9. Were those on inventions that you yourself had made?

A. Most of them were on inventions that I had made, but some were made by other employees of the company. Of these applications, I do not know the exact number maturing into patents, but it is probably fifteen or eighteen. I also obtained and negotiated licenses for the company.

About 1925 or 1926 Mr. A. Atwater Kent, of the Atwater Kent Company, became interested in the Tuska Company, [fol. 2101] and acquired all of the capital stock. This purchase was made primarily to obtain the patent rights which I had acquired for the company. The activities of the Tuska Company were suspended, and I joined the Atwater Kent Company in 1926. Prior to my official employment, I worked for Mr. Kent on patent matters, particularly in

regard to litigation. I was with the Kent Company from 1926 to 1935, and during that time assisted in the preparation of the defense of suits filed against the Kent Company, and testified in several of the suits; for example, RCA vs. Edmonds.

Q. 10. Pardon me. Were you testifying as an expert, or a fact witness, or what?

A. I think in those suits it was a mixture. I also assisted in the preparation of Hazeltine versus Wildermuth. Edmonds was also involved in a suit with Latour. I did a great deal of work in the accounting which resulted from the suit entitled Hazeltine versus Wildermuth.

In 1935 I resigned from the Atwater Kent Company and joined the Patent Department of the Radio Corporation of America. Since I have been with the Radio Corporation I have testified as an expert in the suit in Boston, which, I believe, was Cabot versus Eastern Radio.

Q. 11. Was that a suit involving radio apparatus?

A. Yes.

* * * * * * *

[fol. 2102] Q. 63. Have you read and are you familiar with Marconi patent No. 763,772?

A. Yes, I have, and I am familiar with it.

Q. 64. In your opinion, is the disclosure of the patent summed up in the tables on page 4 of the patent?

A. No. The tables on page 4 are included in the patent as examples, and not a summation.

Q. 65. And, in your opinion, are there other combinations that are within the teaching and disclosure of the patent?

A. Yes; an infinite number of combinations.

Q. 66. Do you understand that the patent teaches that the condenser h in Figure 2 is or is not to be variable?

* * * * * * *

A. I understand that the condenser h of Figure 2 is variable. The specification points that out rather distinctly, first describing the condenser e of the transmitter, and then later saying that the condenser h is similar to the condenser e which is previosly described as variable.

Q. 67. Now, will you please describe the operation of Figure 2 of the Marconi patent, as you understand it, from the disclosure of the patent?

Q. 68. I will add to my question that, in answering, I would appreciate it if you would comment on the descrip-

[fol. 2103] tions of the operation of that figure by defendant's witnesses.

* * * * * * *

(Tr. 2640)

A. Figure 2 of the Marconi patent is a schematic diagram of a radio receiver. It includes an antenna circuit comprising a capacity area f′, a lead A, a loading coil g′, a coupling inductor j′, which is shunted by a tuning condenser h, and a connection to Earth. The secondary circuit includes coupling inductors j_2 and a pair of variable loading inductors g_2, the shunt condenser h′, coherer T, a by-pass condenser j_3, choke coils C_1, C_2, a battery B, and a receiver R. The patent teaches different modes of operation of this circuit. In the simplest mode of operation, the circuit h j′ would be tuned to the transmitting wavelength and the secondary circuit, by adjusting the shunt condenser h′ or the loading coils g_2, would be tuned to the same wavelength.

Q. 69. Let me interrupt you. You say that the circuit h j′ would be tuned to the desired wavelength. Do you mean that circuit, regardless of its associated elements?

A. Oh, no. The circuit including all of the associated elements, and including all of the reactive effects of each on the other. That is generally recognized when we speak of tuning.

[fol. 2104] Q. 70. You mean the entire primary circuit?

A. Entire primary circuit. It includes the antenna capacity, antenna inductance, and any other capacities or inductances which are associated.

Q. 71. Including g′, for example?

A. Including g′, if g′ is used.

Q. 72. All right.

A. While this is the simplest mode of operation, the patent teaches other modes by means of which octave tuning may be employed. In the case, for example, of tune No. 3, as noted in the tables on page 4 of the patent, the transmitting wavelength will be approximately 200 meters.

Q. 73. Was that figure given by any of defendant's witnesses?

A. That is the approximate figure given by Dr. Wheeler. I have independently checked that figure, and find that 200 is approximately the correct wavelength for the transmitter circuits of tune No. 3.

[fol. 2105] Q. 74. Let me interrupt on another thought there. You say you have calculated; is it possible to calculate that with a high degree of accuracy upon such data as there is in the patent?

A. I am not sure whether you mean that the calculations can be correct, or whether you mean that, having calculated it, the operating wavelength will correspond exactly to the calculations.

Q. 75. That is what I mean, the latter.

A. With the latter meaning, no. You can calculate it to a fair degree of approximation, but not exactly.

Q. 76. You have stated the transmitting wavelength as approximately 200 meters. Will you continue?

A. The receiving station for the 200-meter wave is also [fol. 2106] specified. It will be observed that the loading coil g′ in the receiver is adjusted up to 21 turns, which permits the operator to resonate the antenna circuit as a whole to 200 meters. But there is also an element h j′, which must receive consideration. Dr. Wheeler calculated that the frequency to which h j′ would respond when connected in the antenna circuit would be about 1640 meters. Assuming that Dr. Wheeler's calculations are sufficiently accurate for our purpose, it follows that the antenna circuit as a whole will be responsive to wavelengths of 200 meters and approximately 1600 meters. It is apparent that 1600 meters is eight times the wavelength of 200 meters or corresponds to what Marconi calls octave tuning.

I am sure that Marconi had this type of tuning in mind in describing tune No. 3, because I have determined the essential information which Dr. Wheeler said he was unable to calculate. In so far as his calculations are concerned, I agree with Dr. Wheeler that it would be very difficult to calculate the secondary of the receiving transformer No. 2. Unfortunately, Dr. Wheeler made a glaring error when he considered this portion of the circuit. Because he couldn't calculate the secondary of the receiving transformer No. 2, he elected to take an entirely different transformer, simply because he could calculate it.

Q. 77. In which figure of the patent is transformer No. 2 shown?

[fol. 2107] A. Transformer No. 2 corresponds to the schematic diagram Figure 6.

Q. 78. And in which figure of the drawing is the trans-

former shown as to which Dr. Wheeler made his calculations?

A. As I recall his testimony, he used the fourth form of induction coil, as shown in Figure 8.

Q. 79. And which form of transformer coil is required by Tune No. 3 that you are now considering?

A. Tune No. 3, as now considered, requires receiving transformer No. 2, which is schematically illustrated in Figure 6.

* * * * * * *

(Tr. 2644)

A. The receiving transformer No. 2, which I am now considering, has a secondary circuit which has two serially connected windings——

* * * * * * *

A. Transformer No. 2 is described in Figure 6 and in the specification, page 3, lines 85 to 108, inclusive. As I pointed out, the receiving transformer No. 2 has two windings, j_2, which are serially connected. Each of these windings includes 500 turns, making 1,000 turns in all.

Furthermore, they are wound in layers, so that the first layer includes 77 turns and the subsequent layers each include a lesser number of turns. If Dr. Wheeler had consid- [fol. 2108] ered this transformer secondary and had attempted to estimate carefully the probable inductance, I think he would have reached the conclusion which I reached, namely: that the secondary would be tuned to 1600 meters. Confronted with the difficulty of calculating, I had a duplicate made, so that I would be sure of my preliminary estimate, and I found that, as closely as can be determined, making due allowance for the difference in the British and American wire gauges, the transformer secondary of Figure 6 resonates at 1600 meters. It now becomes very clear that Marconi described, in tune 3, a transmitter operating with both circuits tuned to 200 meters radiating a wavelength of 200 meters, which is received by the circuit of Figure 2 at both the fundamental and an octave, and the octave is impressed on the secondary. This mode of operation is a feasible one, and clears up a great deal of the misunderstanding which I mentioned when I began the original answer.

I would like to go on. It should be understood that since the antenna circuit as a whole responds to 1600 meters, it is not essential that it also respond to 200 meters. Furthermore, I should like to emphasize that the octave mode of tuning is not the only method of tuning the circuit of Figure 2 to a transmitter radiating waves on 200 meters. The patent teaches that the antenna and secondary circuits of the antenna should be tuned either to the fundamental fre- [fol. 2109] quency of the transmitter or to multiple of that frequency; and in discussing the second case we should not lose sight of the normal mode of operation in which the antenna circuit as a whole would be tuned to the fundamental and the secondary as a whole would be tuned to the same wavelength. It should also be understood that the transformer No. 2 given in the receiving table would not be used, as this transformer is designed to operate at the long wavelength.

By the Commissioner:

Q. 83. I wonder if you can point out to me where the specification ties in the transformer, receiving transformer, illustrated in Figure 6 of the drawings, and described on page 3, beginning with line 85, with a specification number for the transformer.

A. May I answer that at length, your Honor?

Q. 84. Surely.

A. The first receiving transformer is described on page 3, in the first full paragraph.

By Mr. Blackmar:

Q. 85. Of the second column?

A. Of the second column. The second receiving transformer is described in the second full paragraph of the second column of page 3. The third form of receiving transformer is described in the third full paragraph of the second column of page 3, while the fourth form of receiving [fol. 2110] transformer is described in the fourth full paragraph of page 3. Since the two last paragraphs very definitely state the third form and the fourth form, I am sure that the first paragraph describes the first form, the second paragraph the second form, while the third and fourth definitely state that they are the third and fourth, respectively. Does that answer your Honor's question?

The Commissioner: Yes.

Q. 86. I think you might also explain the reference in the tables on page 4 to the transformers; tie that in together.

A. The explanation which I have just given about the first four paragraphs of the second column on page 3 is further borne out by the tables of the receiving station on page 4 of the patent. It will be noted that the receiving station employs for tunes Nos. 1 and 2, No. 1 induction coil; for tunes 3 and 4, No. 2 induction coil; for tune No. 5, No. 3 induction coil; and tune No. 6 calls for the fourth induction coil. Therefore, these four induction coils which are the receiving transformers, the receiving transformers we are discussing, must be found within the specification of the patent; and I have already pointed out that 3 and 4 are identified definitely, and, since they follow the other two paragraphs, it seems only reasonable to conclude that the first paragraph fully describes the receiving the trans- [fol. 2111] former No. 1 and the second paragraph in question the receiving transformer No. 2.

Q. 87. Are those transformers in the text referred to as induction coils?

A. Yes; in the text they are referred to as induction coils.

Q. 88. What, if any, consideration did Professor Hazeltine give, when he was considering the operation of this Figure 2, to the secondary circuit?

A. Professor Hazeltine's testimony, as I heard it, definitely stated that the coupling between the primary and secondary was loose, and therefore he would entirely neglect the effect of the secondary, so I do not believe that he gave them careful consideration.

Q. 89. And, utilizing transformer No. 2 for tune 3, could the secondary circuit, in your opinion, be tuned to in the neighborhood of 200 meters?

A. No, it could not be tuned to 200 meters.

Q. 90. You spoke of the primary circuit, the antenna circuit, responding both to 200 meters and 1600 meters, approximately. Will you briefly explain how that can be?

A. A complex circuit in which inductance and capacity are connected in separate branches, such as shown in Figure 2, will have a resonant characteristic in which the antenna circuit as a whole, or the circuit including these inductances [fol. 2112] and capacities as a whole, responds to two different wavelengths. These two wavelengths correspond generally to the wavelengths of the circuits taken separ-

ately, but when they are coupled together the effect of the coupling is to move the wavelength which is lower to a still lower value, and the wavelength which is higher to a still higher value.

However, the network as a whole will respond to applied currents of the two frequencies, so that two distinct resonant points are present in the network considered as coupled together or as a whole.

Q. 91. Is either of those wavelengths the same as any separate portion of the circuit considered by itself?

A. Oh, no. They are always changed by the association with the associated circuit.

Q. 92. Now, you stated, I think, that, when two circuits, each having its own natural wavelength, are brought together and associated, the two wavelengths of the system—if there are two—would go in opposite directions, so to speak; that is, the modification of the wavelengths would go in opposite directions. In that correct?

A. That is correct.

Q. 93. Will you refer, please, to Defendant's Exhibit BBB, which is entitled "Marconi Antenna Circuit Tunes," which was put in through Dr. Wheeler, and state whether there is anything in there that appears to you to be incon- [fol. 2113] sistent with that operation of two associated circuits.

A. Yes. First of all, the figures in the right-hand column of Exhibit BBB, 1640 which refers to the wavelength of the circuit h j' alone indicate a longer wavelength than is obtained when the so-called wheel circuit h j' is associated with the antenna circuit, including the loading coil. The figures given for the associated circuit from 1628 to 1641 are not the figures which would be obtained if 1640 is correct. However, I have checked Dr. Wheeler's calculations and think that the error is not necessarily in the second column from the right, but, rather, is in the column to the extreme right. I feel that he made a pardonable error, so far as slide rule calculations are concerned, but an unpardonable error so far as knowledge of the operation of tuned circuits is concerned. He could have seen on mere inspection that there was something wrong and looked for the error. The actual wavelength constants which Dr. Wheeler calculated will be approximately 1590 meters. When using this resonant wavelength for the so-called wheel circuit

h j′ alone, the figures in the second column from the right on Exhibit BBB are substantially correct.

There is another method which should, perhaps, have occurred to Dr. Wheeler. In circuits of the type illustrated at Figure 2 of the Marconi patent, not only are the resulting [fol. 2114] wavelengths spread further apart, but there is a fundamental relationship, namely, the product of the two circuit wavelengths taken alone should equal the product of the wavelengths of the resulting combination. It would, therefore, only have been necessary to multiply two sets of two figures to determine the accuracy of the original figures.

Q. 94. Is this principle of the spreading of the wavelength when the circuits are brought together and this rule as to products set forth in the literature of the art?

A. The manner of the spreading of the wavelengths, due to the mutual coupling of two circuits, is set forth in the art in considerable detail. However, I do not know offhand of any textbook which shows this relationship. As a matter of fact, the relationship which I have given for the type of circuits illustrated at Figure 2 of the Marconi patent, probably does not apply when the two circuits are coupled solely by mutual inductance. In such case,—for example, I am thinking of the work of Dr. Pierce, set forth in his textbook, which was first published in 1910—he gives the relationship between the original wavelengths and the resulting wavelengths, but he does not give the fundamental relationship which from his formulas may be derived; and I think, upon derivation, we find that the multiplied wavelengths which result from the coupling must in turn be [fol. 2115] modified by a constant. This is because he was discussing only the case of mutual inductive coupling.

Q. 95. You have used and defendant's witnesses have used the term "wheel circuit." Will you explain what you mean by that?

A. By the term "wheel circuit" I mean, and I believe the defendant's witnesses also mean, a circuit including lumped inductance and capacity, and resonating at one specific frequency, such as h j′ of Figure 2 in the Marconi patent when taken alone.

Q. 96. Now, Commander Dow said that the condenser h in Figure 2 of the Marconi patent acts as a series condenser. Dr. Wheeler also said that the antenna circuit was

essentially a series circuit. Do you agree with those statements?

A. No, I do not agree. In so far as the antenna circuit is concerned, it is a complex circuit, but viewed from the resulting wavelengths in the secondary circuit, the antenna circuit is a shunt tuned circuit; that is, the condenser h shunting the coil j′ tunes to the wavelength of 1600 meters.

Q. 97. And in that respect is the operation of Figure 2 of the Marconi patent the same as that of the antenna circuits in defendant's receivers, where the parallel con- [fol. 2116] denser is connected across both the primary of the coupling coil and across the loading coil?

A. In so far as the defendant's receivers are concerned, the connections are the same—although I don't know if defendant's receivers were tuned to octaves.

Q. 98. In those defendant's receivers are the antenna circuits shunt-tuned, as you described Figure 2 of the Marconi patent?

A. Well, the defendant's receivers, in the case of Group No. I in the longer wavelength ranges, are shunt-tuned.

Q. 99. I am referring to where the parallel connection to the condenser is used.

A. Wherever the parallel connection is used in defendant's receivers, they are shunt-tuned.

Q. 100. Commander Dow, at page 1375 of the record, testified as follows:

"As explained, the condenser h of Figure 2 is used as a part of the resonant system, consisting of the antenna f′, the coil g′, the condenser h, and the earth. When this circuit is resonant to the incoming frequency, the maximum voltage exists across the condenser h.

"The Commissioner: You are now talking about the disclosure of the patent?

[fol. 2117] "The Witness: Oh, yes.

"A. (Continuing:) The maximum voltage exists across the condenser h, and a maximum of current flows through the coil j′. However, the current which flows through the coil j′ under these conditions is not nearly as great as would be the case where the condenser h and the coil j′ were adjusted so as to provide a circuit which was resonant to the received frequency or wavelength."

Do you agree with that statements?

A. No, I do not agree; and I disagree for at least two reasons. First, with a complex circuit such as Figure 2, I do not believe that we are justified in neglecting any element of the circuit, which is apparently what is done in the first part of the testimony you have quoted at page 1375. Further, considering that the coil j′ is connected as apparently he assumes in the second part of the quoted testimony, then the maximum current will flow when the circuit as a whole is tuned, rather than the wheel circuit taken alone. When the wheel circuit alone is tuned to the transmitter wavelength and the wheel circuit is then connected in the antenna circuit of Figure 2, the resultant frequency will be different and will not necessarily be the best tuning, and, in fact, we know that the best tuning will be when the circuit as a whole is tuned to the wave-length to be received or an octave thereof.

(Tr. 2656.)

[fol. 2118] Q. 105. Are you generally familiar with the circuits of the defendant's receivers that are involved in this accounting?

A. Yes.

Q. 106. I call your attention to Plaintiff's Exhibit 96, a copy of which is found in Volume I of the record on the main case opposite page 183. Referring particularly to the receivers made in 1917 and later, with type numbers, will you please compare the antenna circuits of those receivers with the antenna circuit of the receiver shown in Plaintiff's Exhibit 95. If you wish to refer to any particular receiver——

A. I would like to. Referring to defendant's receiver SE 143, SE 143, when operated with the shunt tuning condenser corresponds to Plaintiff's Exhibit 95, as follows: The antenna f corresponds to the antenna which would be connected to the receiver SE 143. The load coil g′ corresponds to one of the inductance sections in the primary circuit of the receiver SE 143. The coupling inductance j of Exhibit 95 corresponds to the coupling inductance of the primary of the receiver SE 143, while the shunt-tuning condenser h of Exhibit 95 corresponds to the shunt-tuning condenser of the primary circuit of the receiver SE 143.

This similarity which I have described with respect to SE 143 would correspond to a similar connection of all

of the receivers in Group III, and would correspond to the longer wavelength operation of the receivers in Groups I and II.

(Tr. 2657.)

[fol. 2119] Q. 107. And in defendant's receivers, when utilized with the parallel connection, is the antenna condenser connected across the same elements as in Plaintiff's Exhibit 95?

A. Yes. The connection in Plaintiff's Exhibit 95 shows the condenser h shunted across both the coupling coil j and the loading coil g'. This corresponds to the connection of the shunt condenser in the primary circuit of defendant's receivers.

Q. 108. If there are any differences between the antenna circuit of defendant's receivers utilizing the parallel connection and the circuit of Figure 2 of the Marconi patent, are those differences also present between Exhibit 95 and Figure 2 of the Marconi patent?

A. Yes; there is a difference in Figure 2 of the Marconi patent. The condenser h is shunted across the coupling coil j', but not across the loading coil g'.

Q. 109. And in that respect Figure 2 is different, not only from defendant's receivers with the parallel connection, but also from Plaintiff's Exhibit 95. Is that correct?

A. Yes; the same difference exists between Figure 2 and Exhibit 95 as exists between Figure 2 and the defendant's receivers.

Q. 110. What do you find in the Marconi patent as to whether or not Marconi contemplated omitting or reducing [fol. 2120] to zero coil g' in Figure 2?

A. In the table on page 4 which describes the specific arrangements of some of the receiving stations, the coil g' is omitted in tune No. 1, 45 turns of the coil g' are included in tune No. 2, while a variable number of turns is specified in tune No. 3. In tune No. 4, 100 turns of the coil g' are specified; whereas in tunes Nos. 5 and 6, no turns of the loading coil g' are specified.

Q. 111. What do you find in the patent as to whether or not the coil g' is to be located between the antenna f' and the coil j'?

A. Page 2, column 2, beginning line 86, specifies that the variable inductance g' is interposed in the primary cir-

cuit of the transformer and preferably located between the cylinder f′ and the coil j′.

Q. 112. If the coil g′ were omitted, or reduced to zero, then would the difference which you spoke of between the Figure 2 of the Marconi patent, and Plaintiff's Exhibit 95 or defendant's receivers with the parallel connection still exist?

A. No; there would be no practical distinction between Plaintiff's Exhibit 95 and Figure 2 of the Marconi patent or the defendant's receivers when used with the parallel shunt connection, because the coil g′ of Exhibit 95 may also be reduced to zero. I think perhaps that last state- [fol. 2121] ment isn't clear. The coil g′ of Exhibit 95 may also be reduced to zero.

* * * * * * *

(Tr. 2666.)

Q. 127. Referring to extending the wavelength range of the primary circuit of a given receiver utilizing a series tuning condenser, it has been testified, as I understand it—I think by Commander Dow—that you could obtain as long a wave range as you desired for that circuit by increasing either the inductance or the capacity, or both. Do you agree with that?

A. That statement has to be broken down. There are three statements, and I don't agree with all of it.

Q. 128. Will you state the situation?

A. The statement is that the operating range may be increased by increasing the series tuning capacity or the inductance, or both. First, let us consider increasing the tuning range by increasing the series capacity. I don't agree with that statement. There is a practical limit, which is a very definite limit, to the amount the range may be increased by increasing the series capacity. The limit of series capacity is infinity, or a short-circuit.

[fol. 2122] Q. 129. You mean infinity and short-circuit are the same thing, electrically?

A. They correspond, electrically. So that, if the capacity is made very large, we merely approach the capacity of the antenna, and it becomes a limit. After that, you can increase the series capacity 1,000 times or a million times, and it will have practically no effect.

Q. 130. You said that is a practical matter. Is it also true theoretically?

A. It is true theoretically. You can show by checking up these values that an increase of 1,000 times only means a very few meters theoretically; and after that it doesn't increase at all.

Q. 131. Before you take up the others, and, preliminarily, what is a reasonable value for the natural wavelength of an antenna as of the period we are considering?

A. During the period we are considering, the natural wavelength of antennas on battleships was approximately 365 meters.

Q. 132. I will ask you to assume that there is inserted in that antenna sufficient inductance, which, without any capacity, will bring the wavelength up to 1,000 meters. How much, if at all, above 1,000 meters could you tune that antenna to by utilizing a series condenser?

[fol. 2123] A. You never can tune the antenna to wavelengths longer than 1,000 meters by inserting series capacity. The series capacity reduces the natural wavelength of the antenna and does not increase it. Probably, one of the main reasons why series capacity is used, is to enable the antenna to respond to wavelengths less than, for example, 365 meters.

Q. 133. I think you wanted to complete your answer to the original question.

A. The second part of this apparently simple statement is that the antenna's natural period can be increased by increasing the inductance—it is possible to increase the natural period by increasing the inductance. However, one should not forget that the resistance of a loading inductor increases more rapidly than the reactance, which would lead to an intolerable situation, because the resistance of the antenna circuit would increase rapidly and the resultant selectivity and sensitivity would be less than would be obtained if we didn't put in so much inductance that the resistance became very much greater than it should be. So that I can only agree in part with this portion of Commander Dow's statement.

With respect to the third possibility, that we increase both series capacity and series inductance, we run into the difficulty that the minimum of the variable capacitor or variable condenser increases as we attempt to make the [fol. 2124] maximum larger. He pointed out that, by very, very careful design, it is possible to keep the minimum

fairly low. I don't think that would be a practical matter. I think this is more of a theory. If we retain the series condenser, no matter how large its maximum may be, then we must add still more loading inductance than if we did away with the series condenser. So this makes the situation even worse, because you are adding one thing to make the wavelength greater, and another thing that takes it away. So this is one of those "lift yourself up by your own bootstraps" propositions.

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(Tr. 2671.)

Q. 134. Mr. Tuska, will you explain what is meant by the terms "series circuit" and "parallel circuit"?

A. As the terms are used in the radio art, and are applied to circuits, including capacity and inductance, the meanings are as follows: first, a "series circuit" means a circuit in which the generator, the capacity and the inductance, are serially connected. Second, the "parallel circuit" means a circuit in which the generator, the inductance and the capacity, are all connected in shunt. Any circuit, including only capacity and inductance, may be viewed as a series circuit or a parallel circuit provided we specify where the generator shall be located.

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(Tr. 2672.)

[fol. 2125] Q. 137. Commander Dow, in his testimony, arrived at the conclusion that, to increase the wavelength of an antenna circuit in a receiver, it was better to add a loading inductance than to use a parallel condenser. Do you agree with that?

A. No, I do not agree; because a loading inductance increases the cost, size, and weight of the receiver, and it is not—it does not possess the electrical advantages of the parallel condenser when used for tuning to the long wavelengths.

Q. 138. You heard Commander Dow testify, I think?

A. No, I did not hear him.

Q. 139. Have you studied his testimony?

A. I have read his testimony and studied it.

Q. 140. Will you state, as you recollect them, the reasons advanced by Commander Dow for the conclusion that I stated in the preceding question, and state whether you

consider those reasons to be correct; and, if not, why not?

A. I think that Commander Dow had two principal reasons. The first reason, he said, was the matching of the antenna impedance to the impedance of the tuned circuit, such as h j' in Figure 2 of the Marconi patent. The second reason which he gave was that the loss of resistance [fol. 2126] in a condenser used in series was less important to the operation of the receiver then than in the case of the condenser not being there.

As to the first reason, the matching of impedances, the Commander was evidently confused by modern art, in which a transmission line is connected between an antenna and a tuned circuit. In such installations, it is desirable to match the impedance of the line to the terminating networks, so that the line acts as a transmission line and not as a tuned or tunable circuit. If the transmission line is not properly terminated, standing waves appear, and the purpose of the line, or the utility, is defeated. This action of the transmission line is entirely different—an entirely different affair from the connection of an antenna to a circuit which includes the antenna and resonates as a whole. If the antenna is viewed in the normal and proper manner, it becomes a part of the tuned circuit, and the impedance of the tuned circuit as a whole is just the same, whether the antenna is connected in series or in parallel. The resulting impedance is the same in both cases. I think the difficulty that Commander Dow may have had, was his failure to view the equivalent circuit to the antenna which is the capacity of the antenna with the generator in series.

[fol. 2127] Under these circumstances, the generator itself never shunts the tuned circuit, but is always in series with one of the elements.

As to the second ground, as I understood his reason, the resistive losses in the series condenser are present, whether you use loading coils or not, so we can dismiss this matter. However, we should not dismiss the resistance of the loading coil, which increases more rapidly as the loading coil is increased than the reactance of the coil.

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(Tr. 2676.)

Q. 147. To summarize: does the problem of impedance matching arise in the antenna circuit of the radio receiver

such as is shown in Figure 2 of the Marconi patent, or such as is utilized in the defendant's receivers, either with a series condenser or with a parallel condenser?

A. No. It is not a problem to be considered. It does not arise.

Q. 148. And is there ever any effort made to match impedances?

A. Under these circumstances?

Q. 149. Under those circumstances, yes.

A. No.

* * * * * * *

[fol. 2128] (Tr. 2681.)

Q. 157. Mr. Tuska, speaking comparatively briefly, what does this Pupin patent teach as of its date, to one skilled in the art?

A. Pupin Patent No. 640516 teaches how two or more audi-frequency line telegraph transmitters may be connected to two or more selective line telegraph receivers. The patent clearly relates to a low frequency telegraph system, which is connected to a transmission line.

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(Tr. 2683.)

Q. 159. Mr. Tuska, do you find anything which is utilized in any of the defendant's receivers which are asserted to infringe claim 16 of the Marconi patent, and which is disclosed in the Pupin patent and not disclosed in the Marconi patent? I might add to that——

A. The answer to your question is No.

Q. 160. I might add to that—confine your answer to the radio frequency portion of the defendant's receivers.

A. The answer is No.

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(Tr. 2683.)

[fol. 2129] Q. 162. Have you read and are you familiar with the Fessenden patent 706735, which is Defendant's Exhibit LL in this case?

A. Yes; I have read the patent in question and I am familiar with it.

Q. 163. Have you also read Defendant's Exhibit CCCC, a copy of the application, as filed, for this Fessenden patent?

A. Yes, I have read Defendant's Exhibit CCCC, which is a certified copy of the Fessenden application, as filed.

Q. 164. Will you briefly explain the disclosure of the Fessenden application, as filed, Defendant's Exhibit CCCC?

A. The Fessenden application, as filed, Defendant's Exhibit CCCC, discloses a transmitter which includes an antenna conductor 5, a capacity element 5-a, a serially connected spark gap 4, 4, which is shunted by a capacitor 18, and an induction coil, which is connected through the key to a power source and through its secondary to a spark gap which I have designated. This transmitter is an open antenna transmitter, and it radiates waves which are received by a receiving station. The receiving station includes an antenna conductor 6, which is serially connected through field coils 7 to the earth. Within field coils 7 is included a coil 8 which acts as a galvanometer to indicate visually the reception of signals. Condenser 19, which [fol. 2130] shunts the field coils, may be used. In some of the other figures of the patent, variations of this arrangement are shown. In one such variation, Figure 2, the antenna circuit is connected through an induction coil to the field coils of the galvanometer. The induction coil consists of an antenna winding, 12, and secondary winding, 11.

A further modification is shown in Figures 3 and 4, in which the coil 8 is arranged to contact variably a resistor, 14, which is included in a local telephone circuit 15-16. A still further modification is shown in Figure 5. In this arrangement, the field coils are indicated as 16 and 16-a, and these coils are differentially connected.

Q. 165. With respect to Figure 1, you used the phrase "open antenna transmitter." What do you mean by that?

A. I meant a transmitter in which the spark gap was serially connected in the antenna circuit, and in which no tuning was used.

Q. 166. According to the application, as filed, what purported to be the new disclosure therein as to subject matter?

A. The disclosure was that of a receiving device in which the operation depended upon the reaction of coils in one element upon a second element. This meant the reaction of the coils in the field coils 7 upon the galvanometer 8.

[fol. 2131] Q. 167. Commander Dow referred specifically to the condensers 18 and 19 in Figure 1. What do you find

in the application, as filed, describing those condensers or their function?

A. In the last paragraph of the specification, page 5, lines 23 to 26, it is stated that "a condenser 18 may be connected in series or in shunt with the sending or generating wire or wires 5 and a condenser as 19 may be connected in series but preferably in shunt with the field coil or coils 7."

Q. 168. Is there anything else in the application as filed with respect to those two condensers?

A. Yes; claim 4 on page 6.

Q. 169. Disregarding the claims; in the specification, is there anything additional?

A. That is all I find.

Q. 170. What is the reference in claim 4?

A. The latter part of claim 4 specifies, "A condenser connected in series or shunt with said field."

Q. 171. What field is referred to there?

A. The antecedent is a little ambiguous, but he means the field coil or coils.

Q. 172. Commander Dow, at pages 1640 to 1642, and again at page 1647 of the record, pointed out where he found in the Fessenden patent, Defendant's Exhibit LL, a teaching [fol. 2132] that Fessenden's antenna circuits were tuned. Do you find the same or any similar or equivalent statements in the Fessenden application, as filed, Defendant's Exhibit CCCC?

A. The first reference to Commander Dow's testimony, on page 1640, refers to the Fessenden patent, and quotes line 101 on page 2. I do not find the same statement in the Fessenden application, as filed.

Q. 173. Or any similar or equivalent statement?

A. The first part of the statement which says "A condenser 19 may be connected in shunt with the field coil or coils" is included; but the entire meaning of the sentence is changed when he adds that "For the purpose of obtaining as large a current as possible in the field coil 7, as this increase in current will give a greater torque to the ring h." There is no such thought in the original filed application.

On page 1641, Commander Dow states that the Fessenden specification, beginning line 106 of the patent, includes a rather long explanation showing the results which he (Fessenden) got. The statement of Commander Dow goes on

to refer to the antenna current in the Fessenden device, being larger than the current in the receiving conductor. There is no corresponding statement in the Fessenden application, as filed.

[fol. 2133] Q. 174. You refer to line 106. On what page in the specification is that?

A. Commander Dow did not give the number, but I will. He apparently had in mind page 2, second column, lines 106 and so forth. (Witness examines paper.) Mr. Blackmar, will you please point to the reference you had in mind on page 1647?

Q. 175. I was referring to XQ. 771 and the answer.

A. This question XQ. 771 says, in effect, that the condenser 19 and the inductance 7 are stated by Fessenden to be made resonant to the incoming frequency, and it refers to the same place in the specifications to which the witness, Commander Dow, has previously referred. I do not find any statement in the Fessenden application, as filed, about tuning the "circuit, consisting of the condenser 19 and the inductance 7, so as to make them resonant to the incoming frequency."

Q. 176. What, if anything, is there in the radio frequency circuits of the defendant's receivers that are asserted to infringe claim 16 of the Marconi patent which is present in the disclosure of the Fessenden patent that is not present in the disclosure of the Marconi patent?

A. Nothing.

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(R. 2718.)

[fol. 2134] The Commissioner: Mr. Tuska, during the mid morning recess, I will ask you to prepare as an exhibit a sketch illustration of the testimony which you have given, relative to what you have termed the parallel connection and what you have termed the series connection of the tuning condenser. These sketches can be merely diagrammatic in character, and for comparative purposes; and I am not interested in having the details, we will say, of the secondary circuit, such as the detector or receiver. In other words, I think the sketches should be limited to the radio circuits.

The Witness: Yes, sir.

Mr. Blackmar: Would it be helpful to add a third sketch, showing a loading coil in a series circuit?

The Commissioner: Yes. I think that might additionally [fol. 2135] illustrate the testimony of the witness.

Mr. Blackmar: As I understand it, these are antenna circuits?

The Commissioner: Antenna circuits.

* * * * * * *

(Tr. 2720)

By Mr. Blackmar:

Q. 249. Mr. Tuska, have you prepared the sketch that the Commissioner requested?

A. Yes.

Q. 250. I notice that in the left-hand or series connection and the loading coil sketch the primary tuning condenser is below the inductances, whereas in the right-hand the series connection sketch, it is above. Is there any particular reason for that?

A. No. It could be in either place.

Mr. Blackmar: I ask that the sketch be marked Plaintiff's Exhibit 477 in evidence.

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(Tr. 2721)

By the Commissioner:

Q. 253. Let me ask this: in conventional sets at this time where was the condenser actually connected?

A. In general, it was between the coupling coil and the ground.

* * * * * * *

[fol. 2136] Cross-examination. (Tr. 2722)

By Mr. Houghton:

X Q. 255. Mr. Tuska, referring to Plaintiff's Exhibit 477, the sketches just prepared by you, the middle figure, as I understand it, is drawn to show the shunt primary tuning condenser?

A. Yes.

X Q. 256. Then, is not the word "or" in your legend there surplusage?

A. The reason I put "or" there is because I have used both terms in my testimony, and I understand this was an illustration of my testimony. Sometimes, I have called it the primary condenser of the antenna circuit; sometimes I have just called it the primary tuning condenser, and sometimes just the shunt condenser?

X Q. 257. And the same reason accounts for the "or" in the right-hand sketch on the exhibit?

A. Yes.

X Q. 258. When you were testifying regarding Figure 2 of the Marconi patent and tune No. 3 thereof, it was not clear to me just which circuits or which component subcircuits you identified as responding to the several wavelengths that you mentioned. If I understood correctly, you testified that the transmitter of tune 3, transmitted on approximately 200 meters.

A. That is correct.

[fol. 2137] X Q. 259. What parts of the primary circuit of Figure 2 resonated to 1600 meters approximately and to 200 meters approximately?

A. Referring to Figure 2 of the Marconi patent, it will be seen that the antenna circuit is a complex circuit Because it is a complex circuit, it responds to two wavelengths. One is 200 meters, and the other is 1600 meters. I cannot pick out any elements and say this element is responsible. I have to take all of the elements into consideration. If you wish to know what branch of the complex circuit is responsive to a wave slightly above 200 meters, and which branch is responsive to a wave slightly below 1600 meters when the two circuits are disconnected, the two branches are disconnected, I can answer that.

X Q. 260. Please do.

A. The element which, if disconnected, responds to a wavelength slightly greater than 200 meters in Figure 2 of Marconi patent are the antenna f', connecting wire A, loading coil g' and the earth connection E. The elements which are responsive, when they are disconnected, to a wavelength slightly less than 1600 meters are the condenser n and the coil j' connected to the earth E.

X Q. 261. With respect to the circuit you have just identified as responding to 200 or slightly above 200 meters, you identified only an incomplete course from the antenna [fol. 2138] to the ground. Did you omit an element in that circuit that responds to slightly over 200 meters?

A. I thought I said that g′ was connected to the ground. If I didn't, I should have.

X Q. 262. And in that circuit resonating to slightly above 200 meters, approximately what number of turns of the load coil g′ are considered as in the circuit?

A. Approximately 21 turns.

X Q. 263. Suppose you consider the series circuit comprising antenna f′, 21 turns of the load coil g′, the condenser h, with the value given for tune three, connected to earth. In connection with that circuit, did you check the wavelength given in the third column of defendant's Exhibit BBB?

A. Do you mean whether I checked the mathematics to see whether any error had been made in the calculation?

X Q. 264. That is correct.

A. So far as I know, Dr. Wheeler's calculation was correct.

X Q. 265. And he shows there, in the third column, in the line identifying coil g′ as having 21 turns, that the wavelength of that series circuit f′, g′, h and E is approximately 200 meters, is it not?

A. That is correct.

[fol. 2139] X Q. 266. Do you mean to indicate that it would make virtually no difference in the wavelength whether the antenna and 21 coils of g′ were connected directly to the earth or connected to earth through the value of the condenser identified for tune 3?

A. Yes; I assume when you say it is essentially the same, it isn't a big change. It would be practically the same. The reason for it is that the condenser is so large that it can be neglected. When one has a very large condenser connected in series with a very small condenser, you can neglect the larger one.

X Q. 267. Did you further testify that the over-all frequency or one of the over-all frequencies to which the primary circuit of figure 2, tune 3, responds, is also nearly 200 meters?

A. I do not think I used the term "over-all"; but I did testify that one of the resulting frequencies upon coupling the two branches was a response of about 200 meters.

X Q. 268. Then it is your testimony that it makes practically no difference in the tune of the primary circuit to include the large condenser h in series or to omit it; and

also that it makes practically no difference in the tune when the condenser h is present to include the coil j'?

A. That is correct. The effect of the circuit which we have called the wheel circuit, h j' on the 200-meter response, is very slight. There is an effect, but it isn't great.

[fol. 2140] X Q. 269. Then the principal element in establishing the 200-meter tune of the entire primary circuit of Figure 2, tune 3, is what element?

A. I think I have already testified that you can't pick one element out of this complex circuit, and say that that element is responsible for any particular tune. You must view the thing as a whole, as it is, and I don't know any way that I could put the blame on any one element for what happens.

X Q. 270. It is your testimony, is it not, that the antenna and the coil g' connected directly to earth will resonate at substantially the same frequency as the entire primary circuit shown in Figure 2. Is that correct?

A. No, that is not correct. I testified—and I tried to be very clear about it—that the antenna circuit as a whole in tune 3 responds to two frequencies, or two wavelengths, one of which is about 200 meters, and the other is 1600; and you cannot separate them. They are there together.

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(Tr. 2727)

[fol. 2141] X Q. 273. Assume that the antenna f' is connected through 21 turns of the load coil g' and the other end of the load coil g' is connected directly to earth—that is, assume that the circuit h j' is entirely omitted. Is it your testimony that the antenna load coil to earth circuit would resonate to substantially 200 meters?

A. I have not testified to that. Accepting the assumption, it does respond to approximately 200 meters.

X Q. 274. Referring further to your testimony regarding the Marconi patent, Figure 2, tune No. 3, did I understand correctly that the secondary j_2, the secondary circuit including j_2, according to your calculations and observation, resonates to 1600 meters, approximately?

A. You haven't quite phrased what you thought my testimony was. I think you will find I said that the coils j_2—there are two of them—and the coils g_2—there are two of them, plus the distributive capacities of the circuit, bracket the wavelength of about 1600; and by that I meant that by

varying the coils g_2, you could tune the secondary circuit to 1600 meters.

X Q. 275. Did you further testify that 1600 meters is an octave of the 200 meter fundamental; that is, it is eight times 200?

A. Yes.

[fol. 2142] X Q. 276. By "octave" do you mean an harmonic?

A. No. The terms, even today, are confused in the art; but I did not mean an harmonic.

By the Commissioner:

X Q. 277. An octave would be double, would it not?

A. As I understand it, the "octave" comes from the word "eight,"—numbers bearing a relation to eight.

X Q. 278. Then you use the term "octave" in a different sense from the musical scale?

A. Yes. We didn't include the term "eight." It just happens that eight comes in twice; that is just a mere incident. By "octave" I meant a certain integral number of times the wavelength.

By Mr. Houghton:

X Q. 279. An integral number of times the wavelengths?

A. An integral number of times the wavelength.

X Q. 280. Was it your testimony that in tune 3, the circuit of Figure 2, receiving a signal at the transmitting frequency of 200 meters would excite the secondary tune to resonate to this octave or 1600 meters?

A. Yes.

By the Commissioner:

X Q. 281. As we use these terms today, is not the term "harmonic" appropriate?

[fol. 2143] A. Yes, we use it today; but I do not think "harmonic" is the term to use, although there is some confusion on that because we get into the problem of whether you mean multiples of or fractions of—and then we have to begin to talk about sub-harmonics; and there is another trouble, what you are going to call the fundamental. Some people call it one thing, some another.

X Q. 282. In this case, 200 meters was the fundamental?

A. And 1600 meters is eight times 200, which is an integral number——

X Q. 283. Would it be appropriate to term that the "harmonic"?

A. There is a lot of question whether you would call it the eighth or third or some other number. It is eight. If we say eight times, we avoid confusion.

By Mr. Houghton:

X Q. 284. You also testified that the secondary in Figure 2, tune 3, could not be tuned to 200 meters with the constants given in the table for the values of j_2 and g_2, I believe?

A. The fundamental response of the secondary circuit with the constants give will be 1600 meters. I don't think I testified that it couldn't be tuned to 200, or it might not respond to 200, but that, with the constants given, it will be 1600 meters.

[fol. 2144] X Q. 285. You have just referred to "harmonies". What is your definition of an harmonic with reference to a fundamental?

A. I should say that an harmonic is an integral number of times the frequency of the fundamental.

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(Tr. 2731)

X Q. 287. What is the frequency corresponding to 200 meters?

A. 200 meters is 1500 kilocycles.

* * * * * * *

X Q. 289. What is the frequency corresponding to a wave length of 1600 meters?

A. That is approximately 188 kilocycles. There is a fraction. I don't recall what it would be.

X Q. 290. Regarding the 1500 kilocycles corresponding to the 200-meter wavelength as the fundamental, could that give an harmonic of 188 kilocycles corresponding to 1600 meters?

A. Not as I have defined "harmonic." As I have defined "harmonic."—I only went to higher frequency, and, as I said to the Commissioner, with that definition you also have to bring in sub-harmonics, so that we would have in this case

a sub-harmonic; or, if you prefer to start with 1600 meters, you can go the other way and call it the harmonic.

X Q. 291. I believe the Commissioner's question established that you were regarding 200 meters as the fundamen- [fol. 2145] tal. If you had a receiver receiving this fundamental and receiving the quantity of current which a normal receiver would obtain on a received signal strength could that primary circuit of the receiver excite a secondary tuned to 1600 meters?

A. In the time of Marconi and the circuits we are discussing, yes, it would. The reason is that the waves are complex. They are not pure waves. They are highly damped, and they include not only the fundamental frequency but many other frequencies—so that you get into a situation which cannot be compared to the conditions of today, where we have pure waves or continuous waves.

X Q. 292. Then it is your testimony that, with a transmitted wave of 200 meters received by the circuit of Marconi Figure 2, tune 3, the secondary circuit of that figure tuned to 1600 meters would respond to the received signal; is that correct?

A. I have not testified quite that way. I have said that the antenna circuit as a whole is responsive to two frequencies; and one of these frequencies is 200 meters and the other is 1600 meters; that the coupling between the antenna circuit and the secondary is the coupling between two circuits which are responsive to 1600 meters; and that combination, the thing we are dealing with here, will respond to 1600. It will also have some response, due to the impure waves, to 200 meters, other than this direct [fol. 2146] 1600-meter response; but the antenna responds equally well to the wavelengths, and the secondary circuit responds to the 1600.

X Q. 293. But the transmitted wavelength on tune 3 is 200 meters?

A. That is correct.

By the Commissioner:

X Q. 294. Would it be proper to say that the 1600 meters is an even harmonic of the fundamental 200 meters?

A. Not as I have defined "harmonic." I started with the fundamental frequency, and I defined "harmonic" as the integral number of higher frequencies, which I think is the

way we use the word today; so that, under these circumstances, it would not be correct.

X Q. 295. As I understand your testimony, you have your fundamental wavelength of 200 meters coming in on your primary circuit, and your secondary circuit is tuned to 1600 meters. Now, what is the particular relationship between the two? I know that one is numerically eight times the value of the other. Does that combination operate better than if the secondary circuit was tuned with, say, 1400 meters or 1300 meters? Is there some particular relationship caused——

A. Yes; it must be multiplied by a whole number, such as [fol. 2147] 2, 3, 4, 5, 6, 7, 8, 9, and so on; and that is what we mean.

By Mr. Houghton:

X Q. 296. When you say "it must be multiplied," what do you mean—the frequency?

A. The fundamental wavelength, if we start with the fundamental wavelength of 200 meters in this particular arrangement.

By the Commissioner:

X Q. 297. Then would this be true: that the 200-meter wavelength—with the 200-meter wavelength coming in on the primary, suppose we could adjust the secondary through a range of frequencies, would we get a response if the secondary were adjusted at 200 and then, as we increased the wavelength of the secondary down to, say, 300, we would get a decrease of response, and if we came up to 400 we would get an increase?

A. That is correct.

X Q. 298. And that would be true when we went to 500? What would we get—a decrease?

A. 500 would be a decrease.

X Q. 299. How about 600?

A. 600 would be an increase.

X Q. 300. So, then, as we take each multiple, even an even multiple of the fundamental, we get an increase?

A. That is correct.

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[fol. 2148] (Tr. 2745)

Redirect examination.

By Mr. Blackmar:

R. D. Q. 326. Referring to the sketch that you made at the Commissioner's request, just to be sure what it means, is it intended to be representative in detail either of the circuits of the Marconi patent or of the circuits of the defendant's receivers?

A. No, it is not intended to represent details of these circuits.

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(Tr. 2746)

R. D. Q. 327. Is it merely illustrative of more or less conventional circuits?

A. It is a schematic illustration of conventional circuits to show the difference between the parallel connected condenser and a series connected condenser and a series connected condenser with a loading coil in the antenna circuit.

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(Tr. 2747)

[fol. 2149] R. D. Q. 333. Using the analogy of music, or, more particularly, of the piano, if 200 meters is analogous to two octaves above the middle C, using octaves in the ordinary sense of the piano, is it correct to say that 1600 meters is equivalent to two octaves below middle C?

A. Yes.

R. D. Q. 334. That is, in the piano scale, as you go upward or downward an octave, you double or halve the frequency?

A. Yes, that is my understanding.

R. D. Q. 335. Or halve or double the wavelength?

A. Yes.

* * * * * * *

(Tr. 2748)

R. D. Q. 338. Considering the Marconi patent as a whole, does it, in your opinion, as one skilled in the art, teach that the only ways in which the condenser h of Figure 2 would be utilized, are those set forth in tunes 3 and 4 of page 4 of the patent?

A. No, it certainly is not my understanding that the condenser h is limited to the uses corresponding to tunes 3 and 4 of the tables on page 4 of the Marconi patent. Might I specifically refer to the paragraph which I recently read on the second column on page 2, at the bottom of the column, [fol. 2150] which specifically describes the tuning of the four circuits, all to the same wavelength or natural period? It is quite clear, from the teaching of the patent, that the receiver for tune 3 of the transmitter could be tuned to the same wavelength, rather than an octave. I think that the octave tuning is more unusual than the tuning to the fundamental. I might add that it has some utility to tune to octaves, and that use can be made of this feature. But the principal teaching of the patent is certainly that all four circuits are tuned to the same wavelength.

R. D. Q. 339. You mean primary teaching of the patent; is that what you mean?

A. Yes.

R. D. Q. 340. Because it also discloses the octave tuning?

A. Yes.

R. D. Q. 341. If the arrangement of the receiver of tune 3 was to be tuned both in primary and secondary to the transmitter, would the inductance coil or transformer, No. 2, be used?

A. No. You would select another of the transformers; or, in accordance with the specific instructions of the patent—I will read from the last paragraph, second column, of page 3: "Other forms of transformers which may be employed by me are described and claimed in my British Patent No. 7777 of 1900."

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[fol. 2151] (Tr. 2783)

Mr. Blackmar: If your Honor please, Mr. Tuska has supplied a formal drawing of Plaintiff's Exhibit 477, and I have his drawing and request that it be marked Plaintiff's Exhibit 477-A in accordance with the understanding on the record.

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[fol. 2152] George H. Clark

(Tr. 2811)

A. Within the years mentioned in the records, namely, 1918 and 1919, I have only two records. I have previous

records to a great extent when such work was being done in greater quantity. These two records are, first, a log of station IDO, Rome, and in my log book 3/29/18 I placed the notation that the wavelength of this station was 11,000 meters. The second entry is station LCM, Stavanger, under date 2/18/18. I have in my log book a record that it was received at 10,300 meters on the secondary dial of the receiver.

Q. 275. Now, you have, I believe, also, records—possibly in other years—for the German stations at Nauen and Eilvese. Is that right?

A. I have a record for Nauen only. I never measured Eilvese, though I copied it often.

Q. 276. What was the date of the German record?

A. 11/8/17.

Q. 277. November 8, 1917?

A. Yes, sir.

Q. 278. What were the call letters and what was the wavelength of that station?

A. The call letters were POZ, and my record gives a value of 12,500 meters for the station.

Q. 279. What other stations have you there for the year 1917——

[fol. 2153] Mr. Edwards: Are those in his records, you mean? Do you mean that is in his record?

Q. (279, continued). Yes, I mean those in your records, and of which you have a memorandum before you.

A. I have a record of Station MUU, Carnavon; and as of date 11/5/17 I recorded the wavelength as 14,000 meters.

Q. 280. Do you know where Carnavon is located?

A. Carnavon is in Wales. I also have a record——

Q. 281. That, I assume, is by hearsay, is that right?

A. I never was at the station. It is from reading and hearsay and official lists of the Navy.

Q. 282. Will you continue, please?

A. I have also copied station YN, Lyons, France. That was recorded by me under date of 12/5/17 as sending on a wavelength of 15,000 meters. Those are all that I have for that year.

* * * * * * *

(Tr. 2908)

Q. 2. Have you recently at my request considered the various receivers purchased by the defendant herein and asserted by the plaintiff to utilize the subject matter of Claim 16 of Marconi Patent No. 763,772 for the purpose of estimating the monetary saving that resulted in connection with these receivers from the use of the subject matter of that claim?

A. I have.

Q. 3. Generally speaking, was there any direct monetary saving resulting to the defendant from the use of the subject matter of that claim?

A. There was.

Q. 4. What was the cause of that monetary saving?

A. If the defendant had not used the parallel connection of the antenna tuning condenser in the primary circuit it would have been necessary to use additional inductance in the antenna circuit.

Q. 5. In order to do what?

A. In order to cover the wavelength range of the secondary circuit as given in the instruction books or testimony here.

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(Tr. 2952)

[fol. 2155] Q. 109. In arriving at that opinion, Mr. Tuska, what advantages did you consider as resulting from the use of the parallel connection?

A. I considered a number of advantages:

(1) At the longer wavelengths within the operating range the sensitivity is greater with the parallel connection;

(2) At the longer operating wavelengths the selectivity is greater;

(3) If no additional inductance coils are required the receiver may be made more compact with the parallel connection;

(4) If no additional loading coils are required the receiver with the parallel connection will weigh less than a receiver using the series connection with additional loading coils;

(5) The original design of a receiver with parallel tuning in the antenna circuit is less complex and less expensive than a receiver employing the series connection:

By the Commissioner:

Q. 110. That is the monetary advantage that you have already covered, is it not?

A. I think it is the design, not the cost of the receiver after it is built; but the initial receiver has to be designed.

[fol. 2156] By Mr. Blackmar:

Q. 111. Referring to development cost?

A. The development cost of the first model of the receiver. Then there are a number of operating advantages. For example, the range is greater with the parallel connection at the longer wavelengths. The interference problem is diminished at the longer wavelengths with the parallel connection.

Q. 112. There are others?

A. The question of material certainly is a factor which I had to consider, since these receivers involved war operations. With the parallel connection no loading coils would be supplied and this would mean a diminished problem of service. There would be less parts to keep in stock, less bases to be equipped with additional devices. There would also be less possibility of the receiver being damaged if it had less parts. There would be less possibility of the receiver requiring repairs with a smaller number of parts.

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[fol. 2157] ROBERT A. LAVENDER

(Tr. 1699)

ROBERT A. LAVENDER, a witness produced on behalf of the defendant, having been first duly sworn by said Commissioner, was examined, and, in answer to interrogatories, testified as follows:

Q. 1. Will you state your occupation?

A. I am an officer of the United States Navy in retired status.

Q. 2. Will you state your education, training, and the various positions you have occupied, having in mind par-

ticularly knowledge of radio transmission and reception and also in particular the use or operation of such instruments in the navy and, further, the value of licenses under patents for the use of such apparatus?

A. I graduated from the United States Naval Academy in 1912, in which or in the course of which instruction was a fundamental course in electricity and radio engineering.

* * * * * * *

(Tr. 3010)

Examining the wavelength formula, it is seen that the wavelength is equal to a constant (used by me as 1884) multiplied by the square root of the product of the inductance and the capacity.

In other words, for a definite wavelength you may select a certain value of inductance; that will determine the capacity that is necessary for the La value combined, and by [fol. 2158] multiplication will give you the value determining the other side of the equation, namely, the wavelength; so that if you double the capacity of an antenna, you immediately halve the value of the inductance that is necessary to have that particular wavelength.

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[fol. 2159] Clarence Denton Tuska

(R. 3272)

Before referring to Plaintiff's Exhibit 480-A for Identification, I should like to refer briefly to the Bureau of Standards book, circular C 74, beginning on page 66, and extending through page 68. This section describes "Effects of Dead Ends."

All inductance coils used in radio frequency circuits have not only the property of inductance but also have capacity as well. Whenever the amount of capacity of a coil is material, it cannot be neglected in determining the characteristics of the coil and the circuit in which the coil is used.

In this section the Bureau of Standards book points out that the turns which are substantially "dead," are, in effect, far from "dead," but are an important element of the circuit. By way of example, the book describes a wave-meter circuit in which the inductance coil is tapped. Fig. 51 illustrates such a circuit with four taps. The coil La which is

represented as a turn or two, corresponds to the coupling coil in the SE 950. The condenser C_1 corresponds to the algebraic sum of the antenna capacity and the tuning capacity. The coil marked large L, corresponds to the new coil on Exhibit MMMM. The turns in use are represented as small l, while the overhanging or unused turns are [fol. 2160] represented as l'. The distributed capacity of the unused turns is represented as C_o. Every element of Figure 51 will be found in the modification of the SE 950 proposed by Commander Lavender.

On the next page, the tuning characteristic of this wave-meter is shown in Figure 53. Actually, the characteristic should be a single line extending from a point near the zero of the wave-meter condenser upwardly to the maximum of 180. Instead of finding a single line, the measured characteristic of this wave-meter showed that the wavelength increased gradually from 100 meters to approximately 220 meters, at which point the curve breaks abruptly, and begins again at about 260 meters at 10 on the condenser scale; thereafter rising until the maximum range of 500 meters is reached at 180 on the scale. One who wished to use this wave-meter would not be able to receive any wavelengths between approximately 220 and 260 meters. In my previous testimony I referred to this condition as a hiatus in the tuning characteristic.

By the Commissioner:

Q. 240. Was that the same thing that Mr. Clark testified to this morning as a "trap circuit"?

A. I think it is, due to a different reason. I think the trap circuit has a slightly different characteristic, but it still has the property of two circuits which are coupled, and which have different wavelength characteristics. They may have the same wavelength characteristics.

[fols. 2161-2162] Q. 241. And they both involve distributed capacity?

A. And they both involve distributed capacity, but not quite in the same manner. It may be the same.

Mr. Blackmar: I notice that this happens to be a comparatively recent edition of the Bureau of Standards circular. May I borrow the circular which is the earlier edition, for a minute? While I do not think it makes any difference when it was published, because the only thing we are inter-

ested in is how the receiver would have worked with such a coil, yet Commander Lavender has been kind enough to lend me the 1918 edition of such a circular, and I should like Mr. Tuska to state whether similar subject matter appears therein.

A. (Continuing:) Yes, the same subject matter appears on both pages; on pages 66, 67, and 68.

Mr. Blackmar: I will offer in evidence as Plaintiff's Exhibit 481 and 1918 Edition of the Bureau of Standards Circular, so that it may appear what the witness was talking about, pages 66, 67, and 68 of the 1918 Edition of the Bureau of Standards Circular, and I request permission to put in photostatic copies of those pages.

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[fol. 2163] In Supreme Court of the United States

Stipulation Dispensing With the Printing of Exhibits on Application for Writ of Certiorari

It Is Hereby Stipulated and Agreed by and between counsel for petitioner and respondent herein, subject to the approval of the Court, that, on the application for writ of certiorari herein, only those portions of the record herein comprising the pleadings, the testimony, and the findings of fact, conclusions of law, orders, judgment, and opinions of the Court of Claims of the United States herein as set forth at pages 1 to 169, inclusive, of the Transcript of the pleadings, etc., herein, and at pages 1 to 1599, inclusive, [fol. 2164] and pages 2833 to 3196, inclusive, of the "Parts of the Evidence Agreed to by Parties as Material to Errors Assigned", need be printed, said pages 1 to 1599, inclusive, comprising Volumes I and II thereof, and said pages 2833 to 3196, inclusive, comprising Volume VI thereof; and that the Exhibits of the parties comprising those referred to in the Findings of Fact of the Court of Claims and made a part thereof by reference, now enclosed in a sealed package endorsed by the Assistant Clerk of the Court of Claims, and also those embodied in Volumes III, IV, V, and VII of the "Parts of the Evidence Agreed to by Parties as Material

to Errors Assigned", need not be printed but may be submitted to this Court on the application for writ of certiorari herein in the form in which they are filed in this Court.

Dated, August 28, 1942.

Abel E. Blackmar, Jr., Richard A. Ford, Counsel for Petitioner.

Aug. 31, 1942.

Charles Fahy, Solicitor General of the United States.

[fol. 2165] SUPREME COURT OF THE UNITED STATES, OCTOBER TERM, 1942

No. 369

ORDER EXTENDING TIME WITHIN WHICH TO FILE PETITION FOR CERTIORARI

Upon Consideration of the application of counsel for the Petitioner,

It Is Ordered that the time within which to file petition for certiorari in the above-entitled cause be, and the same is hereby, extended to, and including, Sept. 4th, 1942.

Harlan F. Stone, Chief Justice of the United States.

Dated this 21st day of May, 1942.

[fol. 2166] IN THE SUPREME COURT OF THE UNITED STATES, OCTOBER TERM, 1942

No. 373

ORDER EXTENDING TIME WITHIN WHICH TO APPLY FOR A WRIT OF CERTIORARI

On consideration of the motion of counsel for the petitioner in the above-entitled cause, and good cause therefor having been shown, it is now here ordered that the time within which a petition for writ of certiorari may be filed

herein is hereby extended for a period of sixty days from July 6, 1942, i.e. to September 4, 1942.

Harlan F. Stone, Chief Justice of the Supreme Court of the United States.

June 23rd, 1942.

Endorsed on Cover: File No. 46,870, 46,874. Court of Claims. Term No. 369. Marconi Wireless Telegraph Company of America, Petitioner, vs. The United States. Term No. 373. The United States, Petitioner vs. Marconi Wireless Telegraph Company of America. Petitions for writs of certiorari and exhibit thereto. Filed September 2, 1942, September 3, 1942. Term No. 369 O. T. 1942, 373 O. T. 1942.

SUPREME COURT OF THE UNITED STATES, OCTOBER TERM, 1942

No. 369

ORDER ALLOWING CERTIORARI—Filed December 14, 1942

The petition herein for a writ of certiorari to the Court of Claims is granted. And it is further ordered that the duly certified copy of the transcript of the proceedings below which accompanied the petition shall be treated as though filed in response to such writ.

SUPREME COURT OF THE UNITED STATES, OCTOBER TERM, 1942

No. 373

ORDER ALLOWING CERTIORARI—Filed December 14, 1942

The petition herein for a writ of certiorari to the Court of Claims is granted. And it is further ordered that the duly certified copy of the transcript of the proceedings below which accompanied the petition shall be treated as though filed in response to such writ.